

# An Engine, Not a Camera

How Financial Models Shape Markets



Donald MacKenzie



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## ***An Engine, Not a Camera***

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A list of the series will be found on page 369.

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*How Financial Models Shape Markets*

Donald MacKenzie

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## *Performing Theory?*

Chicago, late evening, October 19, 1987. Leo Melamed leaves a dinner meeting in the Metropolitan Club on the sixty-seventh floor of the Sears Tower. He walks along Wacker Drive to the twin skyscrapers of the Mercantile Exchange, where his office is on the nineteenth floor, high above the exchange's now-silent trading pits. His assistant greets him with a stack of pink message slips from those who have telephoned in his absence. As midnight approaches, "with sweating hands" he makes his first return call, to the Adolphus Hotel in Dallas. It is to Alan Greenspan, who two months earlier had been appointed to chair the Federal Reserve System's Board of Governors.<sup>1</sup>

Leo Melamed's life is a quintessential twentieth-century story. He was born in Bialystok, Poland. In 1939, at the age of seven, he watched, peeking through a crack in the shutters of his parents' home, as German troops entered the city. He witnessed the macabre ceremony in which Bialystok was handed over, under the temporary pact between Hitler and Stalin, to the Soviet Union. He and his family took the last train from Bialystok across the closing border into Lithuania. Almost certainly, they owed their lives to one of the good people of a bad time: Chiune Sugihara, who headed Imperial Japan's consulate in Kovno, Lithuania.

Against his government's instructions, Sugihara was issuing letters of transit to Lithuania's Jewish refugees—hundreds every day. One of Sugihara's visas took Melamed's family to Moscow, to Vladivostok, and to Kobe. The American embassy in Tokyo (Japan and the United States were not yet at war) provided them with a visa, and in 1941 they reached Chicago, where Melamed eventually became a "runner" and then a trader at the Chicago Mercantile Exchange.<sup>2</sup>

The "Merc" had been Chicago's junior exchange. The Board of Trade, with its glorious art deco skyscraper towering over LaSalle Street, dominated futures on grain, the Midwest's primary commodity. The Merc traded futures on humbler products—when Melamed joined it, eggs and onions. As Melamed's

influence grew, he took the exchange in a new direction. Trading futures on currencies, on Treasury bills, on “Eurodollar” interest rates, and on stock indices, it was the first modern financial derivatives exchange. By the mid 1980s, it was central to global finance.

That October night in 1987, however, all Melamed had built—indeed much of the U.S. financial system—was close to ruin. During the day, America’s financial markets had crashed. The Dow Jones industrial average had plummeted 22.6 percent. The Standard and Poor’s (S&P) 500 index had lost about 20 percent. Futures on the S&P 500 were traded on the Mercantile Exchange, and they should have moved in tandem with the index. Instead, they had “disconnected,” falling 29 percent (Jackwerth and Rubinstein 1996, p. 1611).

What Greenspan wanted to know from Melamed was whether the Mercantile Exchange would be able to open the following morning. Melamed was not able to promise that it would. Every evening, after a futures exchange such as the Merc closes, the process of clearing is undertaken. Those whose trading positions have lost money must transfer cash or collateral to the exchange’s clearinghouse for deposit into the accounts of those whose positions have gained. After a normal day on the Merc in the late 1980s, \$120 million would change hands. On the evening of October 19, however, those who had bought S&P futures contracts owed those who had sold such contracts twenty times that amount (Melamed and Tamarkin 1996, p. 359).

Across the United States, unknown numbers of securities-trading firms were close to failure, carrying heavy losses. Their banks, fearing that the firms would go bankrupt, were refusing to extend credit to see them through the crisis. That might leave those firms with no alternative other than “fire sales” of the stocks they owned, which would worsen the price falls that had generated the crisis. It was the classic phenomenon of a run on a bank as analyzed by the sociologist Robert K. Merton<sup>3</sup> (1948)—fears of bankruptcy were threatening to produce bankruptcy—but at stake was not an individual institution but the system itself.

For example, by the end of trading on Monday October 19, the New York Stock Exchange’s “specialists,” the firms that keep stock trading going by matching buy and sell orders and using their own money if there is an imbalance, had in aggregate exhausted two-thirds of their capital. One such firm was rescued only by an emergency takeover by Merrill Lynch, the nation’s leading stockbroker, sealed with a handshake in the middle of that Monday night (Stewart and Hertzberg 1987, p. 1).

If clearing failed, the Mercantile Exchange could not open. That would fuel the spreading panic that threatened to engulf America’s financial institutions in a cascade of bankruptcies. Melamed knew, that Monday night, just how

important it was that clearing be completed. Frantic activity by Melamed and his colleagues throughout the night (including a 3 A.M. call to the home of the president of Morgan Stanley to tell him that his bank owed them \$1 billion) achieved the transfer of \$2.1 billion, but as morning approached \$400 million was still owed to Continental Illinois Bank, which acted as the Merc's agent.

"We hadn't received all the pays," says Barry Lind, who had chaired the Mercantile Exchange's Clearing House Committee and who was called upon in 1987 to advise the Merc's board. "We were missing one huge pay." Some members of the board, which was meeting in emergency session, felt that the Merc should not open. Lind told them to think of the bigger picture, especially the Federal Reserve's efforts to shore up the financial system: "The Fed just spent all these billions of dollars that you are about to demolish. If we don't open, we may never open again. You will have ruined everything they did. Closing the Merc will not help. If you're broke, you're broke."<sup>4</sup>

Around 7 A.M., with 20 minutes to go before the scheduled opening of the Merc's currency futures, Melamed called Wilma Smelcer, the executive of the Continental Illinois Bank who oversaw its dealings with the exchange. This is how he recalls the conversation:

"Wilma . . . You're not going to let a stinking couple of hundred million dollars cause the Merc to go down the tubes, are you?"

"Leo, my hands are tied."

"Please listen, Wilma; you have to take it upon yourself to guarantee the balance because if you don't, I've got to call Alan Greenspan, and we're going to cause the next depression."

There was silence on the other end of the phone. . . . Suddenly, fate intervened. "Hold it a minute, Leo," she shouted into my earpiece, "Tom Theobald just walked in." Theobald was then the chairman of Continental Bank. A couple of minutes later, but what seemed to me like an eternity, Smelcer was back on the phone. "Leo, we're okay. Tom said to go ahead. You've got your money." I looked at the time, it was 7:17 A.M. We had three full minutes to spare. (Melamed and Tamarkin 1996, pp. 362–363)

The crisis was not over. By lunchtime on Tuesday, the New York Stock Exchange was on the brink of closing, as trading in even the most "blue chip" of corporations could not be begun or continued. But the NYSE, the Chicago Mercantile Exchange, and the U.S. financial system survived. Because the wider economic effects of the October 1987 crash were remarkably limited (it did not spark the prolonged depression Melamed and others feared), the threat it posed to the financial system has largely been forgotten by those who did not experience it firsthand.

The resolution of the crisis shows something of the little-understood network of personal interconnections that often underpins even the most



global and apparently impersonal of markets. The Merc's salvation was, as we have seen, a verbal agreement among three people who knew and trusted each other. What eased Tuesday's panic was likewise often quite personal. Senior officials from the Federal Reserve telephoned top bankers and stockbrokers, pressuring them to keep extending credit and not to hold back from settling transactions with firms that might be about to fail. Those to whom they spoke generally did what was asked of them. Bankers telephoned their corporate clients to persuade them to announce programs to buy back stock (First Boston, for example, called some 200 clients on Tuesday morning), and enough of their clients responded to help halt the plunge in stock prices.<sup>5</sup>

The crisis of October 1987 is also the pivot of the twin stories told in this book. One story is of the changes in the financial markets in the United States since 1970, in particular the emergence of organized exchanges that trade not stocks but "derivatives" of stocks and of other financial assets. (The S&P 500 futures traded on Melamed's Mercantile Exchange, for example, are contracts that "derive" their value from the level of the index and thus permit what might be called "virtual ownership" of large blocks of stock.<sup>6</sup>)

In 1970, the market in financial derivatives in the United States and elsewhere was very small by today's standards (there are no reliable figures for its total size), and to trade many of today's derivatives, such as the Merc's S&P 500 futures, would have been illegal. By 1987, derivatives played a central role in the U.S. financial system, which is why the fate of the Mercantile Exchange was so critical to that system. Derivatives markets were also beginning to emerge around the world.

By June 2004, derivatives contracts totaling \$273 trillion (roughly \$43,000 for every human being on earth) were outstanding worldwide.<sup>7</sup> The overall sum of such contracts exaggerates the economic significance of derivatives (for example, it is common for a derivatives contract to be entered into to "cancel out" an earlier contract, but both will appear in the overall figure), and the total must be deflated by a factor of about 100 to reach a realistic estimate of the aggregate market value of derivatives. Even after this correction, derivatives remain a major economic activity. The Bank for International Settlements estimated the total gross credit exposure<sup>8</sup> in respect to derivatives of the sixty or so largest participants in the over-the-counter (direct, institution-to-institution) market at the end of June 2004 as \$1.48 trillion, roughly equivalent to the annual output of the French economy. If the dense web of interconnected derivatives contracts represented by that exposure figure were to unravel, as began to happen in the 1998 crisis surrounding the hedge fund Long-Term Capital Management, the global financial system could experience extensive paralysis.

This book's other story is the emergence of modern economic theories of financial markets. Finance was a mainstream subject of study in the business schools of U.S. universities, but until the 1960s it was treated largely descriptively. There was little or nothing in the way of sophisticated mathematical theory of financial markets. However, a distinctive academic specialty of "financial economics," which had begun to emerge in the 1950s, gathered pace in the 1960s and the 1970s. At its core were elegant mathematical models of markets.

To traditional finance scholars, the new finance theory could seem far too abstract. Nor was it universally welcomed in economics. Many economists did not see financial economics as central to their discipline, viewing it as specialized and relatively unimportant in almost the same way as the economics of ketchup, studied in isolation, would be trivial. ("Ketchup economics" was how the economist Lawrence Summers once memorably depicted how work on finance could appear to the discipline's mainstream.<sup>9</sup>)

The academic base of financial economics was not in economics departments; it remained primarily in business schools. This often brought higher salaries,<sup>10</sup> but it also meant an institutional separation from the wider discipline and a culture that differed from it in some respects. Nevertheless, by the 1990s finance had moved from the margins of economics to become one of the discipline's central topics. Five of the finance theorists discussed in this book—Harry Markowitz, Merton Miller, William Sharpe, Robert C. Merton, and Myron Scholes—became Nobel laureates as a result of their work in finance theory, and other economists who won Nobel Prizes for their wider research also contributed to finance theory.

The central questions addressed by this book concern the relationship between its two stories: that of changing financial markets and that of the emergence of modern finance theory. The markets provided financial economists with their subject matter, with data against which to test their models, and with some of at least the more elementary concepts they employed. Part of the explanation of why financial economics grew in its perceived importance is the gradual recovery of the stock market's prestige—badly damaged by the Great Crash of 1929 and the malpractices it brought to light—and its growing centrality, along with other financial markets, to the U.S. and world economies. But how significant was the other direction of influence? What were the effects on financial markets of the emergence of an authoritative theory of those markets?

Consider, for example, one of the most important categories of financial derivative: options. (A "call option" is a contract that gives its holder the right

but does not oblige the holder to buy a particular asset at a set price on or up to a given future date. A “put option” conveys the right to sell the asset at a set price.) The study of the prices of options is a central topic of financial economics, and the canonical work (Black and Scholes 1973; Merton 1973a) won Scholes and Merton their 1997 Nobel Prizes. (Their colleague Fischer Black had died in 1995, and the prize is never awarded posthumously.)

In 1973, the year of the publication of the landmark papers on option theory, the world’s first modern options market opened: the Chicago Board Options Exchange, an offshoot of Melamed’s rivals at the Board of Trade. How did the existence of a well-regarded theoretical model of options affect the fortunes of the Options Exchange and the pattern of prices in it? More generally, what consequences did the emergence of option theory have for financial markets?

### ***Models and Their “Assumptions”***

The question of option theory’s practical consequences will be answered, at least tentatively, in the chapters that follow. However, before I turn to the effect of finance theory on markets I must say more about the nature of the models the theorists developed. “Models” are now a major topic of the history, philosophy, and sociology of science, but the term covers a wide range of phenomena, from physical analogies to complex sets of equations, running on supercomputers, that simulate the earth’s climate.<sup>11</sup>

The models discussed in this book are verbal and mathematical representations of markets or of economic processes. These representations are deliberately simplified so that economic reasoning about those markets or processes can take a precise, mathematical form. (Appendix E contains a very simple example of such a model, although to keep that appendix accessible I have expressed the model numerically rather than algebraically.)

The models described in the chapters that follow are the outcomes of analytical thinking, of the manipulation of equations, and sometimes of geometric reasoning. They are underpinned by sophisticated economic thinking, and sometimes by advanced mathematics, but computationally they are not overwhelmingly complex. The Black-Scholes-Merton model of option pricing, for example, yields as its central result a differential equation (the “Black-Scholes equation”—equation 1 in appendix D) that has no immediately obvious solution but is nevertheless a version of the “heat” or “diffusion” equation, which is well known to physicists. After some tinkering, Black and Scholes found that in the case of options of the most basic kind the solution of their equation is a relatively simple mathematical expression (equation 2 in appendix D). The

numerical values of the solution can be calculated by hand using standard mathematical tables.

The theoretical work discussed in this book was conducted primarily with pen or pencil and paper, with the computer in the background. The computer's presence is nevertheless important, as would be expected by readers of Philip Mirowski's (2002) account of the encounter between modern economics and the "cyborg sciences." Two major contributors to finance theory, Harry Markowitz and Jack Treynor, worked in operations research (a field whose interweaving with computing and whose influence on economics have been investigated by Mirowski), and the exigencies of computerization were important to William Sharpe's development of Markowitz's model.

Computers were needed to apply finance theory's models to trading. They also were needed to test the models against market data. As will be discussed in chapter 4, the results of those tests were by no means always positive, but as in analogous cases in the natural sciences (Harvey 1981) the very fact of finance theory's testability added to its credibility. It also helped the field to grow by creating roles in financial economics for those whose skills were primarily empirical rather than theoretical. Without computers, testing would have been very laborious if not impossible.

The "mathematicization" of the academic study of finance that began in the 1950s paralleled changes in the wider discipline of economics. Economics had developed in the eighteenth and nineteenth centuries predominantly as what the historian of economics Mary Morgan calls a "verbal tradition." Even as late as 1900, "there was relatively little mathematics, statistics, or modeling contained in any economic work" (Morgan 2003, p. 277). Although the use of mathematics and statistics increased in the first half of the twentieth century, economics remained pluralistic.<sup>12</sup>

However, from World War II on, "neoclassical" economics, which had been one approach among several in the interwar period, became increasingly dominant, especially in the United States and the United Kingdom. The "full-fledged neoclassical economics of the third quarter of the [twentieth] century" gave pride of place to "formal treatments of rational, or optimizing, economic agents joined together in an abstractly conceived free-market, general equilibrium<sup>13</sup> world" (Morgan 2003, p. 279). This approach's mathematical peak was for many years the sophisticated set-theoretical and topological reasoning that in the early 1950s allowed the economists Kenneth Arrow and Gerard Debreu to conclude that a competitive economy, with its myriad firms, consumers, and sectors, could find equilibrium.<sup>14</sup> In 1951, just over 2 percent of the pages of the flagship journal, the *American Economic Review*, contained an equation. In 1978, the percentage was 44 (Grubel and Boland 1986, p. 425).

The mathematicization of economics was accompanied, especially in the United States, by a phenomenon that is harder to measure but real nonetheless: the recovery of confidence, in the economics profession and in the surrounding culture, in markets. The Great Depression of the interwar years had shaken faith in the capacity of markets to avoid mass unemployment. In response, economists following in the footsteps of John Maynard Keynes emphasized the possibility of far-from-optimal market outcomes and the consequent need for government action to manage overall levels of demand. Their analyses were influential both within the economics profession and among policy makers in many countries.<sup>15</sup>

As Melamed's telephone call to Smelcer shows, even in 1987 the fear of a repetition of the interwar catastrophe was still alive. Gradually, however, disenchantment with Keynesian economics and with government intervention grew. The experience of the 1970s—when the tools of such intervention often seemed powerless in the face of escalating inflation combined with faltering growth—was a factor in the growing influence of free-market economists such as Milton Friedman of the University of Chicago, with his “monetarist” theory that the cause of inflation lay in over-expansion of the money supply.

Within economics, the rational-expectations approach became increasingly prominent. In this approach, economic actors are modeled as having expectations consistent with the economic processes posited by the model being developed: the actor “knows as much” as the economist does. From such a viewpoint, much government intervention will be undercut by actors anticipating its likely effects.<sup>16</sup>

No simple mechanical link can be drawn between the way economics as a whole was changing and the way financial markets were theorized. The unity of orthodox, neoclassical economics in the postwar United States is easy to overstate, as Mirowski and Hands (1998) have pointed out, and, as was noted above, even in the 1960s and the 1970s the financial markets did not seem to many economists to be a central topic for their discipline. The mainstream economists who did take finance seriously—notably Franco Modigliani, Paul Samuelson, and James Tobin—often had Keynesian sympathies, while Milton Friedman was among the economists who doubted that some of finance theory counted as proper economics (see chapter 2). Nevertheless, the mathematicization of scholarship on finance paralleled developments in the wider discipline of economics, and finance theorists largely shared their colleagues' renewed faith in free markets and in the reasoning capacities of economic agents. There is, for example, an affinity between rational-expectations economics and the “efficient-market” theory to be discussed in chapter 2.<sup>17</sup>

Like their “orthodox” colleagues in the wider profession, financial economists saw systematic knowledge about markets as flowing from precisely formulated models. As was noted above, finance theory’s models are often computationally quite simple. The solutions they yield are often single equations, not large and elaborate sets of equations to be fitted painstakingly to huge amounts of data. To social scientists in disciplines other than economics, to many practitioners in and commentators on financial markets, and perhaps to some of the financial economists’ colleagues in the wider discipline, this immediately raises the suspicion that finance theory is *too* simple in its models of markets.

The suspicion of over-simplicity can often be heightened when one examines the “assumptions” of finance theory’s models—in other words, the market conditions they posit for the purposes of economic analysis. Typically, those assumptions involve matters such as the following: that stocks and other financial assets can be bought and sold at prevailing market prices without affecting those prices, that no commissions or other “transaction costs” are incurred in so doing, that stocks can be “sold short” (e.g., borrowed and sold, and later repurchased and returned) freely and without penalty, and that money can be borrowed and can be lent at the same “riskless” rate of interest. (The model in appendix E is an example of those assumptions.) Surely such assumptions are hopeless idealizations, markedly at odds with the empirical realities of markets?

For half a century, economists have had a canonical reply to the contention that their models are based on unrealistic assumptions: Milton Friedman’s 1953 essay “The Methodology of Positive Economics,” which was to become “the central document of modernism in economics” (McCloskey 1985, p. 9). Friedman was already prominent within the discipline by the 1950s, and in later decades his advocacy of free markets and of monetarism was to make him probably the living economist best known to the general public.

In his 1953 essay, Friedman distinguished “positive” economics (the study of “what is”) from “normative” economics (the study of “what ought to be”). The goal of positive economics, he wrote, “is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields. In short, positive economics is, or can be, an ‘objective’ science, in precisely the same sense as any of the physical sciences.” (1953a, p. 4)

To assess theories by whether their assumptions were empirically accurate was, Friedman argued, fundamentally mistaken: “Truly important and significant hypotheses will be found to have ‘assumptions’ that are wildly inaccurate descriptive representations of reality. . . . A hypothesis is important if it

‘explains’ much by little . . . if it abstracts the common and crucial elements from the mass of complex and detailed circumstances . . . and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions.” (p. 14) The test of a theory was not whether its assumptions were “descriptively ‘realistic,’ for they never are, but . . . whether the theory works, which means whether it yields sufficiently accurate predictions” (p. 15).

To a reader versed in the philosophy of science, aspects of Friedman’s position—especially his insistence that “factual evidence can never ‘prove’ a hypothesis; it can only fail to disprove it” (p. 9)—are immediately reminiscent of the writings of Karl Popper. Economic methodologists question, however, just how close Friedman’s views are to Popper’s, and indeed have found the former hard to classify philosophically.<sup>18</sup>

Popper and Friedman were founding members of the Mont Pèlerin Society, a meeting place of opponents of postwar statist collectivism set up in April 1947 by the free-market economist Friedrich von Hayek. (The society was named after the site of the society’s ten-day inaugural meeting, a gathering that Friedman later said “marked the beginning of my involvement in the political process.”<sup>19</sup>) Friedman himself certainly sees a similarity between his and Popper’s stances. “My position is, essentially, the same as Popper’s,” he says, “though it was developed independently. . . . I met Popper in 1947, at the first meeting of the Mont Pèlerin Society, but I had already developed all of these ideas before then.” (Friedman interview<sup>20</sup>)

Ultimately, though, Friedman’s “Methodology of Positive Economics” was oriented not to the philosophy of science but to economics,<sup>21</sup> and his stance provoked sharp debate within the profession. The best-known opponent of Friedman’s position was Paul Samuelson, an economist at the Massachusetts Institute of Technology. With *Foundations of Economic Analysis* (1947) and other works, Samuelson played a big part in the mathematicization of economics in the postwar United States. He wrote the discipline’s definitive postwar textbook (*Economics*, which sold some 4 million copies<sup>22</sup>), and in 1970 he was the third recipient of the Prize in Economic Sciences in Memory of Alfred Nobel. Samuelson ended his Nobel Prize lecture by quoting the economist H. J. Davenport: “There is no reason why theoretical economics should be a monopoly of the reactionaries.” (Samuelson 1971, p. 287)

Samuelson seemed to share, at least in part, the suspicion of some of Friedman’s critics that Friedman’s methodological views were also political, a way of defending what Samuelson called “the perfectly competitive laissez faire model of economics.” It was “fundamentally wrong,” wrote Samuelson, to think “that unrealism in the sense of factual inaccuracy even to a tolerable

degree of approximation is anything but a demerit for a theory or hypothesis. . . . Some inaccuracies are worse than others, but that is only to say that some sins against empirical science are worse than others, not that a sin is a merit. . . . The fact that nothing is perfectly accurate should not be an excuse to relax our standards of scrutiny of the empirical validity that the propositions of economics do or do not possess.” (1963, pp. 233, 236)

Just as there is no unitary “scientific method,” faithful following of which guarantees scientific advances,<sup>23</sup> there is not likely to be a productive, rule-like economic methodology. For example, Friedman noted that the “rules for using [a] model . . . cannot possibly be abstract and complete.” How the “entities in [a] model” are to be connected to “observable phenomena . . . can be learned only by experience and exposure in the ‘right’ scientific atmosphere, not by rote” (1953a, p. 25). And on page 9 of the 1953 essay he inserted a crucial parenthetical phrase into the passage putting forward falsificationism, writing that a hypothesis should be “rejected if its predictions are contradicted (‘frequently’ or more often than predictions from an alternative hypothesis)” — a formulation that left room for the exercise of professional judgment.

By the standards of a strict falsificationism, for example, virtually all the models discussed in this book should have been discarded immediately on the grounds that some of their predictions were empirically false. Yet financial economists did not discard them, and they were right not to. For instance, the Capital Asset Pricing Model (discussed in chapter 2) led to the conclusion that all investors’ portfolios of risky assets are identical in their relative composition. That was plainly not so, and it was known not to be so, but the model was still highly prized.

Friedman’s methodological views were, therefore, not a precise prescription for how economics should be done. His view that economic theory was “an ‘engine’ to analyze [the world], not a photographic reproduction of it” (1953a, p. 35) was in a sense a truism: a theory that incorporates all detail, as if photographically, is clearly as much an impossibility as a map that reproduces exactly every aspect and feature of terrain and landscape. Nevertheless, the view that economic theory was an “engine” of inquiry, not an (infeasible) camera faithfully reproducing all empirical facts, was important to the developments discussed in this book.

When, in the 1950s and the 1960s, an older generation of more descriptively oriented scholars of finance encountered the work of the new finance theorists, their reaction was, as has already been noted, often a species of “the perennial criticism of ‘orthodox’ economic theory as ‘unrealistic’” (Friedman 1953a, p. 30) that Friedman’s essay was designed to rebut. Friedman made explicit a vital aspect of what, borrowing a term from Knorr Cetina (1999),



we might call the “epistemic culture” of modern orthodox economics. In so doing, he gave finance theorists a defense against the most common criticism of them, despite his doubts as to whether some parts of finance theory were genuine contributions to economics.<sup>24</sup>

“Around here,” the prominent finance theorist Merton Miller told me, “we just sort of take [Friedman’s viewpoint] for granted. Of course you don’t worry about the assumptions.” (Miller interview) By “here” Miller meant the University of Chicago, but he could as easily have been describing much of finance theory. Attitudes to the verisimilitude of assumptions did differ, with Samuelson and (to a lesser extent) his student Robert C. Merton distancing themselves somewhat from the more Friedmanesque attitudes of some of their colleagues. However, that a model’s assumptions were “unrealistic” did not generally count, in the epistemic culture of financial economics, as a valid argument against the model.

### ***The Infrastructures of Markets***

The “machineries of knowing” (Knorr Cetina 1999, p. 5) that make up finance theory’s engines of inquiry are among this book’s topics. More central to the book, however, is another issue. Financial economics, I argue, did more than analyze markets; it altered them. It was an “engine” in a sense not intended by Friedman: an active force transforming its environment, not a camera passively recording it.<sup>25</sup>

Economists themselves have had interesting things to say about how their subject affects its objects of study,<sup>26</sup> and there is a variety of philosophical, sociological, and anthropological work that bears on the topic.<sup>27</sup> However, the existing writing that best helps place this theme in a wider context is that of the economic sociologist and sociologist of science Michel Callon. Callon rightly refuses to confine economic sociology to the role economists often seem to expect it to take—as an effort to demonstrate irrational “social” elements intruding into market processes—and sees it instead as what might be called an “anthropology of calculation” which inquires into the processes that make calculative economic action and markets possible:

... if calculations are to be performed and completed, the agents and goods involved in these calculations must be disentangled and framed. In short, a clear and precise boundary must be drawn between the relations which the agents will take into account and which will serve in their calculations and those which will be thrown out of the calculation. ... (Callon 1998, p. 16)

Callon contrasts modern market transactions with the “entangled objects” described by ethnographers such as Thomas (1991). An object linked to spe-

cific places and to particular people by unseverable cultural and religious ties cannot be the subject of market transactions in the same way that, for example, today's consumer durables can. The contrast should not be overdrawn (Callon emphasizes the new entanglements without which markets could not function, and also the way in which market transactions "overflow" their frames<sup>28</sup>), but it helpfully focuses attention on the infrastructures of markets: the social, cultural, and technical conditions that make them possible.

Markets' infrastructures matter. Consider, for example, the market in "futures" on agricultural products such as grain, which is relevant to this book because it was from agricultural futures markets—the Chicago Mercantile Exchange and Board of Trade—that the first of today's financial derivatives exchanges emerged. A "future" is a standardized, exchange-traded contract in which one party undertakes to sell, and the other to buy, a set quantity of a given type of asset at a set price at a set future time. Futures markets did not originate in the United States. What seem to have been in effect rice futures were traded in eighteenth-century Osaka (Schaefer 1989), and some European markets also predated those of the United States (Cronon 1991, p. 418). But futures trading developed in Chicago on an unprecedented scale in the second half of the nineteenth century, and it is the subject of a justly celebrated analysis by the historian William Cronon (1991).<sup>29</sup>

A futures market brings together "hedgers" (for example, producers or large consumers of the grain or other commodity being traded), who benefit from being certain of the price at which they will be able to sell or to buy the grain, and "speculators," who are prepared to take on risk in the hope of profiting from price fluctuations. However, successful futures trading requires more than the existence of economic actors who may benefit from it.

For futures trading to be possible, the underlying asset has to be standardized, and that involves a version of Callon's "disentanglement" and "framing." The grain to which a futures contract makes reference may not even have been harvested yet, so a buyer cannot pick out a representative sack, slit it open, and judge the quality of the grain by letting it run through his or her fingers. "Five thousand bushels of Chicago No. 2 white winter wheat" has to be definable, even if it does not yet physically exist.

As Cronon shows, the processes that made Chicago's trading in grain futures possible were based on the disentanglement of grain from its grower that took place when transport in railroad cars and storage in steam-powered grain elevators replaced transport and storage in sacks. Sacks kept grain and grower tied together, the sacks remaining the latter's property, identified as such by a bill of lading in each sack, until they reached the final purchaser. In contrast, grain from different growers was mixed irreversibly in the elevators' giant bins,

and the trace of ownership was now a paper receipt, redeemable for an equivalent quantity of similar grain but not for the original physical substance.

The standardization of grain was both a technical and a social process. In Chicago the bushel, originally a unit of volume, became a unit of weight in order to permit measurement on scales on top of each elevator. A team of inspectors—employed first by the Chicago Board of Trade and then by the state of Illinois—checked that the scales were fair and made the inevitably contestable judgments that the contents of this bin were good enough to be classed as “No. 1 white winter wheat,” which had to “be plump, well cleaned and free from other grains,” while that bin contained only “No. 2,” which was defined as “sound, but not clean enough for No. 1” (Cronon 1991, p. 118).

With grains thus turned into “homogeneous abstractions” (Cronon 1991, p. 132), disentangled at least partially from their heterogeneous physical reality, it was possible to enter into a contract to buy or to sell 5,000 bushels (the standard contract size) of, for example, “Chicago No. 2 white winter wheat” at a set price at a given future time. Such a contract had no link to any *particular* physical entity, and because its terms were standardized it was not connected permanently to those who had initially entered into it.<sup>30</sup>

If, for example, one of the parties to a futures contract wished to be free of the obligation it imposed, he or she did not have to negotiate with the original counterparty for a cancellation of the contract, but could simply enter into an equal-but-opposite futures contract with a third party. Although when the specified delivery month arrived a futures contract could in principle be settled by handing over elevator receipts, which could be exchanged for actual grain, in practice delivery was seldom demanded. Contracts were normally settled by payment of the difference between the price stated in the contract and the current market price of the corresponding grade of grain. A future was thus “an abstract claim on the golden stream flowing through [Chicago’s] elevators” (Cronon 1991, p. 120).

The disentanglement of the abstract claim from grain’s physical reality and the framing of the latter into standardized grades were never entirely complete. The standardization of grain depended on a “social” matter, the probity of the grain inspectors, and in nineteenth-century Chicago that was seldom entirely beyond question. The possibility of settlement by physical delivery and the role played by the current market price of grain in determining cash settlement sums kept the futures market and the “spot” (immediate delivery) market tied together.

However infrequently the physical delivery of grain was demanded, its possibility was essential to the legal feasibility of futures trading in the United States. If physical delivery was impossible, a futures contract could be settled

only in cash, and that would have made it a wager in U.S. law. There was widespread hostility toward gambling, which was illegal in Illinois and in most other states. The consequent need for Chicago's futures exchanges to preserve the possibility of physical delivery—the chief criterion demarcating their activities from gambling—cast a long historical shadow. As chapter 6 will show, even in the 1970s this shaped the development of financial derivatives.

A further, particularly dramatic way in which futures trading was sometimes tied to the underlying physical substance was a “corner,” in which a speculator or group of speculators purchased large amounts of grain futures and also sought to buy up most or all of the available physical grain. If a corner succeeded, those who had engineered it had at their mercy those who had sold futures short (that is, without owning corresponding amounts of grain). The success of a corner could depend on far-from-abstract matters, such as whether ice-free channels could be kept open in Duluth Harbor or in Thunder Bay long enough to allow sufficient grain to be shipped to Chicago to circumvent the corner. One such attempted corner, the failed “Leiter corner” of 1897–98, was the basis for Frank Norris's classic 1903 Chicago novel *The Pit*.<sup>31</sup>

Another aspect of the infrastructure of agricultural futures trading in the United States was a specific architectural feature of the physical space in which trading took place: the “pit” that gave Norris's novel its title. Overcrowding on the floor of the Board of Trade—which had 2,187 members by 1869 (Falloon 1998, p. 72)—led to the introduction of stepped “amphitheaters,” traditionally octagonal in shape.

Despite the name, pits are generally raised above the floor of an exchange, not sunk into it. Standing on the steps of a pit, rather than crowded at one level, futures traders can more easily see each other, which is critical to facilitating Chicago's “open outcry” trading, in which deals are struck by voice or (when it gets too noisy, as it often does) by an elaborate system of hand signals and by eye contact. Where one stands in a pit is important both socially and economically: one's physical position can, quite literally, be worth fighting for, even though throwing a punch can bring a \$25,000 fine from an exchange.<sup>32</sup>

### ***The Performativity of Economics***

The infrastructures of markets are thus diverse. As we have just seen, the infrastructure of grain futures trading included steam-powered elevators, grain inspectors who were hard to bribe, crowded pits, and contracts that reflected the need to keep futures trading separate from gambling. One important aspect of Callon's work is his insistence that economics itself is a part of the infrastructure of modern markets: “. . . economics, in the broad sense of the term,

performs, shapes and formats the economy, rather than observing how it functions” (1998, p. 2).

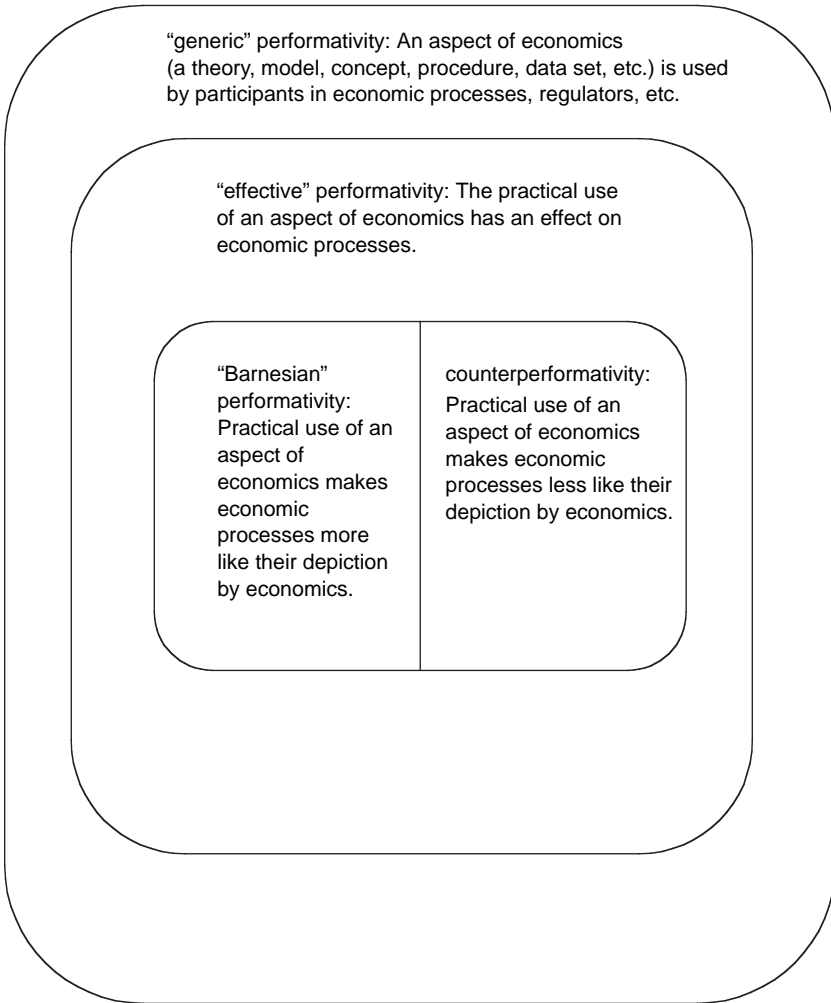
By “economics, in the broad sense of the term” Callon means “all the activities, whether academic or not . . . aimed at understanding, analyzing and equipping markets” (2005, p. 9)—a definition that obviously goes well beyond the academic discipline. However, it is at least sometimes the case that economics in the narrower, academic sense “performs, shapes and formats the economy.” Consider, for example, the “Chicago Boys,” economists from the Universidad Católica de Chile trained by Milton Friedman and his University of Chicago colleagues between 1955 and 1964 as part of a Cold War U.S. program “to combat a perceived leftist bias in Chilean economics” (Valdés 1995; Fourcade-Gourinchas and Babb 2002, p. 547). Especially under the government of General Pinochet, the “Chicago Boys” did not simply analyze the Chilean economy; they sought to reconstruct it along the free-market, monetarist lines whose advantages they had been taught to appreciate.

The Chicago Boys are a well-known and politically controversial example, unusual in that it involves particularly direct access by economists to the levers of political power, but this example is a vivid manifestation of a general phenomenon. The academic discipline of economics does not always stand outside the economy, analyzing it as an external thing; sometimes it is an intrinsic part of economic processes. Let us call the claim that economics plays the latter role *the performativity of economics*.

The coiner of the term “performative” was the philosopher J. L. Austin. He admitted that it was “rather an ugly word,” but it was one that he thought necessary to distinguish utterances that *do* something (performative utterances) from those that report on an already-existing state of affairs. If I say “I apologize,” or “I name this ship the *Queen Elizabeth*,” or “I bet you sixpence it will rain tomorrow,” then “in saying what I do, I actually perform the action” (Austin 1970, p. 235).<sup>33</sup>

Many everyday utterances in financial markets are performative in Austin’s sense. If someone offers to buy from me, or to sell to me, a particular asset for a particular price, and I say “done” or “agreed,” then the deal *is* agreed—at least if I am in a market, such as the Chicago futures exchanges, in which a verbal agreement is treated as binding. But what might it mean for economics, or a particular subset of it such as financial economics, to be performative? Plainly, that is a far more complex matter than the analysis of specific, individual utterances.

At least three levels of the performativity of economics seem to me to be possible (figure 1.1).<sup>34</sup> The first, weakest level is what might be called “generic performativity.” For an aspect of economics to be performative in this sense



**Figure 1.1**

The performativity of economics: a possible classification. The depicted sizes of the subsets are arbitrary; I have not attempted to estimate the prevalence of the different forms of performativity.

means that it is used, not just by academic economists, but in the “real world”: by market participants, policy makers, regulators, and so on. Instead of being external to economic processes, the aspect of economics in question is “performed” in the generic sense of being used in those processes. Whether this is so is, in principle, a straightforward empirical matter: one simply observes whether economics is drawn on in the processes in question. (In practice, of course, the available sources—historical or current—may not be sufficient to allow one to be certain how matters stand in this respect, and one must remember to look not just at what participants say and write but also at whether the processes in question involve procedures and material devices that incorporate economics.)

What is less straightforward conceptually, and more complicated empirically, is to determine what effect, if any, the use of economics has on the economic process in question. The presence of such an effect is what is required for a stronger meaning of “performativity”: the subset of generic performativity that one might call “effective performativity.” For the use of a theory, a model, a concept, a procedure, a data set, or some other aspect of economics to count as effective performativity, the use must *make a difference*. Perhaps it makes possible an economic process that would otherwise be impossible, or perhaps a process involving use of the aspect of economics in question differs in some significant way (has different features, different outcomes, and so on) from what would take place if economics was not used.

Except in the simplest cases, one cannot expect observation alone to reveal the effect of the use of an aspect of economics. One cannot assume, just because one can observe economics being used in an economic process, that the process is thereby altered significantly. It might be that the use of economics is epiphenomenal—an empty gloss on a process that would have had essentially the same outcomes without it, as Mirowski and Nik-Khah (2004) in effect suggest was the case for the celebrated use of “game theory” from economics in the auctions of the communications spectrum in the United States.

Ideally, one would like to be able directly to compare processes with and without use of the aspect of economics in question. Such comparisons, however, are seldom entirely straightforward: the relevant situations will typically differ not just in the extent of the usage of economics but in other respects too. There will thus often be an element of conjecture and an element of judgment in attributing differences in outcome to the use of economics rather than to some other factor.

Most intriguing of all the varieties of the performativity of economics depicted in figure 1.1 are the two innermost subsets. There the use of economics is not simply having effects on economic processes: those processes are

being altered in ways that bear on their conformity to the aspect of economics in question. In the case of the use of an economic model, for example, one possibility is that economic processes or their outcomes are altered so that they better correspond to the model. Let me call this possibility “Barnesian performativity,” because the sociologist Barry Barnes has emphasized (especially in a 1983 article and a 1988 book) the central role in social life of self-validating feedback loops. (In earlier work, I called this type of performativity “Austinian.” That had the disadvantage of being read as invoking not sociology, which is what I wanted to invoke, but linguistic philosophy.)<sup>35</sup>

As Barnes notes, if an absolute monarch designates Robin Hood an “outlaw,” then Robin *is* an outlaw. Someone is a “leader” if “followers” regard him or her as such. A metal disk, a piece of paper, or an electronic record is “money” if, collectively, we treat it as a medium of exchange and a store of value.<sup>36</sup>

The strong, Barnesian sense of “performativity,” in which the use of a model (or some other aspect of economics) makes it “more true,” raises the possibility of its converse: that the effect of the practical use of a theory or model may be to alter economic processes so that they conform less well to the theory or model. Let me call this possibility—which is not explicit in Callon’s work—“counterperformativity.”<sup>37</sup> An aspect of economics is being used in “real-world” processes, and the use is having effects, but among those effects is that economic processes are being altered in such a way that the empirical accuracy of the aspect of economics in question is undermined.

“Barnesian performativity” could be read as simply another term for Robert K. Merton’s famous notion of the “self-fulfilling prophecy” (1948), and “counterperformativity” as another word for its less-well-known converse, the self-negating prophecy. I have three reasons for preferring the terminology I use here.

First, I want the terminology to reflect the way in which the strongest senses of “performativity” are subsets of a more general phenomenon: the incorporation of economics into the infrastructures of markets.

Second, the notion of “prophecy,” whether self-fulfilling or self-negating, can suggest that we are dealing only with beliefs and world views. While beliefs about markets are clearly important, an aspect of economics that is incorporated only into beliefs “in the heads” of economic actors may have a precarious status. A form of incorporation that is in some senses deeper is incorporation into algorithms, procedures, routines, and material devices.<sup>38</sup> An economic model that is incorporated into these can have effects even if those who use them are skeptical of the model’s virtues, unaware of its details, or even ignorant of its very existence.



Third, in Robert K. Merton's original article on "the self-fulfilling prophecy," as in much subsequent discussion, the notion carries the connotation of pathology: an incorrect belief, or at least an arbitrary one, is made true by the effects of its dissemination. It is emphatically not my intention to imply that in respect to finance theory. For example, to say of Black-Scholes-Merton option-pricing theory that it was "performative" in the Barnesian sense is not to make the crude claim that any arbitrary formula for option prices, if proposed by sufficiently authoritative people, could have "made itself true" by being adopted. Most such formulas could not do so, at least other than temporarily.

Even if a formula for option pricing had initially been adopted widely, it would soon have ceased to hold sway if it led those using it systematically to lose money, or if it gave rise to unconstrained opportunities for others to conduct arbitrage. (Arbitrage is trading that exploits price discrepancies to make riskless or low-risk profits.) Imagine, for example, that as a result of a mistake in their algebra Black and Scholes had produced a formula for the value of a call option that was half or double their actual formula (expression 2 in appendix D), that no one noticed, and that the formula was then used widely to price options. It would not have been a stable outcome: the sellers or buyers of options would have incurred systematic losses, and attractive arbitrage opportunities would have been created.

There was, furthermore, much more to the Black-Scholes-Merton model than an equation that could be solved to yield theoretical option prices. The model was an exemplar (in the sense of Kuhn 1970) of a general methodology for pricing a derivative: try to find a continuously adjusted portfolio of more basic assets that has the same payoffs as the derivative. (Such a portfolio is called a "replicating portfolio.") If one can do that, then one can argue that the price of the derivative must equal the cost of the replicating portfolio, for otherwise there is an arbitrage opportunity. Today it would be unusual to find the Black-Scholes-Merton model being used directly as a guide to trading options: in options exchanges, banks' trading rooms, and hedge funds, the model has been adapted and altered in many ways. However, the model's "replicating portfolio" methodology remains fundamental.

The methodology offers not just "theoretical" prices but also a clear and systematic account of the economic process determining those prices. This account altered how economists conceived of a broad range of issues: the pricing not just of derivatives but also of more "basic" securities, such as bonds, and even the analysis of decisions outside of the sphere of finance that can be seen as involving implicit options. It affected how market participants and regulators thought about options, and it still does so, even if the phase of the Barnesian performativity of the original Black-Scholes-Merton model has passed.

### ***Detecting Barnesian Performativity***

One way of detecting the Barnesian performativity of an aspect of economics such as a theory or a model is by comparing market conditions and patterns of prices before and after its widespread adoption. (By “market conditions” I mean matters such as the typical level of transaction costs or the feasibility and expense of short sales.) If those conditions or prices have changed toward greater conformity to the theory or model, that is evidence consistent with Barnesian performativity. It does not *prove* performativity, because the change could have taken place for reasons other than the effects of the use of the theory or model. Unfortunately, certainty in this respect tends to be elusive, but that is no reason to abandon the inquiry. All it means is that, as with “effective” performativity, we are dealing with a question of historical or social-science causation on which evidence can throw light but which it would be naive to expect to be resolved unambiguously.

Inquiring into Barnesian performativity thus involves more than an examination of the extent, the manner, and the general effects of the use of economics in economic practice. In investigating market conditions and prices and in judging whether they have moved toward (or away from) conformity to an aspect of economics, one is not just examining economics and those who develop and use it; inevitably one is also studying the “objects” that economics analyzes. That is something that the field to which much of my work has belonged—the sociology of scientific knowledge—has sometimes been reluctant to do.<sup>39</sup>

Certainly one should not underestimate the complexity of judging whether patterns of market prices, for example, have moved toward greater conformity with a model such as Black-Scholes-Merton. One way of formulating the question is to examine the extent to which the model’s predictions are borne out. However, what a model predicts is often not straightforward. The Black-Scholes-Merton model, for example, yields an option price only after the characteristics of the option and the values of the parameters of the Black-Scholes equation have been set. One parameter, the volatility of the stock, is acknowledged not to be directly observable, so there is no unique theoretical price to compare with “actual” prices.

Furthermore, “actual” or “real-world” market prices are complex entities. As Koray Caliskan (2003, 2004) points out in a delightful ethnographic discussion of cotton trading, markets abound with prices and price quotations of many kinds. What gets reported as cotton’s “world price,” for instance, is a complicated construction involving not only averaging but also subjective adjustments.

Difficulties remain even if one restricts oneself to the prices at which transactions are actually concluded. For example, the most thorough early empirical tests of option-pricing models were conducted by the financial economist Mark Rubinstein (see chapter 6). He obtained the Chicago Board Options Exchange's own electronic records of transactions, so he did not have to rely on price quotations or on closing prices, but he still faced problems. For instance, it was common for options to trade at different prices when the price of the underlying stock did not alter at all. In a typical case, "while the stock price [of the Polaroid Corporation] apparently remained constant at  $37\frac{1}{2}$  [\$37.50], one July/40 call [option] contract was traded at  $3\frac{1}{4}$  [\$3.25], eight at  $3\frac{3}{8}$  [\$3.375], and, one at  $3\frac{1}{2}$  [\$3.50]. Will the true equilibrium option price please stand up?" (Rubinstein 1985, p. 465<sup>40</sup>)

Thus, Rubinstein had to analyze not "raw" prices of options, but weighted averages. He also filtered out large numbers of price records that he regarded as problematic. For example, he excluded any record that referred to either the first or the last 1,000 seconds of the options exchange's trading day. He removed transactions close to the start of the day because they often reflected the "execution of limit orders<sup>41</sup> held over from the previous day." He eliminated those in the final minutes before the close of trading because prices then were influenced by "trades to influence market maker margin" (1985, p. 463)—in other words, the level of deposit that had to be maintained in order to be allowed to continue holding a position.

Transactions close to the start or the end of the day involved what Rubinstein called "artificial pricing" (p. 463). Filtering them out from the analysis was a perfectly sensible procedure (Rubinstein had been a trader on an options exchange and so had an insider's understanding of trading-floor behavior), but embedded in the exclusion of what were often the periods of most frantic trading activity was a view of the "natural" operations of markets.

The potentially problematic nature of "real-world" prices is only an example of the complexities of econometric testing: many of the points that historians and sociologists of science have made about scientific experiment can also be made about the testing of finance theory's models. As Callon's colleague Bruno Latour (among many others) has pointed out, detailed attention to the active, transformative processes by which scientific knowledge is constructed breaks down the canonical view in which there is a "world" entirely distinct from "language" and thus undermines standard notions of reference in which "words" have discrete, observable "things" to which they refer.<sup>42</sup>

Replication and the reproducibility of results are at least as problematic in econometrics as the sociologist Harry Collins has shown them to be in the natural sciences.<sup>43</sup> A later test will often contradict an earlier one—see the

extensive lists of examples in Goldfarb's 1995 and 1997 papers. In that situation, there may be no a priori way of knowing whether the original test was at fault, whether the new one is incompetent, or whether the discrepancy is to be explained by historical and geographical variation or other differences in the economic processes being studied.

It is also the case that, as was noted above in respect to volatility, what a finance-theory model implies for a specific situation depends not on the model alone but also on auxiliary assumptions about that situation. What is being tested, therefore, is not the model in isolation but the model plus auxiliary assumptions, just as is always the situation in scientific experiment. (This is the "Duhem-Quine" thesis of the philosophy and sociology of science. See, for example, Barnes 1982, pp. 73–76.) An empirical result that apparently falsifies a model can therefore be blamed on a fault in one of the auxiliary assumptions.

For example, many efforts were made empirically to test two developments in finance theory: the Capital Asset Pricing Model and the efficient-market hypothesis. Normally it was not possible to disentangle these entirely so that only one was being tested at a time; typically the tests were of both the model and the hypothesis simultaneously. Tests of market efficiency usually involved examining whether investment strategies were available that systematically generated "excess" risk-adjusted returns. A criterion for what constitutes an "excess" return was thus needed, and in the early years of such testing the Capital Asset Pricing Model was usually invoked as the criterion.<sup>44</sup> When "anomalies" were found in the results of the tests, how to interpret them was therefore debatable: were they cases of market inefficiency, or evidence against the Capital Asset Pricing Model?

Conversely, central to the Capital Asset Pricing Model was what the model posited about the returns expected by investors on assets with different sensitivities to market fluctuations, but typically no attempt was made to measure these expected returns directly—for example, by surveying investors. (The results of any such survey would have been regarded as unreliable by most financial economists.) Instead, in empirical tests of the Capital Asset Pricing Model, more easily measurable after-the-fact realized returns were used as a proxy for expected returns—a substitution that rested on an efficient-market, rational-expectations view of the latter.<sup>45</sup>

Even something as basic as the "cleaning" of price data to remove errors in data entry can, in a sense, involve theory. The main original data source against which finance theory's models were tested was the tapes of monthly stock returns produced by the Center for Research in Security Prices at the University of Chicago. An already-known (and in one sense a theoretical) feature

of stock-price changes was used as the basis for the computerized algorithm for detecting data-entry errors:

Rather than coding and punching all prices twice and then resolving discrepancies manually, we found a better procedure. We know that the change in the price of a stock during one month is very nearly independent of its change during the next month. Therefore, if a price changes a large amount from one date to a second date, and by a similar amount in the opposite direction from the second date to a third, there is a reason to believe that at the second date the price was misrecorded. A “large change” was rather arbitrarily taken to mean a change in magnitude of more than 10 per cent of the previous price plus a dollar. (Lorie 1965, p. 7)

Because of the complexities of econometric testing, the extent of the “fit” between a theoretical model and patterns of prices cannot be determined by simple inspection. “There just isn’t any easy way to test a theory,” said Fischer Black (1982, p. 32). Knowledge of whether patterns of prices have moved toward greater conformity with a theory is the outcome of a difficult, and often a contested, process. It is therefore tempting to set the issue aside, and to abandon the strongest meanings of performativity (Barnesian performativity and counterperformativity). However, to do that would involve also abandoning a central question: Has finance theory helped to create the world it posited—for example, a world that has been altered to conform better to the theory’s initially unrealistic assumptions?

Has the practical use of finance theory (for example, as a guide to trading, or in the design of the regulatory and other frameworks within which trading takes place) altered market processes toward greater conformity to theory? If the answer to that question is at least partially in the affirmative, we have identified a process shaping the financial markets—and via those markets perhaps even the wider economies and societies of high modernity—that has not received anything like sufficient attention. If, on the other hand, the practical use of finance theory sometimes undermines the market conditions, processes, and patterns of prices that are posited by the theory, we may have found a source of danger that it is easy to ignore or to underestimate if “reality” is conceived of as existing entirely independently of its theoretical depiction.

As the economist and economic policy maker Alan Blinder has pointed out, in many respects global economies have in recent decades moved closer to the standard way in which economists model them, with, for example, its assumption of “single-minded concentration on profit maximization.” Blinder suspects that “economists . . . have bent reality (at least somewhat) to fit their models” (2000, pp. 16, 18). The anthropologist Daniel Miller likewise asserts that “economics has the authority to transform the world into its own image” (1998, p. 196).

Whether Blinder and Miller are right is a question this book seeks to answer, at least for one area of economics. The question requires us to examine the strongest level of performativity, despite the methodological difficulties it poses. The reader is warned, however, that there are complexities in the judgment of the correspondence of patterns of prices to models that are only touched on here. This is a study of finance theory and of its relations to markets, not a study of financial econometrics. I have done little more than distinguish those issues about which econometricians seem to agree (for example, the existence after 1987 of the “volatility skew”) from those on which there is no clear consensus.

### ***The Book's Goals***

If academic pursuits are not to be narrow, they ought to seek to contribute to what Donald (now Deirdre) McCloskey called the conversations of humankind. One such set of conversations, a very old one,<sup>46</sup> is about markets. Those conversations are not always as free-flowing or as civilized as they should be. This is partly because of inequalities of wealth or power and the desire for outcomes economically beneficial to particular sets of participants, but it is also because those who come to those conversations often bring strong, deeply felt preconceptions. Some are convinced that markets are sources of human freedom and prosperity; others believe markets to be damaging generators of alienation, exploitation, and impoverishment. Currently, that divide tends to map onto a disciplinary one, with mainstream economists approving profoundly of markets and with sociologists and anthropologists frequently manifesting deep, albeit often unexplicated, reservations about them.<sup>47</sup>

This book plainly is not economics, although some of it is history of what eventually became one of the most important branches of modern economics. Nor is it economic sociology, at least as traditionally conceived, although it touches on some of that field's concerns. Instead, it is intended in the first instance as a contribution to “social studies of finance.”<sup>48</sup> The term has a variety of possible meanings, but one way of describing the underlying enterprise is as drawing on, and developing, the intellectual resources of the social studies of science and technology in order to embark on a conversation about the *technicality* of financial markets. Economic sociology, for example, has been strong in its emphases on matters such as the embedding of markets in cultures, in politics, and in networks of personal interconnections.<sup>49</sup> It has traditionally been less concerned with the systematic forms of knowledge deployed in markets or with their technological infrastructures,<sup>50</sup> yet, if the social studies

of science and the history and sociology of technology are right, those too are social matters, and consequential ones.

“We have taken science for realist painting,” writes Bruno Latour, “imagining that it made an exact copy of the world. The sciences do something else entirely—paintings too, for that matter. Through successive stages they link us to an aligned, transformed, constructed world.” (1999, pp. 78–79) If finance theory is one of Latour’s sciences—and this book’s conjecture is that it is—then simply to praise it is not to add much to humanity’s conversations about markets, and simply to denounce it is to coarsen those conversations. To try to understand how finance theory has “aligned, transformed [and] constructed” its world—which is also everyone’s world, the world of investment, savings, pensions, growth, development, wealth, and poverty—may, in contrast, contribute a little to conversations about markets.

Humanity’s conversations about markets are not just intellectual; they bear on the question of the appropriate role for markets in our societies. Debates about that role sometimes remind me of debates about technology in the 1960s and the 1970s. Technology was then often taken as either to be adulated or to be condemned, and each of the apparent options frequently involved an implicit view of technological change as following an autonomous logic. The surrounding culture could choose to conform to that logic or to reject its products, but could not modify it fundamentally.

If the history and sociology of technology of the last 25 years have had a single dominant theme, it is that the view of technological change as following an autonomous logic is wrong, and the stark choice between conformity and refusal that it poses is an impoverished one. Technologies can develop in different ways according to circumstances, the design of technical systems can reflect a variety of priorities, and “users” frequently reshape technical systems in important ways. Ultimately, the development and the design of technologies are political matters.<sup>51</sup>

A nuanced and imaginative politics of technology is thus a better option than either uncritical acceptance or downright rejection of technical change. An equivalent approach to markets—one that is more nuanced and more specific than most current ways of thinking about them and of acting in relation to them—is badly needed. I do not claim to provide such an approach (that is a task beyond one book and one author), but my hope for this book is that it helps to begin a conversation with that aim in mind.

## ***Sources***

This book takes the form of a series of historical narratives of the development of finance theory and of its interaction with the modern financial

markets. Although I touch on what I think would widely be agreed to be the theory's most salient achievements, I have not attempted a comprehensive account of its history. I am even more selective in my discussion of markets, focusing on developments that seem to me to be of particular relevance from the viewpoint of the issues, especially those to do with the performativity of economics, sketched in this chapter. Indeed, the core of the book—chapters 5, 6, and 7—is in a sense a single, extended case study of the development of option theory, of its impact on markets, and of the empirical history of option pricing.

Since relevant, accessible archival material for a book such as this is still sparse, the book's main unpublished source is a set of more than 60 oral-history interviews of the finance theorists and market participants listed in appendix H, and of a number of others who do not wish their names to be disclosed. In the case of the theorists, these interviews complement what can be gleaned from the published literature of their field, and the interviews with practitioners were crucial in helping me to disentangle complex matters such as the impact on markets of option theory or the celebrated debacle of Long-Term Capital Management. I was, however, also fortunate enough to be allowed access to finance theory's most important archive: the papers of Fischer Black, held in the Institute Archives at MIT.

Reasonably comprehensive interview coverage of the most influential finance theorists was possible.<sup>52</sup> Plainly, no such comprehensive coverage is possible in the case of the much larger and more heterogeneous body of people who have played important roles in the development of modern financial markets, even in a limited segment of those markets such as financial derivatives exchanges. My interviewing of market participants was therefore much more ad hoc, and was focused on episodes of specific interest like the emergence of modern derivatives trading in Chicago. These interviews were supplemented by the use of sources such as the trade press and by examination of econometric analyses. In particular, I had the good fortune that the analysis of the prices of options in the Chicago markets has become a locus classicus of modern financial econometrics.

Oral-history interviews have well-known disadvantages. In particular, interviewees' memories of events, especially of specific events long in the past, may be fallible, and they may wish a particular version of events to be accepted. In consequence, I have tried to "triangulate" as much I can, checking one interviewee's testimony against that of others and (where possible) against the published record or econometric analyses of the markets they were describing. In the case of Long-Term Capital Management, for example, I checked for any "exculpatory" bias in insiders' views of the fund's 1998 crisis by interviewing others who had been active in the same markets at the same time. The account



of LTCM's crisis presented in chapter 8 was also checked for its consistency with price movements in the relevant markets in 1998 (see MacKenzie 2003b).

I have also had the advantage of having previous historical and sociological work on finance theory and financial markets to build on. Particularly worth singling out is Peter Bernstein's history of finance theory, *Capital Ideas* (1992).<sup>53</sup> Bernstein's emphases differ from mine; for example, he does not address what I call Barnesian performativity, and in regard to the theory's applications he is concerned more with stock-portfolio management than with derivatives markets. However, I owe a great debt to Bernstein, as will future historians of finance theory.

Effectively the only existing sociological analyses of the rise of modern finance theory are those by Richard Whitley (1986a,b). Although I disagree with him in some respects (for example, I think he understates the tension between finance theorists and practitioners), I have been influenced heavily, particularly in chapter 3, by his analysis of the role of changes in the business schools of American universities in creating a favorable environment for the development of the new financial economics.

### ***Overview of the Book***

Although finance theory is a mathematical domain, I have kept the book as non-mathematical as possible, banishing equations to endnotes or appendixes. Finance theory's technical terminology cannot be avoided entirely, but I have used it as sparingly as I can, explaining it in the chapters and in a glossary. (The glossary also contains explanations of relevant financial-market terms.) I hope the resultant account will be accessible to readers with no background in economics or in the financial markets, yet not too tediously simplistic for those with such backgrounds.

Chapter 2 describes the shift in the United States in the 1950s and the 1960s from descriptive scholarship in finance to the new analytical, mathematical, economics-based approach. The first of the three strands in my discussion is the work of Franco Modigliani and Merton Miller, whose "irrelevance" propositions were the most explicit challenge to the older approach. The second strand is Harry Markowitz's work on the selection of optimal investment portfolios and its development by William Sharpe into the Capital Asset Pricing Model, finance theory's canonical account of the way stock prices reflect a tradeoff between expected return and risk (in the sense of sensitivity to overall market fluctuations). The third strand is random-walk models of stock-price changes and the eventual culmination of those models in the efficient-market hypothesis. It is easy to imagine that by diligent study one can find patterns in

stock-price changes that permit profitable prediction, but random-walk models denied the existence of such patterns. The efficient-market hypothesis generalized this denial into the assertion that prices in mature capital markets always, and effectively instantaneously, take into account all available price-relevant information, including not only the record of previous price changes but also economic information about corporations of the kind that stock analysts pore over. Since all available information is already incorporated into prices, Eugene Fama and other efficient-market theorists argued, it is not possible to make systematic, risk-adjusted, excess profits on the basis of it. Stock prices are moved by new information, but by virtue of being new such information is unpredictable and thus “random.”

Chapter 3 broadens the discussion from the specific ideas of finance theory discussed in chapter 2. It discusses how the new finance scholarship developed into the distinct academic subfield of financial economics. (I use the term “financial economics” to include not only finance theory but also efforts to test theories and more general empirical and econometric work on finance.) The chapter also describes the ambivalent and frequently hostile reaction by market practitioners to finance theory. The theory could be drawn on to subject the performance of investment managers to a disconcerting mathematical gaze, and its central tenet—the efficient-market hypothesis—suggested that practitioners’ beliefs about markets were often mistaken, that many of their activities were pointless, and that often their advice was of no real benefit to their clients.

Amidst the general hostility, however, there were pockets of practitioners who saw merits in finance theory. Indeed, some found in it ideas with which they could make money—for example, by calculating and selling values of the Capital Asset Pricing Model’s most important parameter, beta, which indicates the extent to which the returns on a stock or some other financial asset are sensitive to fluctuations in the market as a whole.

The most significant early practical innovation to emerge from financial economics was the index fund. If, as financial economics suggested, managers’ stock selections failed systematically to outperform broad market indices such as the S&P 500, then why not simply invest in the stocks that made up the index in such a way that the performance of one’s portfolio would automatically track the level of the index? Such an index fund, its proponents suggested, would perform as well as the portfolios chosen by traditional managers, and it would not be hampered by the high fees those managers charged.

Index funds, first launched in the early 1970s, have become a major feature of modern stock markets. Rooted in financial economics, they can be seen as one way in which that field has been performed in the markets. There is even

a Barnesian strand to this performance: the popularity of indexing has made a prediction of the Capital Asset Pricing Model that troubled Sharpe (the prediction that all investors would hold the same portfolio of risky assets, the market itself) less untrue.

Chapter 4 discusses the empirical testing of the strands of finance theory described in chapter 2 and begins to broaden the discussion of performativity. Given the difficulties of econometric testing discussed above, it is not surprising that it proving or disproving the empirical validity of finance theory's models turned out to be difficult. It was hard to construct empirically testable versions of the Modigliani-Miller propositions, and even the apparently directly testable Capital Asset Pricing Model was argued not to be testable at all. Central to the model was the "market portfolio" of all risky assets, but this, the financial economist Richard Roll argued, is not the same as the S&P 500 index or even the entire stock market; its true composition is unknown.

Tests of the efficient-market hypothesis by those who generally supported it led to the identification of "anomalies"—phenomena apparently at variance with the hypothesis—but the "failed" tests frequently led to practical action that had performative effects. The identification of anomalies gave rise to investment strategies to exploit them, and the pursuit of those strategies seems often to have reduced or eliminated the anomalies.

Chapter 4 also describes a path not taken by mainstream finance theory. In what became the standard model of changes in stocks' prices, the statistical distribution of changes in the natural logarithms of stock prices is the normal distribution—the canonical "bell-shaped" curve of statistical theory. This "log-normal" model is an example of what the mathematician and chaos theorist Benoit Mandelbrot calls "mild" randomness: the tails of the normal distribution, representing the probabilities of extreme events, are "thin." In the 1960s, Mandelbrot put forward a different model: one in which the tails are so "fat" that the standard statistical measure of a distribution's spread (the standard deviation or its square, the variance) is infinite.

Mandelbrot's model was of "wild" randomness: periods of limited price fluctuation can be interrupted unpredictably by huge changes. The model initially attracted considerable interest within financial economics (Eugene Fama, in whose work the efficient-market hypothesis crystallized, was the most prominent enthusiast for it), but, as chapter 4 describes, it also met fierce opposition because it undermined standard statistical procedures. In the words of one critic quoted in chapter 4, adopting Mandelbrot's model meant "consigning centuries of work to the ash pile."

Chapter 5 deals with how much options ought to cost, an apparently minor and esoteric problem in finance theory that nevertheless gave rise to a model

that some see as “the biggest idea in economics of the century” (Fama interview). In the period in which much of option theory was developed, options were “specialized and relatively unimportant financial securities” (Merton 1973a, p. 141) and were stigmatized by being associated widely with gambling and with market manipulation. However, option pricing seemed a tantalizingly straightforward “normal science” problem, in the terminology of Kuhn (1970). With an established model (the log-normal model) of how stock prices fluctuate, it did not seem too difficult to work out how much an option on that stock should cost. Options—and a particular form of option called a “warrant”—also offered opportunities to perform arbitrage (that is, to make low-risk profits from price discrepancies), and that was another reason for interest in the problem.

Finding a satisfactory solution to the problem of option pricing turned out to be harder than it looked. In chapter 5 the development of the eventually successful solution by Fischer Black and Myron Scholes is contrasted with the work of Edward Thorp, a mathematician famous for showing how to beat the house at blackjack by “card counting” (that is, keeping a careful, systematic mental record of the cards that have been played). Black and Scholes were trying to solve a theoretical problem by applying the Capital Asset Pricing Model; Thorp was working on option pricing without use of the CAPM, and his chief goal was identifying arbitrage opportunities.

The work by Black and Scholes, published in 1973, unleashed a torrent of further theoretical innovation. As they suggested, many other securities that on the surface did not look like options nevertheless had option-like features and so could be valued following the same approach, and, as noted above, the approach was also extended to the analysis of decisions as well as of securities. Among other contributors to option theory were Robert C. Merton and his mentor Paul Samuelson. They believed that the original version of the Capital Asset Pricing Model rested on objectionable assumptions.

Merton developed an approach to option pricing that led to the same equation that Black and Scholes had derived but which rested on different foundations. Merton’s derivation did not invoke the Capital Asset Pricing Model, although it too involved assumptions about markets that were markedly at odds with the actual conditions of the early 1970s. Other contributions to option theory quickly followed, and their level of mathematical sophistication rapidly grew. Merton had introduced the use of rigorous stochastic calculus, and by the end of the 1970s the problem of derivatives pricing was reformulated in terms of martingale theory, an advanced area of “pure mathematics.” The mathematical repertoire of Wall Street’s quantitative finance specialists (first called “rocket scientists,” then “quants”) was being assembled.<sup>54</sup>

Chapter 6 turns to the Chicago derivatives markets, the most important early site in which option theory was performed. It was above all in Chicago that the apparently quite unrealistic Black-Scholes-Merton model began to gain verisimilitude. (Black and Scholes took on board enough of Merton's derivation to justify the joint attachment all three names to the form eventually taken by the model.) The chapter traces how the Chicago financial derivatives exchanges emerged, how economics was deployed to provide the proposals for these exchanges with legitimacy in the face of suspicion that derivatives were dangerous wagers on price movements, and how the establishment of the new markets required collective action on the part of the memberships of the parent agricultural futures exchanges, the Chicago Mercantile Exchange and the Board of Trade. The chapter emphasizes the intensely bodily experience of trading in Chicago's apparently chaotic open-outcry pits, yet also notes how closely patterns of derivatives prices in those pits came to resemble those posited by the theory.

Was the theory's empirical success performative, and if so in what sense? Did "theory" and "reality" mesh because the former discovered preexisting patterns in the latter, or was "reality" transformed by the performance of theory? What made the Black-Scholes-Merton model, apparently an abstract, unrealistic professors' product, attractive to hard-bitten Chicago floor traders? When first formulated, the Black-Scholes equation was only an approximate fit to patterns of options prices. During the 1970s, however, the fit improved rapidly. Two processes seem to have been involved: market conditions began to change (albeit in many respects slowly) in ways that made the Black-Scholes-Merton model's assumptions more realistic; and, crucially, the model was employed in arbitrage, in particular in an arbitrage called "spreading," in which it was used to identify options that were cheap, or expensive, relative to each other.

Given the above discussion of econometric testing, it is worth remarking that a trader using spreading would have been exploiting—and thus reducing—precisely the discrepancies in options prices that were the focus of the most sophisticated econometric testing of the Black-Scholes-Merton model in this period. (As has been noted, this testing was conducted by the financial economist Mark Rubinstein.) It therefore seems plausible that the use of the model in spreading did more than add generally to its verisimilitude; spreading may have had a direct effect on specific features of price patterns examined in the model's econometric tests.

"Truth" *did* emerge—the fit between the Black-Scholes-Merton model and the Chicago option prices of 1976–1978 was good, by social-science standards,

on Rubinstein's tests—but it inhered in the process as a whole; it was not simply a case of correspondence between the model and an unaltered external reality. Knowledge, according to Latour, “does not reside in the face-to-face confrontation of a mind with an object. . . . The word ‘reference’ designates the quality of the chain in its entirety. . . . Truth-value circulates.” (1999, p. 69, emphases removed) The Black-Scholes-Merton model itself became a part of the chain by which its fit to “reality” was secured, or so chapter 6 conjectures.

“I have conceived of a society,” writes Barnes, “as a distribution of self-referring knowledge substantially confirmed by the practice it sustains” (1988, p. 166). The Black-Scholes-Merton model informed practices such as spreading, and those practices in their turn helped to create patterns of prices of which the model was a good empirical description. In that sense, the performativity of the model was indeed Barnesian.

The already reasonably close fit between the Black-Scholes-Merton model and Chicago stock-option prices became even better for index options, once such options, and also futures on stock-market indices, were introduced in the early 1980s. However, perhaps the Black-Scholes-Merton model succeeded because it was simply the right way to price options, but market participants learned that only slowly, with their markets only gradually becoming efficient? If that were so, “Barnesian performativity” would be an empty gloss on a process that could better be described in simpler, more conventional terms.

In chapter 7, however, I draw on the econometric literature on option pricing to note that after the 1987 crash the fit between the Black-Scholes-Merton model and patterns of market prices deteriorated markedly. A “volatility skew” or “smile” at odds with Black-Scholes emerged, and it seems to be durable; it has not subsequently diminished or vanished. Option theory has left its permanent imprint on the options markets: the theory is embedded in how participants talk and in technical devices that are essential to their markets. But what is performed in patterns of prices in those markets is no longer classic option-pricing theory.

The volatility skew thus reveals the historicity of economics, at least of the particular form of economics examined here. The U.S. markets priced options one way before 1987 and have priced them differently since, and the change was driven by a historical event: the 1987 crash. Chapter 7 also inquires into the mechanisms of that crash, focusing on the possible role in it of “portfolio insurance,” a technique for setting a “floor” below which the value of an investment portfolio will not fall. Since portfolio insurance was an application of option theory, this raises the issue of counterperformativity: perhaps the fit between option theory and reality was ended by an event in which one of its

own applications was implicated? Unfortunately from the viewpoint of analytical neatness, however, it seems impossible to determine how large a role portfolio insurance played in exacerbating the crash.

Chapter 8 turns to an episode with echoes of 1987: the 1998 crisis surrounding the hedge fund Long-Term Capital Management (LTCM). Because the partners who ran the fund included the finance-theory Nobel laureates Robert C. Merton and Myron Scholes, its near-failure (it was re-capitalized by a group of the world's leading banks) has often been blamed on blind faith in finance theory's models and has been seen as suggesting fatal flaws in those models. Much of the commentary on LTCM has been dismissive, and some of it has included personal attacks on those who were involved. Indeed, the tone of much of the commentary is an example of the coarsening of the "conversations" about markets referred to above.

Merton, Scholes, and the other partners in LTCM were well aware of the status of finance theory's models as engines of inquiry rather than exact reproductions of markets, and the details of the models used by LTCM were often far less critical to its activities than is commonly imagined. The episode *is* interesting from the viewpoint of the relationship between models and "reality," but not by way of the banal observation that the former are imperfect approximations to the latter.

What is crucial is that LTCM conducted arbitrage, the central mechanism invoked by finance theory. To be sure, there are differences between the "arbitrage" that theory posits and arbitrage as market practice. However, some of what LTCM did was quite close to the paradigmatic arbitrages of finance theory. Aspects of its trading were similar to the arbitrage invoked in Modigliani and Miller's classic proof, and LTCM's option-market activities resembled the arbitrage that imposes Black-Scholes-Merton option pricing.

One way to pursue a better understanding of the relationship between financial models and "reality" is by means of empirical research on arbitrage as market practice, and the case of LTCM is of interest from that viewpoint. A social process—imitation—was at the heart of LTCM's crisis. The hedge fund and its predecessor group of bond-market arbitrageurs, led by LTCM's founder John Meriwether at the investment bank Salomon Brothers, were extremely successful. That success led others to begin similar trading, to devote more capital to it, or (in the case of mortgage-backed securities) even to adjust the models they were using in order to bring them into harmony with the features that they inferred the model being used by Meriwether's group must possess.

The eventual result was what chapter 8 calls a "superportfolio": a large, unstable structure of partially overlapping arbitrage positions. An event that

was in itself less than cataclysmic—the Russian government’s default on its ruble-denominated bonds on August 17, 1998—caused that superportfolio to begin to unravel. Arbitrageurs who suffered losses in Russia had to begin selling other assets in the superportfolio; in an increasingly illiquid market, those sales caused prices to move sharply against the holders of the superportfolio, forcing further sales; and so on. Finally, in September 1998, LTCM itself became the subject of a self-fulfilling prophecy of failure strikingly similar to the classic example of such a process given in 1948 by Robert K. Merton.

Chapter 9, the book’s conclusion, discusses the model-building epistemic culture of finance theory, noting in particular the field’s ambivalent attitude to the empirical adequacy of its models, an ambivalence that can be seen not only in what theorists say on the topic but also in the practical actions they take in markets. The chapter draws together the threads of the book’s investigation of performativity, focusing especially on the extent to which finance theory brought into being that of which it spoke. A number of broader issues are then discussed, including “behavioral finance,” which draws on work in psychology on biases in human decision making to contest orthodox finance’s claims of market efficiency. This chapter pays particular attention to arbitrage, which is pivotal both in market practice and in the relations among orthodox finance, behavioral finance, and social studies of finance. The book ends by returning to the analogy between markets and technologies, and to the need for an informed politics of market design analogous to the politics of technology.





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## *Transforming Finance*

In the 1950s, finance was a well-established part of the curriculum in the business schools of U.S. universities. A typical course would cover matters such as “institutional arrangements, legal structures, and long-term financing of companies and investment projects” (Whitley 1986a, pp. 154–155). What Weston (1966, p. 4) calls “the chief textbook of finance” had for several decades been *The Financial Policy of Corporations* by Arthur Stone Dewing, a professor of finance at Harvard University. The text had first appeared in 1919, and by 1953 its 1,500 pages required two separate volumes.

Dewing began by presenting corporations as institutional and legal entities. He then discussed the ways in which corporations raise money by issuing securities, and described the different varieties of such securities. One main category was (and, of course, still is) stocks: those who buy a corporation’s stock gain rights of ownership in it, and if circumstances are favorable they receive periodic dividend payments. The other main category was and is bonds, a tradable form of debt. (A corporation’s bonds normally commit it to pay a set capital sum at a given date, and until then to pay set amounts in interest.) Dewing discussed the overall valuations of public utilities and of corporations, the basic techniques of accountancy, the causes and forms of business expansion, and the necessity sometimes to “remold the capital structure of the corporation” (Dewing 1953, p. 1175).

Dewing’s chapters contained many historical asides and some allusions to psychology. His view was that the “motives [that] have led men to expand business enterprises . . . on the whole . . . are not economic but rather psychological . . . the precious legacy of man’s ‘predatory barbarism’” (1953, p. 812). What his book did not contain was mathematics beyond simple arithmetic. His primary focus was on institutions and financial instruments, rather than on markets, and his account of those institutions and instruments was descriptive rather than analytical.

Dewing's textbook had been criticized by one of his Harvard colleagues even in 1943, but the main thrust of the criticism was that it was no longer fully up to date in the topics it covered, not that it should have been much more analytical or more mathematical (Hunt 1943).<sup>1</sup> In the 1950s, much research in finance remained descriptive and untheoretical. Peter Bernstein (1992, p. 46) notes that "at most universities, the business school and economics faculties barely greeted each other on the street." The *Journal of Finance*, which began publication in 1946, was the field's leading periodical. "Most of the articles the *Journal* published," Bernstein writes, "had to do with Federal Reserve policy, the impact of money on prices and business activity, taxation, and issues related to corporate finance, insurance, and accounting. The few articles that appeared under the rubric 'Investments' dealt with topics like liquidity, dividend policy, and pension funding. In issues up to 1959, I was unable to find more than five articles that could be classified as theoretical rather than descriptive. The rest contain plenty of numbers but no mathematics." (1992, p. 42)<sup>2</sup>

The topic of this chapter is the move from this predominantly descriptive and institutional approach to the academic study of finance to the analytical, economic, and increasingly mathematical viewpoint that is the focus of this book. The shift had three main early strands. (A fourth, somewhat later strand is option-pricing theory, to be discussed in chapter 5.)

One strand was the work of the economists Franco Modigliani and Merton Miller. It was the most direct early challenge to the older approach, and, in the words of a scholar whose work straddled the two approaches, it was the most important exemplar of the transformation of "the study of finance from an institutional to an economic orientation" (Weston 1989, p. 29).

A second strand of the transformation of the study of finance was the research of Harry Markowitz, William Sharpe, and others in "portfolio theory": the theory of the selection of optimal investment portfolios and of the economic consequences of investors behaving rationally in this respect.

The third strand was the random-walk and efficient-market hypotheses. These hypotheses offered iconoclastic accounts of the statistical form taken by stock-price changes and of the way prices incorporate relevant information. They can be traced back into the nineteenth century, but they came to decisive fruition in the United States in the 1950s and the 1960s.

### ***Modigliani and Miller***

The work of Modigliani and Miller emerged from one of the crucial cockpits of the emerging management sciences in the mid-twentieth-century United

States: the Graduate School of Industrial Administration at the Carnegie Institute of Technology (later Carnegie Mellon University). In 1948, William L. Mellon, the founder of the Gulf Oil Company, gave Carnegie Tech \$6 million to establish the school.

The new business school's three leaders were Lee Bach, its dean; Bill Cooper, an operations research scholar and economist; and Herbert Simon, an organization theorist who became a pioneer of artificial intelligence.<sup>3</sup> Bach, Cooper, and Simon saw "American business education at that time as a wasteland of vocationalism that needed to be transformed into science-based professionalism, as medicine and engineering had been transformed a generation or two earlier" (Simon 1996, p. 139).

The ambition to make Carnegie Tech's business school "science-based" implicitly raised a question, one that as early as 1951 saw the school sharply divided: on what sort of science should research and education in business be based? Herbert Simon helped inspire a "behavioral" account of firms, which was based in empirical studies and in organization theory and which differed radically from the traditional economic portrayal of firms as rational maximizers of profit. He "heckled" his economist colleagues at Carnegie Tech "about their ridiculous assumption of human omniscience, and they increasingly viewed me as the main obstacle to building 'real' economics in the school" (1996, p. 144).

Simon's views could not be ignored. He was the "decisive influence" on the Graduate School of Industrial Administration (Modigliani interview), and he had the ear of its dean. Cooper, the third of the school's leaders, disagreed sharply with Simon. Cooper even tried to have him step down from chairing the department of industrial management, accusing him of "intimidating" (Simon 1996, p. 144) the economists.

The economists in the Graduate School of Industrial Administration organized to defend themselves institutionally, but also responded intellectually. Thus John Muth, the initial proponent of the theory of rational expectations referred to in chapter 1, formulated the theory at Carnegie Tech and presented it explicitly as a response to Simon's accusation that economists presumed too much rationality in individuals and firms. The hypothesis of rational expectations "is based on exactly the opposite point of view" to Simon's, wrote Muth (1961, p. 316): "dynamic economic models do not assume enough rationality."<sup>4</sup>

Among Carnegie Tech's economists were Franco Modigliani (1918–2003), a rising star of the discipline, and Merton Miller (1923–2000), who was to become one of the leading scholars taking the new approach to the study of finance. The two men differed intellectually and politically. Modigliani, a

refugee from Italian fascism, was broadly a Keynesian. Miller's mentor—"a great influence in my life and in bringing me . . . to serious modern economics" (Miller interview)—was George Stigler, a University of Chicago colleague, ally, and close friend of Milton Friedman. (For example, Stigler and Friedman had traveled together in 1947 to the initial meeting of the Mont Pèlerin Society, crossing the Atlantic by ocean liner and stopping off in London and Paris.)<sup>5</sup>

Despite their differences, Modigliani and Miller, who had adjoining offices, found they had enough in common to build a productive, albeit temporary, partnership. Its products were not a direct response to Simon's "behavioral" critique of economics. Their first joint paper "was meant to upset my colleagues in finance," Modigliani recalled (1995, p. 153), not to upset Simon, whom they both respected despite the fact that they were both on the opposite side to him in the intellectual dispute that split the Graduate School of Industrial Administration.

Modigliani and Simon were collaborators and remained friends, but Modigliani, Simon recalled (1996, p. 271), "never mistook me for an ally in matters of economic theory." Miller told me that he saw Simon as "very hostile to economics." While organizational scholars focused on observable behavior, a pervasive concept in the financial economics to be described in this chapter was the expected rate of return on a stock. Simon frequently pointed out to Miller that this rate was not observable. "He'd say, 'well, I don't understand you finance people. How can you hope to build up a science of finance when the basic unit of your field is not observable.' . . . I had a lot of that from Herb. . . . But he was such a towering figure that I guess you put up with it." (Miller interview)

Modigliani and Miller's work addressed Carnegie's divide implicitly rather than explicitly. They tackled central topics in finance, but unlike much existing scholarship in the field they did not do so in an institutional fashion, and they were theoretical rather than descriptive in their approach. Modigliani and Miller argued that economic reasoning showed the essential irrelevance of what apparently were crucial from the viewpoint of an institutional or behavioral perspective on finance.

Their separate routes to their first joint paper are described below and in an endnote.<sup>6</sup> In the paper, they argued that in what they called a "perfect market" (Modigliani and Miller 1958, p. 268) neither the total market value of a corporation (the sum of the market values of its stocks and bonds) nor the average cost<sup>7</sup> to it of capital was affected by its "capital structure"—that is, by the extent to which it finances its activities by borrowing (issuing bonds) rather than by issuing stock.

A second paper (Miller and Modigliani 1961) similarly dismissed as irrelevant another apparently major issue: the choice of how much of a corporation's earnings to distribute as dividends to its stockholders, and how much to retain within the corporation. "In a rational and perfect economic environment" it should not matter "how the fruits of the earning power [of a corporation's assets] are 'packaged' for distribution" to investors, Miller and Modigliani argued (p. 414).

High dividends would reduce investors' capital gains; low dividends would mean higher capital gains. However, if the firm's substantive activities were unaltered (as Miller and Modigliani assumed), to change dividend policy would be to affect only "the distribution of the total return in any period as between dividends and capital gains. If investors behave rationally, such a change cannot affect market valuations." (1961, p. 425)

Let me concentrate on the first of Modigliani and Miller's claims: the irrelevance of capital structure. Stock and bonds have very different characteristics—as noted above, the first is a form of ownership, the second of debt—so the balance between the two looked important. Bonds were seen as a safer investment than stocks (in the 1950s, the reputation of stocks had still not recovered fully from the disasters of the interwar years), yet taking on too much debt made a corporation look risky.

It therefore seemed plausible that there should be an optimum balance between the issuing of stocks and of bonds. One might expect that this optimum balance would depend on investors' attitudes to risk and on matters such as the "psychological and institutional pressures" (Modigliani and Miller 1958, p. 279) on investors to hold investment portfolios of bonds rather than stocks. Those pressures were still strong in the 1950s.

The argument (outlined in more detail in appendix A) by which Modigliani and Miller sought to sweep aside such behavioral and institutional issues was as follows. Suppose that two investments are "perfect substitutes" (Modigliani and Miller 1959, p. 656), in other words that they are entitlements to identical income streams. If the prices of the two investments are not the same, then any holder of the dearer investment can benefit by selling it and buying the cheaper. Nothing need be assumed about investors' willingness to take on risk, or about any "psychological" or "institutional" matters, other than that "investors always prefer more wealth to less" (Miller and Modigliani 1961, p. 412). "The exchange" of the dearer investment for the cheaper, Modigliani and Miller wrote, would be "advantageous to the investor quite independently of his attitudes to risk" (1958, p. 269).

Imagine two firms with identical expected earnings, identical levels of risk associated with those earnings, but different capital structures. Modigliani and

Miller argued that if the total market values of the two firms differed, then “arbitrage”—the above switch from the dearer to the cheaper investment—“will take place and restore” equality of the two firms’ market values (1959, p. 259). By conducting one or other of the switches of investment described in appendix A, investors could take advantage of any discrepancy in market values while leaving themselves with a future income stream of the same expected size and same level of risk. “What we had shown was, in effect, what equilibrium means,” said Miller. “If . . . you can make an arbitrage profit then that market is not in equilibrium, and if you can show that there are no arbitrage profits then that market is in equilibrium.” (Miller interview)

As was noted above, Modigliani and Miller’s claim of the irrelevance of capital structure (and also their claim of the irrelevance of dividend policy) rested on the assumptions of a “perfect market”:

. . . no buyer or seller (or issuer) of securities is large enough for his transactions to have an appreciable impact on the then ruling price. All traders have equal and costless access to information about the ruling price and about all other relevant characteristics of shares. . . . No brokerage fees, transfer taxes, or other transaction costs are incurred when securities are bought, sold, or issued, and there are no tax differentials either between distributed and undistributed profits or between dividends and capital gains. (Miller and Modigliani 1961, p. 412)

Were any of these, or any of Modigliani and Miller’s other assumptions (for example, that investors can buy stocks on credit) not to hold, then capital structure or dividend policy might no longer be irrelevant.

Modigliani and Miller knew perfectly well that they were assuming a world that did not exist. Taxation was the most obvious difference between their assumed world and empirical reality. American corporations were (and are) allowed to set interest payments on their bonds and other debts against their tax liabilities, but cannot do so for dividends on their stock. Until 2003, almost all individual investors in the United States faced a higher rate of tax on dividend income than on capital gains, and they can postpone the tax on capital gains until they actually sell their stock, so they may have good reasons to receive the benefits of a firm’s earnings as capital gains rather than as dividends. Modigliani and Miller were fully aware that matters such as this could invalidate their “irrelevance” propositions.

Modigliani and Miller’s intellectual strategy was to start with a highly simplified but in consequence analytically tractable world. Miller’s mentor, George Stigler, had been one of three economists whose “helpful comments and criticisms” were acknowledged by Friedman at the start of “The Methodology of Positive Economics” (1953a, p. 3). Stigler agreed with Friedman that “economic theorists, like all theorists, are accustomed (nay, compelled) to deal with

simplified and therefore unrealistic ‘models’ and problems” (Stigler 1988, p. 75).

Having shown the irrelevance of capital structure and of dividend policy in their simple assumed world, Modigliani and Miller then investigated the consequences of allowing some more realism (especially in regard to taxes) back in. Just how far to go in adjusting their model to “reality” and the exact consequences of doing so became matters of dispute between Modigliani and Miller. Attuned, as Keynes had been, to the imperfections of markets, Modigliani was the more cautious. Indeed, when first describing the irrelevance of capital structure to a class at Carnegie Tech, he distanced himself: “I announced the theorem and said ‘I don’t believe it.’” (Modigliani interview)

Miller, in contrast, was prepared more radically to set aside the question of the validity of assumptions, in the manner advocated by Friedman (Miller interview). Modigliani and Miller’s published joint work trod a middle path—they explored the consequences of the simple assumptions of a “perfect market,” but also attended carefully to the effects of relaxing those assumptions—but private disagreement between them emerged over the range of conditions under which their propositions would hold, in particular in respect to the tricky question of the effects of corporate and personal taxes (Miller interview; Modigliani interview).

Despite these incipient disagreements, Modigliani and Miller found themselves on the same side with respect to traditional finance scholarship, just as they had with respect to Herbert Simon’s critique of orthodox economic reasoning. Their sharpest dispute was with David Durand, a prominent finance scholar of a more traditional, institutional bent who held a professorship of industrial management at MIT. Durand had himself examined what was in effect Modigliani and Miller’s proposition about the irrelevance of capital structure, and had at least hinted that arbitrage might in principle enforce it. Ultimately, however, he had rejected the proposition.

Institutional restrictions had seemed to Durand to be sufficiently strong to make capital structure relevant, in particular to favor bonds over stocks:

Since many investors in the modern world are seriously circumscribed in their actions, there is an opportunity to increase the total investment value of an enterprise by effective bond financing. Economic theorists are fond of saying that in a perfectly fluid world one function of the market is to equalize risks on all investments. If the yield differential between two securities should be greater than the apparent risk differential, arbitrageurs would rush into the breach and promptly restore the yield differential to its proper value. But in our world, arbitrageurs may have insufficient funds to do their job because so many investors are deterred from buying stocks or low-grade bonds, either by law, by personal circumstance, by income taxes, or even by pure prejudice. These restricted investors, including all banks and insurance companies, have to bid for



high-grade investments almost without regard to yield differentials or the attractiveness of the lower grade investments. And these restricted investors have sufficient funds to maintain yield differentials well above risk differentials. The result is a sort of super premium for safety; and a corporation management can take advantage of this super premium by issuing as many bonds as it can maintain at a high rating grade. (Durand 1952, pp. 230–231)

Hearing Durand's paper at a June 1950 conference on "Research in Business Finance" had led Modigliani to his initial interest in the problem. A significant part of the argument of Modigliani and Miller's 1958 paper had been aimed precisely at showing that the institutional matters of the kind invoked by Durand were not sufficient to make capital structure relevant.

What divided Modigliani and Miller from Durand was at root whether market processes, in particular arbitrage, were strong enough to overcome the effects of institutional restrictions. Modigliani and Miller held that they were. Durand did not believe that, and he published an extensive critique of Modigliani and Miller's claim of the irrelevance of capital structure. He did not deny the logic of their basic reasoning—as noted above, he had himself explored the path they took—but he claimed that their analysis held only in a "limited theoretical context." The situation of what Durand called "real corporations" (1959, p. 640) was quite different, and the market they had to interact with was far from Modigliani and Miller's assumption of perfection.

For example, investors could not buy stock entirely on credit: to do so was prohibited by the Federal Reserve's famous "Regulation T," introduced after the credit-fueled stock-market excesses of the 1920s. Regulation T restricted the extent to which brokers could lend investors money to buy stock. From time to time the Federal Reserve altered the percentage of the cost of a stock purchase that could be borrowed, but in the period discussed here it was typically no more than 50 percent, and sometimes much less. The "arbitrage" operation of switching between investments that Modigliani and Miller invoked might therefore not be available to most investors, and was not free of risk: price fluctuations might lead stocks bought on credit no longer to be worth enough to serve as collateral for the loan, leading an investor's broker forcibly to sell such stock.

Modigliani and Miller's basic conceptual device, homogeneous "risk classes" (see appendix A), had no empirical referent, argued Durand: "To the practically minded, it is unthinkable to postulate the existence of two or more separate and independent corporations with income streams that can fluctuate at random and yet be perfectly correlated from now until doomsday." Modigliani and Miller had started "with a perfect market in a perfect world," wrote Durand. Their examination of the consequences of relaxing their assumptions

was inadequate: “. . . they have taken a few steps in the direction of realism; but they have not made significant progress” (Durand 1959, p. 653).

### ***Portfolio Selection***

Unpalatable as Modigliani and Miller’s assertion of the irrelevance of capital structure might be to a more traditional finance scholar such as Durand, it was undeniably a contribution to economics, published as it was in what was for many the discipline’s premier journal, the *American Economic Review*. The second major strand in the transformation of finance predated Modigliani and Miller’s contributions but was initially far more precarious in its position with respect to economics than theirs.

The initiator of this strand was Harry Markowitz.<sup>8</sup> Born in 1927, the son of Chicago grocers, Markowitz studied economics at the University of Chicago. He received his M.A. in 1950 and became a student member of the Cowles Commission for Research in Economics, then located in Chicago. The Cowles Commission was one of the crucial sites of the mathematicization of postwar U.S. economics, and one of the places where the quantitative techniques of operations research—which had come to prominence in military applications in World War II—had their greatest effect on economics (Mirowski 2002).

The Cowles Commission was a lively, exciting place, buzzing with ideas, many of them from émigré European economists. Herbert Simon, who attended its seminars while teaching at the Illinois Institute of Technology in the 1940s, later recalled:

A visitor’s first impression of a Cowles seminar was that everyone was talking at once, each in a different language. The impression was not wholly incorrect. . . . But the accents may have been more a help than a hindrance to understanding. When several speakers tried to proceed simultaneously, by holding tight to the fact that you were trying to listen to say, the Austrian accent, you could sometimes single it out from the Polish, Italian, Norwegian, Ukrainian, Greek, Dutch, or Middle American. As impressive as the cacophony was the intellectual level of the discussion, and most impressive of all was the fact that everyone, in the midst of the sharpest disagreements, remained warm friends. (Simon 1996, p. 102)

A chance conversation with a broker pointed Markowitz toward the stock market as the subject for his Ph.D. thesis (Markowitz interview). Nowadays, this might seem a natural topic for an ambitious young graduate student, but that was not the case at the start of the 1950s. For example, the elite students of the Harvard Business School shunned Wall Street: in the early 1950s, only 3 percent of them took jobs there. “The new generation considered it

unglamorous. Outside its gargoyled stone fortresses, black limousines waited for men with weary memories. Inside, it was masculine, aging, and unchanged by technology.” (Lowenstein 1995, p. 52)

Wall Street’s low prestige spilled over into academic priorities. At the Harvard Business School, the course on investments was unpopular with students and was allocated an undesirable lunchtime slot, earning it the unflattering sobriquet “Darkness at Noon” (Bernstein 1992, p. 110). “[A]cademic suspicion about the stock market as an object of scholarly research” was sufficiently entrenched that in 1959 the statistician Harry Roberts of the University of Chicago could describe it as “traditional” (1959, p. 3).

In 1950 the economists of the Cowles Commission did not specialize in stock-market research—Markowitz was sent for a reading list to Marshall Ketchum, who taught finance in Chicago’s Graduate School of Business—but they were not in a position to dismiss the topic out of hand. The commission’s initial patron, Alfred Cowles III, grandson of the co-founder of the *Chicago Tribune*, had helped in his father’s investment counseling business and in 1928 had started keeping track of the performance of investment advisory services, publishing an analysis showing this performance generally to be poor (Cowles 1933; Bloom 1971, pp. 26–31). More fundamental, however, than this organizational pedigree for Markowitz’s topic was the fact that the approach he took to it was primed by a course on operations research taught by the Cowles economist Tjalling C. Koopmans (Markowitz interview).<sup>9</sup>

Among the books that Ketchum recommended to Markowitz was one of the few that offered not practical guides to stock-market investment but a systematic account of what stocks ought to be worth. John Burr Williams’s *Theory of Investment Value* (1938) put forward what has become known as the “dividend discount model,” the basic idea of which seems to have come from stock-market practice rather than from academia.

The value of a corporation’s stock is ultimately as an entitlement to the future stream of dividends paid to the stockholders by the corporation, argued both Williams and the market practitioners on whom he drew.<sup>10</sup> The emphasis on dividends seems to be contradicted by the later Miller-Modigliani assertion of the irrelevance of dividend policy, but Williams anticipated the objection that dividend policy was arbitrary by arguing that if corporate earnings that are not paid out as dividends can profitably be reinvested then they will enhance future dividends, and so be taken account of in his model (Williams 1938, pp. 57–58).

Expected future dividend payments cannot, however, simply be added up in order to reach a value for a corporation’s stock. In part, that is because of the effect of inflation, but even without inflation the value of a dollar received

in a year's time is less than that of a dollar received now, because the latter can be invested and earn interest. To work out the value of a stock, expected future dividends have therefore to be "discounted": their present value has to be calculated using an appropriate interest rate.<sup>11</sup> Hence the name "dividend discount model."

The difficulty of reliably estimating future dividends was one obvious objection to Williams's model. (The practitioners on whose work he built seem to have used the model "in reverse" to calculate the rate of dividend growth implied by a stock price, so as to check whether that price seemed reasonable.)<sup>12</sup> Nor was Williams, writing as he was in the aftermath of the huge rise in stock prices in the 1920s and the subsequent calamitous crash, confident that investors used anything approximating to his model. He claimed only that "gradually, as men do become more intelligent and better informed, market prices should draw closer to the values given by our theory" (Williams 1938, p. 189).

When Markowitz read *The Theory of Investment Value* in the library of the University of Chicago's Graduate School of Business in 1950, he was struck by a different objection to Williams's dividend discount model, one rooted in the operations research that Koopmans was teaching him. Williams's response to the obvious uncertainties involved in calculating the theoretical value of a security was to recommend weighting possible values by their probability and calculating the average, thus obtaining what mathematicians call the "expected value" (Williams 1938, p. 67).

What would happen, Markowitz asked himself, if one applied Williams's way of tackling uncertainty not to a single stock but to an investor's entire portfolio? "If you're only interested in the expected value of a stock, you must be only interested in the expected value of a portfolio" (Markowitz interview). Simple reasoning based on the operations-research technique of linear programming quickly convinced Markowitz that an investor who focused only on expected value would put all his or her money into the single stock with the highest expected rate of return.

Plainly, however, investors did not put all their money into one stock: they diversified their investments,<sup>13</sup> and they did so to control risk. Optimal portfolio selection could not be about expected return alone: "You've got two things—risk and return." (Markowitz interview) Risk, Markowitz reasoned, could be thought of as the variability of returns: what statisticians call their "standard deviation," or the square of that standard deviation, their "variance." Asked by me why he had conceived of risk in this way, Markowitz simply cited how often he had come across the concept of standard deviation in the statistics courses he had taken (Markowitz interview).

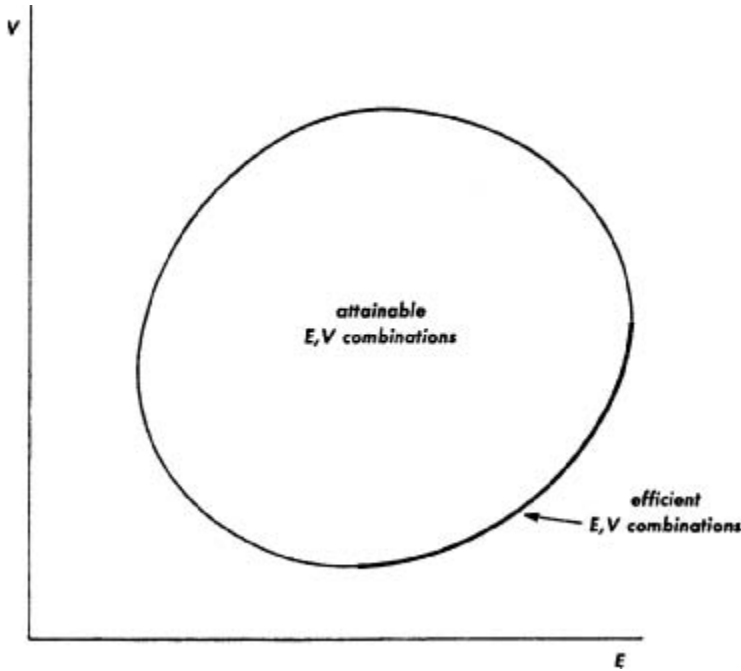
For Markowitz, being trained as he was in operations research as well as in economics, the problem of selecting optimal investment portfolios could then be formulated as being to find those portfolios that were “efficient,” in other words that offered least risk for a given minimum expected rate of return, or greatest return for a given maximum level of risk. The assignment Koopmans had set the students in his course on operations research was to “find some practical problem and say whether it could be formulated as a linear-programming problem” (Markowitz interview).

Markowitz quickly saw that once risk as well as return was brought into the picture the problem of finding efficient portfolios was *not* a linear one. The formula for the standard deviation or variance of returns involves squaring rates of return, so the problem could not be solved using existing linear programming techniques: it fell into what was then the little-explored domain of quadratic programming.<sup>14</sup> Koopmans liked Markowitz’s term paper, giving it a grade of A, and encouraged him to take the problem further, telling him: “It doesn’t seem that hard. Why don’t you solve it?” (Markowitz interview)

Solve the problem of selecting efficient portfolios was precisely what Markowitz went on to do. He worked on the topic in his remaining months at Chicago and then in time left over from his duties at the Rand Corporation, to which he moved in 1952. The Santa Monica defense think tank was a natural destination for a young scholar in whose work economics and operations research were hybridized.<sup>15</sup>

Markowitz’s solution to the problem of portfolio selection presumed that estimates of the expected returns, variances of returns, and correlations<sup>16</sup> of returns of a set of securities could be obtained, for example by administering questionnaires to securities analysts. It was then possible to work out the expected return ( $E$ ) and variance of return ( $V$ ) of any portfolio constructed out of that set of securities. Markowitz provided a simple graphical representation (figure 2.1) of the set of combinations of  $E$  and  $V$  that were “attainable” in the sense that out of the given set of securities a portfolio can be constructed that offered that combination of  $E$  and  $V$ .<sup>17</sup>

Of the set of attainable portfolios, a subset (shown by the thicker “south-east” edge of the attainable set) is going to be efficient. From points in this subset one cannot move down (to a portfolio with the same expected return but lower variance) or to the right (to a portfolio with the same variance but greater expected return) without leaving the attainable set. The subset therefore represents the portfolios that offer “minimum  $V$  for given  $E$  or more and maximum  $E$  for given  $V$  or less” (Markowitz 1952, p. 82): it is what was later to be called the “efficient frontier” of the set of portfolios that can be constructed from the given securities.



**Figure 2.1**

The combinations of expected return ( $E$ ) and variance of return ( $V$ ) that can be attained by constructing an investment portfolio from a set of securities. (Source: Markowitz 1952, p. 52. Courtesy of Harry Markowitz and Blackwell Publishing Ltd.) Markowitz was later to realize that the “inefficient” boundary of the attainable set is not correctly represented here.

Markowitz’s graphical representation was, however, only a way of explaining the underlying idea. To calculate the “efficient frontier” in any particular case (given a set of securities and estimates of their expected returns, variances, and correlations) is computationally demanding. When the set of securities is even moderately large, it is beyond the capacity of an unaided human being. Because the problem fell into a mathematical domain that operations researchers were only beginning to explore, there was no “off-the-shelf” algorithm Markowitz could turn to: it was difficult, innovative work. The Rand-influenced quadratic-programming core of Markowitz’s analysis, his “critical line algorithm,” was good enough to merit publication in the newly established journal of operations research, the *Naval Research Logistics Quarterly* (Markowitz 1956).

Markowitz’s 1952 paper in the *Journal of Finance* describing his portfolio-selection method (though not the detail of the critical line algorithm) was later

to be seen as the “harbinger” of the “new” finance, of “the mathematical and model-building revolution.”<sup>18</sup> It certainly stood out: “No other article in the issue [of the *Journal of Finance*] that carried Markowitz’s paper contains a single equation.” (Bernstein 1992, p. 42) Indeed, Markowitz’s contribution to the *Naval Research Logistics Quarterly*, with its matrix algebra and its careful analysis of the mathematical properties of the critical line algorithm, inhabited an epistemic culture quite different from that traditional in the academic study of finance: essentially the culture of applied mathematics.

The impact in the 1950s of Markowitz’s work was quite limited. Investment practitioners showed effectively no interest in his technique. Nor did the finance scholars active in the 1950s generally seize on it. Modigliani and Miller’s critic David Durand reviewed the 1959 Cowles monograph in which Markowitz systematically presented his technique and its justification. He conceded that Markowitz’s would “appeal to econometricians and to statisticians interested in decision theory,” but he saw “no obvious audience” for Markowitz’s overall approach (Durand 1960, p. 234).

It was wholly unrealistic, Durand suggested, to imagine that real-world portfolio selection proceeded according to Markowitz’s techniques, or perhaps even that it ever *could* proceed in this fashion:

His argument rests on the concept of the Rational Man, who must act consistently with his beliefs. But the history of Wall Street suggests that such consistency may be unwise. . . . His ideal is a Rational Man equipped with a Perfect Computing Machine. . . . Of course, he admits that the Rational Man does not exist at all and that the Perfect Computing Machine will not exist in the foreseeable future, but the image of these nonentities seems to have colored his whole work and given it an air of fantasy.<sup>19</sup>

It was not even clear that what Markowitz had done counted as economics. Milton Friedman was on Markowitz’s Ph.D. board. In 1954, on his way back from Washington to Rand’s Santa Monica headquarters, Markowitz stopped off in Chicago for his thesis defense, thinking to himself “This shouldn’t be hard. I know this stuff. . . . Not even Milton Friedman will give me a hard time.” (Markowitz interview) “So about two minutes into my defense,” Markowitz continues, “Friedman says, ‘Well, Harry, I’ve read your dissertation and I don’t find any mistakes in the math, but this isn’t a dissertation on economics and we can’t give you a Ph.D. in economics for a dissertation that’s not economics.’” Markowitz did receive the degree, but although Friedman cannot recall making the remark (“You have to trust Harry for that”) he believes it would have been justified: “What he did was a mathematical exercise, not an exercise in economics.” (Friedman interview)

The leading economist who responded most positively to Markowitz was James Tobin. Broadly Keynesian in his approach, Tobin succeeded Tjalling

Koopmans as head of the Cowles Commission, and was instrumental in it moving in 1955 from Chicago to Yale University, where Tobin taught. There had been tensions between the Cowles Commission and Friedman and his colleagues in the University of Chicago's economics department, and recruitment was becoming more difficult because of worsening conditions in the South Chicago neighborhoods that surrounded the university (Bernstein 1992, p. 67).

Like Markowitz, Tobin was interested in portfolio selection, but from a different viewpoint. "Markowitz's main interest is prescription of rules of rational behavior for investors," wrote Tobin. His concern, in contrast, was the macroeconomic implications "that can be derived from assuming that investors do in fact follow such rules" (Tobin 1958, p. 85).<sup>20</sup>

For Tobin, a crucial issue was the choice investors made between owning risky assets and holding money. This choice was important to Keynesian theory, because the extent to which people prefer holding money (the extent of their "liquidity preference") will affect interest rates and macroeconomic phenomena such as the levels of economic activity and of unemployment.<sup>21</sup> Tobin simplified the issue by proving, in a mathematical framework similar to Markowitz's, what has become known as the "separation theorem," which asserts the mutual independence of choice among risky assets and choice between such assets and cash. In other words:

... the proportionate composition of the non-cash [risky] assets is independent of their aggregate share of the investment balance. ... Breaking down the portfolio selection problem into stages at different levels of aggregation—allocation first among, and then within, asset categories—seems to be a permissible and perhaps even indispensable simplification both for the theorist and for the investor himself. (Tobin 1958, pp. 84–85)

Tobin's work thus suggested a route by which what Markowitz had done could be connected to mainstream economic concerns. However, after finishing his Cowles monograph on portfolio selection, Markowitz was disinclined to work on the topic. "My book [Markowitz 1959] was really a closed logical piece," he told a 1971 interviewer. "I'd really said all I wanted to, and it was time to go on to something else." (Welles 1971, p. 25) His interests had already begun to shift to problems more central to the work being done at the Rand Corporation, notably the applied mathematics of linear programming (Markowitz 1957) and the development of SIMSCRIPT, a programming language designed to facilitate the writing of software for computer simulations.<sup>22</sup>

### ***The Underlying Factor***

In 1960 a "young man . . . dropped into" Markowitz's office at Rand (Markowitz 2002b, p. 383). William Sharpe (born in Cambridge, Massachusetts in 1934) had



completed his examinations as a graduate student in economics at the University of California at Los Angeles in 1960. His initial idea for a Ph.D. topic had been a study of “transfer prices,” the prices that are set internally for goods moving between different parts of the same corporation, but his proposal had met with an unenthusiastic reaction.

Unusually, however, Sharpe had substituted finance for one of the five “fields” of economics he had been required to study preparatory to his Ph.D. His studies in finance had been guided by UCLA’s J. Fred Weston, who was of the older generation of finance scholars but who was sympathetic to the new work described in this chapter.<sup>23</sup> Weston introduced Sharpe to what Markowitz had done: “I loved . . . the elegance of it. The fact it combined economics, statistics [and] operations research.” (Sharpe interview) When his mentor in economics, UCLA professor Armen Alchian, helped Sharpe obtain a junior economist’s post at Rand, Sharpe sought out Markowitz.

Markowitz agreed to become the informal supervisor of Sharpe’s UCLA Ph.D. thesis. Despite Markowitz’s sense that his work was a “closed logical piece,” there was one issue that he had broached but not fully pursued. His full version of portfolio selection required getting securities analysts to estimate the correlation of every pair of securities among which selection was to be made. The number of such correlations increased rapidly with the number of securities being analyzed. Selection among 1,000 securities, for example, would require estimation of 499,500 correlations (Baumol 1966, pp. 98–99).

No stock analyst could plausibly estimate half a million correlations. Statistical analysis of past correlations was not enough, even if the data for it had been easily available, which in the 1950s they were not, because what was needed for portfolio selection were future correlations. Even if estimates of the latter could somehow be produced, the limited sizes of computer memories in the 1950s and the early 1960s put the resultant computation well beyond the bounds of the feasible.

While at Yale (at Tobin’s invitation) in 1955–56, Markowitz found that a 25-security problem was beyond the capacity of the computing resources available to him (Markowitz 2002b, p. 383). Even on an IBM 7090, in the early 1960s a state-of-the-art digital computer, the Rand quadratic programming code implementing portfolio selection could not handle a problem with more than 249 securities.<sup>24</sup>

Markowitz had realized, however, that the difficulties of estimation and computation when selecting among large numbers of securities would be alleviated if it could be assumed that the correlation between securities arose because they were each correlated with “one underlying factor, the general prosperity of the market as expressed by some index” (Markowitz 1959, p.

100). Instead of asking securities analysts to guess a huge array of cross-correlations, they would have to estimate only one correlation per security: its correlation with the index.

It was with Markowitz's suggested simplification that Sharpe began his Ph.D. work, employing a model in which "the returns of . . . securities are related only through common relationships with some basic underlying factor" (Sharpe 1963, p. 281). He found that the simplifying assumption did indeed reduce computational demands dramatically: large portfolio selection problems became feasible for the first time.

Sharpe's development of Markowitz's "underlying factor" model could still be seen as operations research rather than economics: it was published in *Management Science* (Sharpe 1963) rather than in an economics journal. At root, though, Sharpe was an economist, and Armen Alchian had taught him microeconomics—the foundational part of the discipline that studies how production and consumption decisions by rational firms and individuals shape market prices, giving rise to equilibrium in competitive markets.

Although the concept of "equilibrium" is complex, a simple notion of "equilibrium price" is the price at which the quantity of a commodity that suppliers will sell is equal to the quantity that buyers will purchase. If the market price is below that equilibrium level, there is excess demand: purchasers want to buy more than suppliers will sell, allowing the latter to raise prices. If the price is above equilibrium, there is excess supply: purchasers will not buy the full amount that suppliers want to sell, forcing the latter to lower their prices in order to clear their stocks.

Sharpe went beyond the issue of how a rational investor should select an investment portfolio. "I asked the question that microeconomists are trained to ask. If everyone were to behave optimally (here, follow the prescriptions of Markowitz's portfolio theory), what prices will securities command once the capital market has reached *equilibrium*?" (Sharpe 1995, pp. 217–218). In order to make the answer to this question "tractable" (*ibid.*, p. 218), he assumed that the underlying-factor model applied and also that all investors had the same estimates of expected returns, of variances, and of correlations with the underlying factor. These simplifying assumptions yielded a model in which, using primarily graphical reasoning, Sharpe could identify a precise mathematical formulation of equilibrium.

In equilibrium, securities prices would be such that there would be a simple straight-line relationship between the expected return on a security and the extent of its covariation with the posited underlying factor. "Following the conventions" of the standard statistical technique of regression analysis, Sharpe used the Greek letter  $\beta$  (beta) to designate the extent of the sensitivity of the

returns on a stock or other security to changes in the underlying factor. “Thus the result could be succinctly stated: securities with higher betas will have higher expected returns.” (Sharpe 1995, p. 218)

The logic of the diversification of investment portfolios gave an intuitive explanation of the dependence of expected return on beta. The effects on the performance of a portfolio of the idiosyncratic risks of a particular stock can be minimized by diversification, but one cannot in that way eliminate the risk resulting from stocks’ correlation with a factor underlying the performance of all stocks. Thus, in equilibrium, a stock that was highly sensitive to changes in that underlying factor (in other words, a stock with a high beta) would have to have a price low enough (and thus an expected return high enough) to persuade investors to include it in their portfolios. A stock that was not very sensitive to changes in the underlying factor (a stock with a low beta) would in contrast command a higher relative price (and thus a lower expected return).

The linear relationship between beta and expected return was a strikingly elegant result. However, Sharpe knew that by assuming a single common underlying factor he had “put the rabbit in the hat” in the first place, and “then I pull it out again and how interesting is that?” (Sharpe interview). That is, his modeling work might be viewed as trivial. So he set to work (again proceeding to a large extent geometrically, using graphical representations akin to Markowitz’s diagram shown in figure 2.1)<sup>25</sup> to find out whether he could prove his result for “a general Markowitz world rather than this specific single factor. And happily enough, in a matter of relatively few months, guess what, it turns out you get the same result.” (Sharpe interview)

If investors are guided only by risk and return, and if they all have the same estimates of assets’ expected returns, risks, and mutual correlations,<sup>26</sup> then, Sharpe’s analysis showed, the prices of assets had to adjust such that in equilibrium there was a straight-line relationship between the expected return on an asset and its beta, the extent of its sensitivity to the return on an optimal portfolio. This result was at the core of what was soon to become known as the Capital Asset Pricing Model (CAPM).

### ***Assumptions, Implications, and Reality***

Sharpe knew perfectly well that his model rested on assumptions that were “highly restrictive and undoubtedly unrealistic” (Sharpe 1964, p. 434). In presenting his work, he encountered the attitude that “this is idiotic because he [Sharpe] is assuming everybody agrees [in their estimates of expected returns, risks, and correlations] and that’s patently false and therefore a result that

follows from that strong and totally unrealistic presumption isn't worth [much]" (Sharpe interview).<sup>27</sup>

In the paper in the *Journal of Finance* in which he laid out the CAPM, Sharpe defended it against the accusation that it was unrealistic with an implicit invocation of Milton Friedman's methodological views, discussed in chapter 1. "The proper test of a theory," wrote Sharpe, "is not the realism of its assumptions but the acceptability of its implications" (Sharpe 1964, p. 434). Aside from the likelihood that he would have said "accuracy" rather than "acceptability," Friedman himself could have written the sentence.

Sharpe's mentor Alchian shared many of Friedman's convictions. He too was a member of the Mont Pèlerin Society, and like Friedman he regarded whether a model's assumptions were realistic as irrelevant.<sup>28</sup> Sharpe remembers that he and Alchian's other graduate students had "drilled into" them the view that "you don't question the assumptions. You question the implications and you compare them with reality."<sup>29</sup>

More than 40 years later, Sharpe can still recall the Darwinian analogy that Alchian used to convey the irrelevance of the verisimilitude of assumptions: "Assume that creatures crawled up from the primordial slime, looked around and decided that for maximum effect they should grow opposable thumbs." The assumption that anatomical change resulted from conscious judgment of usefulness was plainly absurd, but "the prediction of the model [the development of opposable thumbs] would be correct, even though the actual mechanism (evolution) was very different."<sup>30</sup>

The fact that Sharpe was in this respect "very much in the Friedman camp at the time" meant that he did not allow making unrealistic assumptions to disturb him as he practiced what Alchian had taught in respect to modeling. As he puts it, his approach was to "take the problem, try to distil out the two or three most important things, build a logically coherent model . . . that has those ingredients in it and then see whether or not this can help you understand some real phenomenon" (Sharpe interview).

Sharpe's choice of a word when discussing "the proper test of a theory"—"*acceptability* of [a theory's] implications," not *accuracy*—was, however, not accidental. Although he was not concerned about the lack of verisimilitude of his assumptions, his theory seemed to have a highly unrealistic implication that he worried might lead others to reject the model out of hand. His original mathematical analysis led to the conclusion that "there is only one portfolio of risky securities that's optimal" (Sharpe interview).

Investors who were averse to risk would wish to hold much of their portfolio in the form of riskless assets (such as government bonds held to

maturity) and only a small amount of the optimal risky portfolio; risk-seeking investors might even borrow money to increase their holdings of the latter. However, what was in effect Tobin's separation theorem pertained, and Sharpe's model seemed to suggest that no investor would hold any combination of risky assets other than the unique optimal portfolio. All investors would, for example, hold exactly the same set of stocks, in exactly the same proportions.

Clearly, this implication of Sharpe's model was empirically false. Sharpe balked—"I thought, well, nobody will believe this. This can't be right"—and he tweaked his mathematics to avoid the unpalatable conclusion. "I wanted ever so much for there to be multiple risky portfolios that were efficient . . . it does not come naturally" out of the mathematics (Sharpe interview). In the analysis in his published paper, multiple optimal portfolios—albeit all perfectly correlated with each other—are indeed possible, and Sharpe told his readers that "the theory does not imply that all investors will hold the same combination" of risky assets (Sharpe 1964, p. 435).

The world was, however, changing, in ways to be discussed in chapter 3, in regard to the view that all rational investors will hold portfolios of risky assets that are identical in their relative composition. In the mid 1960s—Sharpe cannot date it more precisely than that—he allowed himself to return to the "egregious" (Sharpe interview) implication that all investors would hold the same portfolio. He knew that the way in which he had reached the conclusion to the contrary in his 1964 paper "was really a matter of wanting it" to be so (Sharpe interview). Gradually, he let himself embrace and put forward the counterintuitive conclusion that all investors would hold the same portfolio of risky assets.

If there was a single optimal portfolio, it was clear what that portfolio had to be. Prices would adjust such that no capital asset remained without an owner and every investor would hold every risky capital asset—every stock, for instance—in proportion to its market value. Sharpe: "When I finally broke out of [the view that there had to be more than one optimal portfolio of risky assets] I said 'If there's only one, it's got to be the market portfolio. It's the only way you can get everything to add up.'" (Sharpe interview<sup>31</sup>)

"The conclusion is inescapable," Sharpe wrote in 1970. "Under the assumed conditions, the optimal combination of risky securities is that existing in the market. . . . It is the market portfolio." (p. 82) Along with the straight-line relationship between expected return and beta, the other essential component of the CAPM was now in place. In equilibrium, all the apparent complication of portfolio selection dissolved. The optimal set of risky investments was simply the market itself.

Although Sharpe did not know it at first, a model essentially the same as his Capital Asset Pricing Model was being developed at about the same time by an operations researcher named Jack Treynor. Very similar models were being developed by John Lintner at the Harvard Business School economist (Lintner 1965a,b) and by Jan Mossin at the Norwegian School of Economics and Business Administration (Mossin 1966).

With the market itself taken to be the unique optimal portfolio of risky assets (a conclusion on which Sharpe, Treynor, Linter, and Mossin concurred<sup>32</sup>), the Capital Asset Pricing Model's parameter, beta, had a simple interpretation: it was the sensitivity of returns on the asset to overall market fluctuations. That sensitivity was the risk that could not be diversified away, and the extent of that "market risk" or "systematic risk" determined the relative prices of risky capital assets such as stocks.

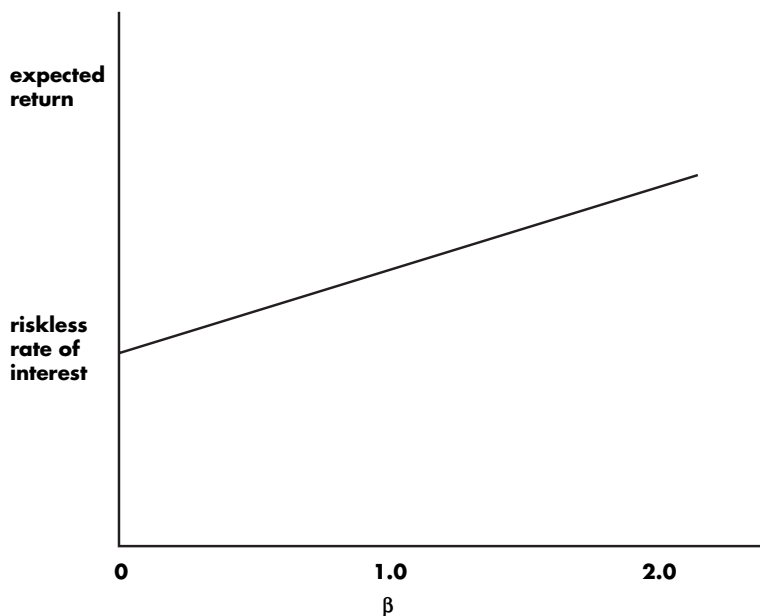
The reasoning that had led Sharpe, Treynor, Linter, and Mossin to the Capital Asset Pricing Model was sophisticated. However, the model predicted an equilibrium relationship between an asset's beta and its expected return that was simplicity itself. (See figure 2.2.) An asset with a beta of zero has no correlation to the market as a whole, so any specific risk in holding such an asset can be eliminated by diversification. Hence, the asset can be expected to yield only the riskless rate of interest: the rate an investor could earn by holding until its maturity an entirely safe asset such as a bond issued by a major government in its own currency. As beta and thus market risk rises, so does expected return, in a direct straight-line relationship.

The theory of investment had been transformed. If the reasoning of Sharpe and of the other developers of the Capital Asset Pricing Model was correct, beneath the apparently bewildering complexity of Wall Street and other capital markets lay a simple tradeoff between systematic risk and return that could be captured by a straightforward, parsimonious, and elegant mathematical model.

### ***Random Walks and Efficient Markets***

The third main strand in the transformation of the study of finance in the United States in the 1950s and the 1960s was the most general. It involved two closely related notions: that prices of stocks and similar securities follow a random walk, and that financial markets—at least the main markets in the United States and similar countries—are efficient.

The idea that the movements in the prices of financial securities are in some sense random—and therefore that the mathematical theory of probability can be applied to them—received its decisive development in the 1950s and the



**Figure 2.2**

The relationship between beta and expected return, according to the Capital Asset Pricing Model.

1960s, but did not originate then. As financial markets blossomed in the nineteenth century, a popular “science of investing” developed and was incorporated into manuals of investment aimed at a readership of middle-class men with worries that investing in securities was gambling (Preda 2001a, 2004a). In 1863, the French broker Jules Regnault published one such text. (See Jovanovic 2000; Jovanovic and le Gall 2001.) Regnault sought to demonstrate to his readers the distinction between gambling on securities and legitimate, patient longer-term investment. He applied a simple probabilistic model to show that those who made frequent bets on short-term movements in prices faced eventual inevitable ruin.

Regnault argued that short-term price movements were like coin-tossing: upward and downward movements will tend to have equal probabilities (of  $1/2$ ), and subsequent movements will be statistically independent of previous movements (Regnault 1863, pp. 34–38). That was a fair game, but even in a fair game a player with finite resources playing an opponent with unlimited resources will eventually lose the entirety of those resources, and if that happens in a financial market the game is over from the player’s viewpoint.

The market, “that invisible, mysterious adversary,” had effectively infinite resources. Thus, even if “chances were strictly equal” those who indulged in frequent short-term speculation faced “an absolute certainty of ruin.” Furthermore, brokers’ commissions and other transaction costs meant that in practice the game was less than fair.<sup>33</sup>

As a follower of the “social physics” of the pioneering statistician Adolphe Quetelet, Regnault sought regularities underlying the market’s apparent randomness. What was from his viewpoint the central regularity could have been derived mathematically from his model, but Regnault seems to have found it empirically (and he certainly tested it empirically).

The regularity was that the average extent of price deviations is directly proportional to the square root of the length of the time period in question (Regnault 1863, pp. 49–50). Modern random-walk theory leads to the same conclusion, and there are passages in Regnault’s work (especially Regnault 1863, pp. 22–24) that bring to mind the efficient-market hypothesis. However, Regnault’s comments on his square-root law remind us that he inhabited a different intellectual world. For him, the regularity was an ultimately theological warning to “earthly princes . . . kings of finance” to be humble in the face of what was, at root, Providential order.<sup>34</sup>

Regnault’s work has only recently been rediscovered. More celebrated as a precursor of modern random-walk theory has been Louis Bachelier, a student of the leading French mathematician and mathematical physicist Henri Poincaré (on whom see, for example, Galison 2003). While Regnault drew on existing and relatively elementary probability theory, Bachelier developed a model of a random or “stochastic” process in continuous time. In Bachelier’s model, the price of a security can change probabilistically in any time interval, however short. (The coin-tossing model is, in contrast, a stochastic process in discrete time: at least implicitly, the model is of a coin tossed only at specific moments, and not between those moments.)

In his Sorbonne thesis, defended in March 1900, Bachelier sought to “establish the law of probability of price changes consistent with the market” in French bonds.<sup>35</sup> He constructed an integral equation that a stochastic process in continuous time had to satisfy, and showed that the equation was satisfied by a process in which, in any time interval, the probability of a given change of price followed the normal or Gaussian distribution, the familiar “bell-shaped” curve of statistical theory.<sup>36</sup> Although Bachelier had not demonstrated that his stochastic process was the only solution of the integral equation (and we now know it is not), he claimed that “evidently the probability is governed by the Gaussian law, already famous in the calculus of probabilities” (Bachelier 1900, p. 37).



We would now call Bachelier's stochastic process a "Brownian motion," because the same process was later used by physicists as a model of the path followed by a minute particle suspended in a gas or liquid and subjected to random collisions with the gas or liquid's molecules. Bachelier, however, applied it not to physics but to finance, in particular to various problems in the theory of options.

In 1900, despite the currency of the popular "science of investing," the financial markets were an unusual topic for an aspirant academic mathematician. "Too much on finance!" was the private comment on Bachelier's thesis by the leading French probability theorist, Paul Lévy (quoted in Courtault et al. 2000, p. 346). Bachelier's contemporaries doubted his rigor, and his career in mathematics was modest: he was 57 before he achieved a full professorship, at Besançon rather than Paris.

Though knowledge of Bachelier's work never vanished entirely, even in the Anglo-Saxon world (Jovanovic 2003), there was undoubtedly a rupture. Successive shocks—the two world wars, the 1929 crash and the subsequent depression, the rise of communism and of fascism—swept away or marginalized many of the surprisingly sophisticated and at least partially globalized nineteenth-century financial markets studied by Regnault, Bachelier, and their contemporaries.

When the view that price changes are random was revived in Anglo-Saxon academia later in the twentieth century, it was at first largely in ignorance of what had gone before. One of the earliest of the twentieth-century Anglo-Saxon writers to formulate what became known as the random-walk thesis was the statistician and econometrician Holbrook Working of the Food Research Institute at Stanford University.<sup>37</sup> Working was not in fact a proponent of the thesis: he wrote that "few if any time series [observations of a variable, such as a price, at successive points in time] will be encountered that reflect in pure form the condition of strictly random changes" (Working 1934, p. 12). However, he thought it worth exemplifying what a random walk (he called it "a random-difference series") might actually look like.

Working took a table of "random sampling numbers" produced by an assistant of the biostatistician and eugenicist Karl Pearson (who in 1905 had coined the term "random walk")<sup>38</sup> and applied to it a transformation Pearson had suggested, making the frequencies of the random numbers correspond to a normal distribution.

Working constructed his random-difference series by starting with a number from the random-number table, adding to it the next number in the table to get the second number in the series, and so on. The differences between

successive terms in the series therefore simulated random “draws” from a normal distribution. Working suggested that the series that he constructed in this way could be compared with real examples of time series, such as the successive prices of stocks or of agricultural commodities such as wheat. (Wheat was of particular interest to the Food Research Institute.) Such comparison could, Working (1934) hoped, help to distinguish non-random structures in time series from the results of random processes.

Working clearly wanted to find non-random structure. That there might be no such structure in economically important time series does not seem to have been an attractive conclusion to him or to others in the 1930s and the 1940s. That, at least, is what is suggested by the reaction that met the statistician Maurice Kendall when he presented the random-walk thesis to a 1952 meeting of Britain’s Royal Statistical Society.

Kendall analyzed indices of stock prices in particular sectors in the United Kingdom, wheat prices from the Stanford Food Research Institute,<sup>39</sup> and cotton prices from the U.S. Department of Agriculture. Using one of the early digital computers (at the U.K. National Physical Laboratory), Kendall looked for “serial correlation” or “autocorrelation” in these time series, for example computing for each price series the correlation between each week’s price change and that of the previous week.

Almost without exception, Kendall found only very small levels of correlation. For instance, “such serial correlation as is present” in the stock index series “is so weak as to dispose at once of any possibility of being able to use them for prediction” (Kendall 1953, p. 18). That was the case also for wheat prices, where “the change in price from one week to the next is practically independent of the change from that week to the week after.”<sup>40</sup>

In the absence of correlation, Kendall saw evidence of the workings of what he called “the Demon of Chance”: “The series looks like a ‘wandering’ one, almost as if once a week the Demon of Chance drew a random number from a symmetrical population of fixed dispersion and added it to the current price to determine the next week’s price.” (Kendall 1953, p. 13) If it were indeed the case that “what looks like a purposive movement over a long period” in an economic time series was “merely a kind of economic Brownian motion,” then any “trends or cycles” apparently observed in such series would be “illusory” (*ibid.*, pp. 13, 18).

“That is a very depressing kind of conclusion to the economist,” commented the British economist and economic statistician R. G. D. Allen, the proposer of a distinctly lukewarm vote of thanks to Kendall after his talk. “This paper must be regarded as the first dividend on a notable enterprise,” Allen

continued. "Some 'shareholders' may feel disappointed that the dividend is not larger than it is, but we hope to hear more from Professor Kendall and to have further, and larger, declarations of dividends." (Allen 1953, p. 26)

S. J. Prais of the (U.K.) National Institute of Economic and Social Research was more explicit in his criticism. He argued that Kendall's variable-by-variable serial correlation tests "cannot in principle throw any light on the possibility of estimating the kind of dynamic economic relationships in which economists are usually interested" (Prais 1953, p. 29).

At MIT, Paul Samuelson learned from a participant in the meeting, the economist Hendrik Houthakker, that the "outsider" statistician Kendall was "invading the economists' turf" (Samuelson interview). Houthakker had not been impressed by Kendall's paper. "Can there be any doubt," he asked, "that the movements of share prices are connected with changes in dividends and the rate of interest? To ignore these determinants is no more sensible or rewarding than to investigate the statistical properties of the entries in a railway time-table without recognizing that they refer to the arrivals and departures of trains." (Houthakker 1953)

Samuelson suspected that his fellow economists' negative reaction to Kendall's invocation of the "Demon of Chance" was misplaced. He recalls saying to Houthakker "We should work the other side of the street" (Samuelson interview)—in other words, seek to develop Kendall's viewpoint. Perhaps price changes were random because any systematic patterns would be detected by speculators, exploited in their trading, and thus eliminated?

Later, Samuelson put this way: "If one could be sure that a price will rise, it would have already risen." (1965b, p. 41) If it is known that the price of a stock will go up tomorrow, it would already have gone up today, for who would sell it today without taking into account tomorrow's rise? Thus, Kendall's results could indicate that "speculation is doing its best because it leaves everybody with white noise"—in other words, with randomness (Samuelson interview).

Samuelson's explanation of Kendall's findings had in fact also struck at least one member of Kendall's audience: S. J. Prais, whose criticism I have already quoted. Said Prais:

... the markets investigated [by Kendall] ... are share and commodity markets [in which] any expected future changes in the demand or supply conditions are already taken into account by the price ruling in the market as a result of the activities of hedgers and speculators. There is, therefore, no reason to expect changes in prices this week to be correlated with changes next week; the only reason why prices ever change is in response to unexpected changes in the rest of the economy. (1953, p. 29)

However, the conclusions drawn by Prais and by Samuelson from their shared analysis of Kendall's findings differed starkly. For Prais, Kendall had

made a mistake in focusing on the stock and commodity markets. Prais put it this way: “. . . from the point of view of investigating the dynamic properties of the [economic] system it is therefore particularly unfortunate that Professor Kendall found it necessary to choose these markets for his investigation” (1953, p. 29).

For Samuelson, in contrast, Kendall’s findings suggested that it might be worthwhile devoting at least part of his professional attention to the financial markets. Another incident sparking Samuelson’s interest was the receipt of a “round-robin letter” sent to “a number of mathematical economists” by the University of Chicago statistical theorist L. J. Savage, asking whether any of them knew of Bachelier.<sup>41</sup> Samuelson did. He had heard Bachelier’s name previously from the mathematician Stanislaw Ulam (Samuelson 2000, p. 2). It was, however, Savage’s letter that prompted Samuelson to search out Bachelier’s thesis.

Samuelson was already interested in options, the central topic to which Bachelier applied his random-walk model, because he believed (wrongly, as he now acknowledges)<sup>42</sup> that study of the options market might yield “a set of empirical data that would give one insight on what the belief in the market was about the future” (Samuelson interview). Samuelson had a Ph.D. student, Richard J. Kruizenga, who was finishing his thesis on options, so he pointed Kruizenga to Bachelier’s work. Kruizenga had been employing as his asset price model a discrete random walk analogous to Regnault’s, rather than Bachelier’s continuous-time random walk, and must have been discomfited to discover “after most of my analysis had been completed” (Kruizenga 1956, p. 180) that someone else had analyzed the same topic, half a century before, in a mathematically more sophisticated way.

Bachelier’s model was an “arithmetic” Brownian motion: it had the symmetry of the Gaussian or normal distribution. If the price of a bond was, for example, 100 francs, the probability on Bachelier’s model of a one-franc upward movement was the same as that of a one-franc downward movement. Given the pervasiveness of the normal distribution, employing such a model was a natural step for someone with a background in mathematics or physics. It was, for example, how Norbert Wiener—the famous MIT mathematical physicist, theorist of prediction in the presence of “noise” (random fluctuations), and pioneer of cybernetics—had modeled Brownian motion. In a physical context, the normal distribution’s symmetry seemed entirely appropriate: “. . . positive and negative displacements of the same size will, for physical considerations, be equally likely” (Wiener 1923, p. 134).

The main practical context in which Wiener’s cybernetics developed was anti-aircraft fire control.<sup>43</sup> Samuelson worked on fire control in MIT’s

Radiation Laboratory in 1944–45. He did not enjoy the experience (“We worked . . . unproductively long hours because we were setting examples”), but he did “learn something about Wiener-like stochastic forecasting” (Samuelson interview).

However, Samuelson realized that he could not simply import Bachelier’s or Wiener’s arithmetic Brownian motion into finance, for it would imply a non-zero probability of a stock price becoming negative. “I knew immediately that couldn’t be right for finance because it didn’t respect limited liability.” (Samuelson interview<sup>44</sup>) A bankrupt corporation’s creditors could not get their money back by suing the owners of its stock, so ownership of a stock was never a liability.

Samuelson therefore turned the physicists’ arithmetic Brownian motion into a “geometric” Brownian motion—“a law of percentage effect that maybe stocks had the same probability of doubling as of halving” (Samuelson interview), even though doubling would be a larger dollar move than halving. Bachelier’s “normal” random walk thus became a “log-normal” random walk: what followed the normal distribution was changes in the *logarithms* of prices. A price following a log-normal random walk would never become negative; limited liability was respected.<sup>45</sup>

Samuelson did not publish his work on the random-walk model in finance until 1965, but he lectured on it in the late 1950s and the early 1960s at MIT, at Yale, at the Carnegie Institute of Technology, and elsewhere.<sup>46</sup> In a random-walk model, he pointed out, the average return on stocks probably had to be “sweeter” than the rate of interest that could be earned from “riskless securities,” for otherwise risk-averse investors could not be persuaded to hold stocks at all (Samuelson n.d., p. 11). However, after allowing for that “fair return,” stock-price changes had to be a “fair game” or what mathematicians call a “martingale,” because “if everyone could ‘know’ that a stock would rise in price, it would *already* be bid up in price to make that impossible.”<sup>47</sup> In consequence, “it is not easy to get rich in Las Vegas, at Churchill Downs, or at the local Merrill Lynch office.”<sup>48</sup>

Although Samuelson was by far the best-known economist to embrace the random-walk model, other analysts did so too. They included Harry V. Roberts, a statistician at the University of Chicago Graduate School of Business, and M. F. M. Osborne, an astrophysicist employed by the U.S. Naval Research Laboratory. Roberts knew of Working’s and Kendall’s papers, but Osborne reached the conclusion that there was “Brownian motion in the stock market” empirically, apparently without knowing of Bachelier, Working, or Kendall.<sup>49</sup> Like Samuelson, Osborne concluded that a log-normal random

walk was a better model of stock-market prices than the physicists' arithmetic Brownian motion.<sup>50</sup>

By 1964, there was a sufficient body of work on the random character of stock-market prices to fill a 500-page collection of readings; it was edited by Paul Cootner of MIT. The definitive statement of the view of financial markets that increasingly underlay this research came in a 1970 article in the *Journal of Finance* by Eugene Fama of the University of Chicago. That view was that financial markets were "efficient"—in other words, that "prices always 'fully reflect' available information" (Fama 1970, p. 383). Drawing on a distinction suggested by his colleague Harry Roberts, Fama distinguished three meanings of "available information," the first two of which correspond roughly to successive phases in the history of the relevant research.

The first meaning of "available information" is the record of previous prices. If the information contained in that record is fully reflected in today's prices, it will be impossible systematically to make excess profits by use of the record. A market in which it is impossible to do that—for example, because prices follow a random walk—is, in Fama's terminology, "weak form" efficient, and much of the work on the random-walk hypothesis in the 1950s and the 1960s was an attempt to formulate and to test this form of efficiency.

Different authors meant somewhat different things by "random walk," but the most common meaning was that "successive price changes are independent, identically distributed random variables. Most simply, this implies that the series of price changes has no memory, that is, the past cannot be used to predict the future in any meaningful way." (Fama 1965, p. 34) That property implied "weak form" efficiency. However, the latter was also implied by martingale ("fair game") models that were more general than the original conception of random walks. After these martingale formulations were introduced in the mid 1960s by Samuelson (and also by Benoit Mandelbrot) they tended to replace random walks as the preferred mathematical formulations of weak-form efficiency.<sup>51</sup>

The second meaning of "available information," and the subject of increasing research attention from the late 1960s on, was "other information that is obviously publicly available (e.g., announcements of annual earnings, stock splits, etc.);" (Fama 1970, p. 383). If excess profits could not be made using this class of information because prices adjusted to take it into account effectively instantaneously on its arrival, a market was "semi-strong form" efficient. By 1970, "event studies," such as Ball and Brown 1968 and Fama, Fisher, Jensen, and Roll 1969, were beginning to accumulate evidence that the U.S. stock market was efficient in this second sense too.

The third category of “available information” was that to which only certain groups, such as corporate insiders, were privy. If this kind of information was always reflected in prices, and insiders could not make excess profits on the basis of it, a market was “strong form” efficient. “We would not, of course, expect this [strong form] model to be an exact description of reality,” wrote Fama (1970, p. 409), and indeed there was already evidence against it.

Fama’s Ph.D. student Myron Scholes had examined the effects on prices of large sales of stock. While Scholes’s findings were broadly consistent with “semi-strong” efficiency, they also suggested that a corporate insider could have “higher expected trading profits than others because he has monopolistic access to some information.”<sup>52</sup>

The “specialists” on the floor of the New York Stock Exchange also enjoyed “monopolistic access” to information. (As was noted in chapter 1, a specialist matches and executes buy and sell orders and is expected to trade on his or her firm’s own account if there is an imbalance.) Specialists, Fama suggested (1970, pp. 409–410), could make excess profits because of their private access to the “book” of unfilled orders.<sup>53</sup>

Corporate insiders and New York’s “specialists” were, however, limited and probably exceptional cases. Their genuine advantages in respect to information were not shared by ordinary investors, nor, probably, by the vast bulk of investment professionals, such as fund managers. “There is,” Fama asserted, “no evidence that deviations from the strong form of the efficient markets model permeate down any further through the investment community.” (1970, pp. 415–416)

## ***Conclusion***

The laying out of the efficient-market hypothesis by Fama was the capstone of the transformation of the academic study of finance that had occurred in the United States in the 1950s and the 1960s. What had started as separate streams—the Modigliani-Miller “irrelevance” propositions, portfolio theory and the Capital Asset Pricing Model, the random-walk model—were by 1970 seen as parts of a largely coherent view of financial markets.

For instance, the hypothesis of market efficiency explained the counterintuitive claim that changes in the prices of stocks followed a random walk. The hypothesis posited that prices reflect all publicly available existing information, including anticipation of those future events that are predictable. (To take a hypothetical example, the prices in winter of the stocks of ice cream manufacturers will reflect the knowledge that demand for their product will rise in the summer.) Thus, only genuinely new information—for example, events that

could not have been anticipated—can move prices. By definition, however, that information was unpredictable and thus “random.”

The Modigliani-Miller propositions, the Capital Asset Pricing Model, and the efficient-market hypothesis were also interwoven in more detailed ways. For example, the CAPM’s co-developer, Jack Treynor, showed that the model implied the Modigliani-Miller proposition that capital structure was irrelevant to total market value (Treynor 1962). Similarly, the CAPM gave a systematic account of the relative amounts by which the prices of stocks had to be such that they offered expected returns “sweeter” than the rate of interest on riskless investments, so that rational risk-averse investors would include those stocks in their portfolios.

The efficient-market hypothesis (and at least the more sophisticated versions of the random-walk model) did not rule out stock returns that were on average positive, indeed “sweeter” than the riskless rate of interest. Efficient-market theorists insisted, however, that higher expected returns were always accompanied, as they were in the CAPM, by higher levels of risk.

Indeed, as was noted in chapter 1, the Capital Asset Pricing Model was incorporated into tests of the efficient-market hypothesis. The systematic “excess” returns that the hypothesis ruled out had to be “excess” relative to some benchmark, and typically the CAPM was used as the benchmark: an “excess” return on an investment was one systematically greater than the return implied by the model as appropriate to the investment’s level of market risk.

By the late 1960s, the descriptive, institutional study of finance had in the United States been eclipsed by the new, analytical, mathematical approaches discussed in this chapter. The financial markets had been captured for economics, so to speak. Other disciplines—Dewing’s invocations of history and psychology, “behavioral” studies of organizations in the tradition of Herbert Simon—were left on the margins. Modigliani, Miller, and the authors of the Capital Asset Pricing Model and the efficient-market hypothesis shared the view that “securities prices are determined by the interaction of self-interested rational agents” (LeRoy 1989, p. 1613). They were, likewise, all convinced that the processes involved had to be analyzed, not simply described, and that the necessary analytical tools were those of economics, not those of other disciplines.

It was a considerable intellectual transformation, but as we shall see in the next chapter it was also more than that. It had institutional foundations; it represented a change in focus, not just in style; it met fierce opposition; and in the 1970s it began to affect its object of study: the financial markets.





“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.<sup>1</sup> 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),<sup>2</sup> 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 realism 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.<sup>3</sup>

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.<sup>4</sup>

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).<sup>5</sup>

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 Associa-

tion 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.<sup>6</sup>

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.<sup>7</sup>

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’être* [*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.<sup>8</sup> 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



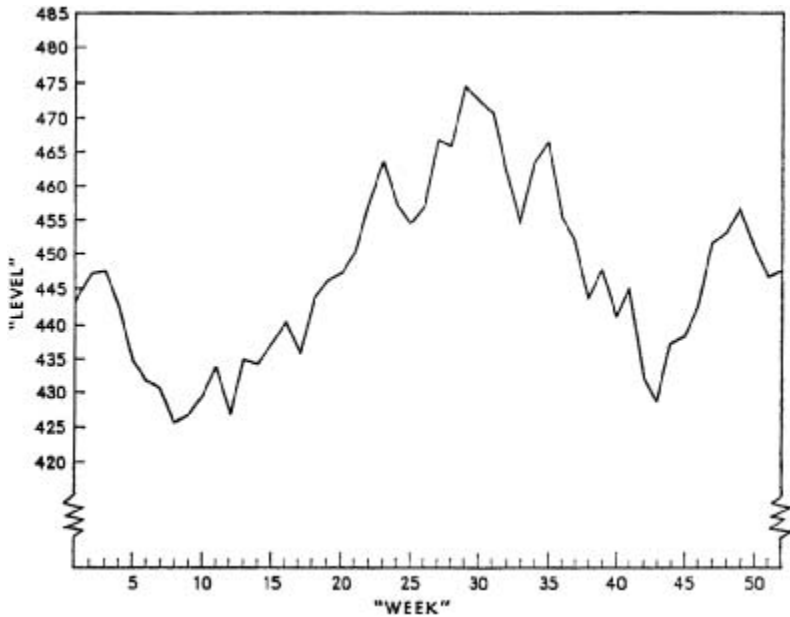
the *Wall Street Journal* (although Preda suggests that Dow contributed only general ideas, not specific techniques).<sup>9</sup>

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.<sup>10</sup>

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.”<sup>11</sup>

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.<sup>12</sup> 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.<sup>13</sup>

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 (Treyner 1981).<sup>14</sup> 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.<sup>15</sup>

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.<sup>16</sup> 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<sup>17</sup>) and Wilshire Associates (a consulting firm, based in Santa Monica, that developed out of John O'Brien's consultancy, O'Brien Associates<sup>18</sup>).

"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.<sup>19</sup>

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.<sup>20</sup>

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).<sup>21</sup> 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 Sinquefeld, “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.’” (Sinquefeld 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 indexing. 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.<sup>22</sup>

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.

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## *Tests, Anomalies, and Monsters*

Almost from the beginning, empirical difficulties shadowed the finance-theory ideas discussed in chapter 2. These difficulties were perhaps at their greatest with the Modigliani-Miller propositions. Merton Miller put it this way: “The M&M [Modigliani and Miller] model was much harder to implement empirically [than the Capital Asset Pricing Model]. . . . It’s very difficult to prove ‘empirically’ the M&M propositions holding even as approximations. You . . . can’t hold everything else constant.” (Miller interview) In other words, you can’t empirically identify in an unequivocal way the “risk classes” described in appendix A.

Fundamental theoretical contributions as they were, the Modigliani-Miller propositions remained controversial as claims about the world. For example, the topic of “dividend policy and capital structure” occupies more than 140 pages in one of the main modern textbooks on corporate finance (Brealey and Myers 2000, pp. 437–579). The Modigliani-Miller propositions provide those pages with their central organizing themes, but the text’s authors treat the empirical validity of those propositions as still an open question. Indeed, my impression is that a significant strand of opinion in financial economics does not regard the propositions as empirical claims, viewing them more as “benchmarks” against which deviations can be analyzed.<sup>1</sup>

Initially, matters seemed entirely different with respect to the Capital Asset Pricing Model and the efficient-market hypothesis. There was, Fama later recalled, “a brief euphoric period in the 1970s when market efficiency and the [capital asset pricing] model seemed to be a sufficient description of the behavior of security returns” (Fama 1991, p. 1590). However, there too difficulties soon began to accumulate.

Of the early attempts empirically to test the Capital Asset Pricing Model, the most supportive major study was by Fama and his Chicago colleague James D. MacBeth (1973), who used the data tapes of monthly stock returns from 1926 to 1968 constructed by the Center for Research in Security Prices

(CRSP). Even Fama and MacBeth's support for the model was qualified in some respects, and other studies found significant discrepancies. For example, Michael Jensen, Myron Scholes, and Fischer Black also used the CRSP tapes on the monthly returns on all stocks on the New York Stock Exchange, in their case for the years from 1931 to 1965. They constructed ten hypothetical portfolios with betas (calculated against the "market" in the sense of all New York-listed stocks) ranging from 0.5 to 1.5. They found that the low-beta portfolios had higher returns than the CAPM predicted, and the high-beta portfolios had lower returns.

Black, Jensen, and Scholes (1972, p. 82) concluded that their evidence was "sufficiently strong to warrant rejection of the traditional form" of the CAPM. As that formulation suggested, however, they believed that much could be salvaged. Although Black, Jensen, and Scholes accepted that there were other possible interpretations, Black himself provided a version of the CAPM that was compatible with their empirical result.

Sharpe had assumed that "all investors are able to borrow or lend funds on equal terms"—that is, at the riskless rate of interest (Sharpe 1964, p. 433). Risk-seeking investors could therefore increase their exposure to market risk (and thus their expected returns) by borrowing money and using it to increase their holdings of the market portfolio. Black experimented with the theoretical consequences of dropping the assumption of unrestricted borrowing. He found that he could explain the pattern of stock returns identified in his work with Jensen and Scholes by "an equilibrium in which borrowing at the riskless rate is either fully or partially restricted" (Black 1972, p. 454).<sup>2</sup>

Wells Fargo Bank supported Black, Jensen, and Scholes's research financially and sponsored the conference at which it was first presented, held at the University of Rochester (where Jensen then taught) in August 1969. Probably at Black's suggestion, McQuown's group at Wells Fargo saw a way to exploit the result of the research. If the anomalous finding was the result of restrictions on borrowing, perhaps it could be exploited by an investment company, which could borrow more easily and more cheaply than an individual could?<sup>3</sup> The idea was to invest in low-beta stocks, with what the study by Black, Jensen, and Scholes had suggested was their high return relative to risk, and to use "leverage" (in other words, borrowing) to increase the portfolio's level of risk to somewhat more than the risk of simply holding the overall market, so also magnifying returns (McQuown interview).

The way in which leverage magnifies risk and return can be seen in an example from housing. Many readers of this book will own a leveraged asset in the form of a home bought with a mortgage. Consider a couple who have bought a home for \$100,000, using \$10,000 of their own funds and borrow-

ing \$90,000. A rise in the home's value of 5 percent increases the value of their equity in it by 50 percent. Their home is now worth \$105,000; they still owe \$90,000; so the value of their equity is now \$15,000. A fall in the home's value of 5 percent reduces their equity by 50 percent. Wells Fargo's idea was not to seek dramatic fluctuations of that kind, but to use the research finding—that the market deviated from the CAPM's predictions—to construct an investment vehicle that would outperform the market by capturing the high relative return of low-beta stocks without being restricted by the low absolute returns those stocks offered.

Had the strategy of investing in leveraged low-beta portfolios been pursued on a large scale it might have had the performative effect of increasing the attractiveness of low-beta stocks, thus raising their prices, reducing the returns they provided, and minimizing the discrepancy between the model and the market. The idea had indeed been the initial focus of the discussion between Samsonite and Wells Fargo, but was dropped in favor of the index fund described in chapter 3. William Fouse of Wells Fargo was a skeptic; he suspected that the Black-Jensen-Scholes result might indicate a deeper problem with the CAPM, not a profit opportunity.<sup>4</sup>

Wells Fargo also considered offering a leveraged low-beta portfolio to the public in the form of a mutual fund. Unfortunately from the viewpoint of performativity, this version of the idea hit a legal barrier, the Glass-Steagall Banking Act of 1933.<sup>5</sup> The act laid down a strict separation between commercial banks (such as Wells Fargo), which took deposits from the public, and investment banks, which underwrote and sold securities. In 1971, the U.S. Supreme Court interpreted the act as implying that commercial banks could not sell mutual funds. For this and other reasons—there was doubt whether the deviation from the model that the fund would exploit was real or a statistical artefact—Wells Fargo had to drop the idea (McQuown interview).<sup>6</sup> Fischer Black continued to believe that the pattern of returns he, Jensen, and Scholes had found was both real and a profit opportunity (Black 1993), but no equivalent of Wells Fargo seems to have stepped forward to exploit the idea.

Empirical evidence that was interpreted as casting the Capital Asset Pricing Model into doubt accumulated in the late 1970s and the 1980s. Fama, in particular, came radically to revise his initially positive assessment. In 1992, he and his Chicago colleague Kenneth R. French put forward the most influential empirical critique. They reported that they could find the CAPM's predicted linear relationship between beta and average return only in the period 1941–1965, years that featured heavily in the early tests of the model. After 1965, there was little relationship between beta and average return, and even in the period 1941–1965 “the relation between [beta] and average return



disappears when we control for [firm] size” (Fama and French 1992, p. 440).

Black had sought to modify the Capital Asset Pricing Model while preserving its essential structure; Fama and French in effect suggested discarding it. Given the prominence in investment practice of the CAPM and of beta, their critique received considerable publicity. “Beta beaten” was how *The Economist* headlined its account (Anonymous 1992). “The fact is,” Fama told a reporter for the *New York Times*, “beta as the sole variable explaining returns on stocks is dead.” (Berg 1992, p. D1)

The epitaph endured: Fama and French’s article became known colloquially to financial economists as “the ‘beta is dead’ paper” (Cohen 2002, p. 3). The CAPM is “atrocious as an empirical model,” says Fama (interview). The early “euphoric period” was misguided. “We should have known better. The [CAPM] is just a model and so surely false.” (Fama 1991, p. 1590)

In 1977, Richard Roll, who had been Eugene Fama’s Ph.D. student at the University of Chicago, went beyond specific empirical criticisms to cast into doubt whether it was possible to test the Capital Asset Pricing Model at all. Roll had begun by sharing the “euphoria” of the early 1970s about the model: “I was pretty much a believer in this being a panacea for everything, for asset pricing.” (Roll interview) However, while Roll was doing theoretical work on asset pricing in the mid 1970s, his doubts began to grow.

At the core of the Capital Asset Pricing Model was the notion of a “market portfolio.” Not only was that the optimal investment portfolio: the beta values needed to test the model were determined by the correlations between the returns on the assets in question and the returns on the market portfolio. But what *was* the market portfolio? Proxies for it such as stock indices were easy to use in econometric tests but almost certainly quite inadequate: the “true market portfolio” was the entire universe of risky capital assets. It thus had to include corporate bonds as well as stocks; land, houses, and other buildings; automobiles and consumer durables; even the training and skills that created “human capital.” Because it encompassed so much, “the true market portfolio’s exact composition” (Roll 1977, p. 158) was quite unknown.

“There is one piece of empirical content” to the CAPM, says Roll, “which is that the true market portfolio should be on the efficient frontier [see chapter 2], if Sharpe’s ideas are right, and maybe it is, but we’ll never know that. . . . We’ll never be able to measure it [the market portfolio] sufficiently when you think about all the stuff that’s missing. I mean certainly it’s not the American S&P 500. I mean that would be foolish to think *that’s* a proxy for the world-market portfolio, which includes human capital and real estate.” (Roll interview)

Roll’s methodological critique was interpreted by some, just as Fama and French’s later empirical criticism was to be, as fatal blow to the Capital Asset

Pricing Model. In July 1980, the magazine *Institutional Investor* reported receiving a letter signed only “Deep Quant.” “Quants” was what Wall Street was beginning to call specialists in mathematical finance, and the writer obviously wished to invoke Deep Throat, the anonymous insider who guided the journalists investigating President Nixon’s Watergate scandal. The letter directed the magazine to Roll’s critique, and told it that “the capital asset pricing model is dead.” “It took nearly a decade for money managers to learn to love beta,” the magazine itself commented. “Now it looks as if they were sold a bill of goods—and the whole MPT [modern portfolio theory] house of cards could come tumbling down.” (Wallace 1980, p. 23)

Robert F. Stambaugh, a Ph.D. student at the University of Chicago, countered Roll’s critique by testing the consequences of including assets such as “corporate bonds, government bonds . . . housefurnishing, automobiles, and real estate” in measures of the “returns on portfolios of aggregate wealth” (Stambaugh 1982, pp. 244 and 237). The performance of the Capital Asset Pricing Model was affected, Stambaugh concluded, by the specifics of how it was tested, and some of Stambaugh’s tests implied that it should be rejected.

However, Stambaugh reported that inferences about the model were not altered substantially by different choices of measures of “aggregate wealth.” Roll’s critique was thus less damning than it seemed. Sensitivity of the CAPM to the particular way in which the performance of the “market portfolio” was measured could indeed make the model “less testable than other models,” said Stambaugh, “but no such sensitivity is found in this study” (1982, p. 266).

But William Sharpe himself conceded that Roll’s critique of the tests of his model was essentially correct. Sharpe told *Institutional Investor*’s reporter: “We cannot, without a shadow of doubt, establish the validity of the capital asset pricing model.” (Wallace 1980, p. 24) Stambaugh’s study meant that the Capital Asset Pricing Model is “maybe OK after all,” says Sharpe (interview). Nevertheless, in his view that kind of empirical defense against criticism of the model misses two deeper points. First, “nobody can confirm a hypothesis” such as the CAPM. “You can only fail to disconfirm it.” (Sharpe interview) Second, statistical difficulties (such as the large sampling errors in estimating the average return on a stock), economic and historical change, and variation across countries render empirical findings in this area unstable.

“It’s almost true,” says Sharpe, “that if you don’t like an empirical result, if you can wait until somebody uses a different [time] period, or a different country, or a different analytical method, you’ll get a different answer.” In consequence, “we’ll never be able to say definitively the CAPM holds. We’ll probably never be able to say definitively the CAPM doesn’t hold as well” as its later “multi-factor” rivals, such as the arbitrage pricing theory of option

theorist Stephen Ross or Fama and French's model, discussed below (Sharpe interview). The difficulties of empirical testing in this area are such, Sharpe suggests, that it is naive to expect market data unequivocally to discriminate among models.

The difficulties in testing the Capital Asset Pricing Model and the negative results of much such testing did not, however, stop it gaining a permanent place in financial economics, and playing, for instance, the central role in the development of option-pricing theory described in chapter 5. Fama, for example, recognizes its lasting impact: "It's . . . a powerful intellectual place to start. . . . It builds a lot of relevant intuition about asset pricing. It doesn't go away when you move on to other models." (Fama interview)

The Capital Asset Pricing Model's central intuition—that an asset's risk, when viewed as a small part of a widely diversified portfolio, is its sensitivity to overall market fluctuations, not its specific, idiosyncratic risks—diffused among practitioners as well as among theorists. Furthermore, the two apparently fatal blows against the model—Fama and French's empirical critique, and Roll's claim that the model cannot be tested empirically because the "market portfolio" cannot be identified unequivocally—were at odds.

Not only was Fama and French's interpretation of their results challenged ("Announcements of the 'death' of beta seem premature," wrote Fischer Black),<sup>7</sup> but Roll's critique undermined the tests with negative results as much as it did those with positive ones. As a leading textbook of financial econometrics points out, perhaps the tests on which the model seems to have failed involve misleading proxies for the market portfolio (Campbell, Lo, and MacKinlay 1997, p. 217).

### ***Efficiency and Anomalies***

In the background lay an issue beyond the question of the empirical accuracy of the Capital Asset Pricing Model: the validity of the efficient-market hypothesis. As was noted in chapters 1 and 2, the hypothesis and the model were interwoven. For example, tests of market efficiency typically took the form of investigating whether investment strategies were available that offered excess risk-adjusted returns, and the CAPM was often invoked in the tests to assess those returns.

Even more than the CAPM, the efficient-market hypothesis was the centerpiece of how the new financial economics conceived of markets. Those involved knew that it too was a model, but belief in its empirical adequacy was more widespread and deeper. "For the purposes of most investors," wrote Fama, "the efficient markets model seems a good first (and second) approxi-

mation to reality.” (Fama 1970, p. 416) “I believe there is no other proposition in economics which has more solid empirical evidence supporting it than the Efficient Market Hypothesis,” wrote Michael Jensen (1978, p. 95).

From the viewpoint of many market practitioners, however, the efficient-market hypothesis threatened their professional identities (because, as noted in chapter 3, it suggested that much of what they did was pointless) and seemed deeply counterintuitive. The arbitrageur David Weinberger, a mathematics Ph.D. who has been involved in financial markets since the mid 1970s, says: “You can’t be an analytical person who’s involved in the marketplace day after day without believing that there are little pockets of structure.” (Weinberger interview) These pockets are economically exploitable departures from randomness. “Anybody analytical [who has] spent meaningful time in the markets knows, in their gut, there’s structure there,” says Weinberger. Indeed, to him the intensity of belief on both sides makes the validity of the efficient-market hypothesis a profound divide: “Either you believe there’s structure or you don’t. . . . It’s like a religious question.”

There is a sense in which the efficient-market hypothesis and its predecessor, the random-walk hypothesis, always co-existed with contrary evidence. Holbrook Working, for example, believed it was possible to find at least some structure in the apparently random fluctuations of stock prices (Working 1958). So did another early formulator of the random-walk hypothesis: Alfred Cowles III, founder (and funder) of the Cowles Commission.

Cowles and Herbert Jones (1937, p. 280) asked “Is stock price action random in nature?” Using what was in essence the same coin-tossing model as Regnault had used, they answered the question in the negative. The excess of “sequences” (in which “a rise follows a rise, or a decline a decline”) over “reversals” (in which “a decline follows a rise, or a rise a decline”) was too great (*ibid.*, p. 281). They warned, however, that forecasting based on this apparent effect “could not be employed by speculators with any assurance of consistent or large profits” (*ibid.*, p. 294).

As was noted in chapter 2, the formulator of the efficient-market hypothesis, Eugene Fama, himself noted cases in which it did not apply, at least not in its strongest form. Some limited categories of market participant, such as the “specialists” in the New York Stock Exchange, could profit from information to which they had exclusive access. That sort of case was what Fama was excluding when he wrote in 1970 (as quoted above) that the efficient-market hypothesis was empirically valid “for the purposes of most investors.”

However, research by financial economists in the 1970s and the 1980s pointed to a wider range of phenomena that potentially were inconsistent with the efficient-market hypothesis. By 1978, there were enough of these

phenomena to merit a special issue of the new *Journal of Financial Economics* devoted to them. Michael Jensen's statement (quoted above) about the unique strength of the evidence for the hypothesis comes from his editorial introducing the special issue (Jensen 1978).

If Milton Friedman implicitly cited Karl Popper's philosophy of science, Jensen explicitly cited Thomas Kuhn's 1970 book *The Structure of Scientific Revolutions*. Research based on a powerful and successful new paradigm was beginning to throw up what Jensen called "anomalous evidence," just as Kuhn's account suggested: "... we seem to be entering a stage where widely scattered and as yet incohesive evidence is arising which seems to be inconsistent with the theory" (Jensen 1978, p. 95). However, as Jensen's remark on the strength of the evidence for the efficient-market hypothesis shows, he was in no sense an opponent of the hypothesis. Indeed, it is striking that many of the anomalies in regard to the latter were identified by those firmly in the efficient-market camp.

Discussion of the full range of the "anomalies" (Jensen's identification of them as such entered the parlance of financial economics) would take us beyond this book's remit,<sup>8</sup> so I have to be selective. Let me begin with what has become known as the "small-firm" or "size" effect. In the mid 1970s, a University of Chicago Ph.D. student, Rolf W. Banz, found that the returns on the stock of firms with small market capitalizations tended on average to be much higher than returns on the stock of larger firms (Banz 1978, 1981).<sup>9</sup>

Banz divided firms in the CRSP data tapes for 1926 to 1975 into five quintiles by the total market value of their stocks. On average, the monthly returns on the smallest 20 percent of firms—the bottom quintile—exceeded the returns on the remainder by around 40 basis points (0.4 percentage points). Cumulate an average monthly difference of that magnitude over years and decades, and the returns on holdings of small stocks were substantially greater than on holdings of larger stocks. Banz found that the difference was not due to different levels of market risk as measured by beta values. "There is no theoretical foundation for such an effect," he commented (Banz 1981, p. 16): it was indeed an anomaly.

Banz's "small-firm" effect initially "did not do down well at Chicago," where Banz's conclusions were "subjected to the most detailed scrutiny" (Jahnke and Skelton 1990, p. 66). However, the effect survived examination of Banz's evidence. Further analysis quickly suggested that a large part of the excess returns on small stocks was concentrated in just two weeks of each year—the first two weeks of January (Keim 1983; Reinganum 1983)—and this feature led this aspect of the anomaly to be called the "turn-of-the-year" effect. Again, it was theoretically puzzling. If changes in stocks' prices were driven by the arrival

of new information, why should the beginning of January almost always see good news with respect to small stocks?

A plausible account of the turn-of-the-year effect is that holders of stocks that have fallen in price tend to sell them in December, before their tax year ends, in order to realize a tax loss that can be set against gains in other parts of their portfolios. The stocks then “jump back up” in January. The effect might be particularly strong for small stocks because of their typically high volatilities (Roll 1983, p. 20).

But although an explanation in terms of tax incentives might satisfy market practitioners, who knew of the turn-of-the-year effect’s existence, though apparently not of its concentration in small stocks, before academics identified it (Branch 1977), it was “ridiculous” from the viewpoint of efficient-market theory (Roll 1983, p. 20). Surely any effects of temporary sales pressures and purchase pressures would be minimized by arbitrageurs exploiting those effects. If the market was efficient, surely other investors not subject to turn-of-the-year tax incentives would make use of the effect to earn excess returns.

Anomalies such as the small-firm and turn-of-the-year effects could, therefore, be construed as evidence against the efficient-market hypothesis. In Kuhn’s analysis of the natural sciences, “a background of firm expectation makes anomalies and exceptions stand out and take on significance” (Barnes 1982, p. 20), and the accumulation of such anomalies was seen by Kuhn as the precursor of a scientific revolution. Jensen indeed suggested that the growing list of anomalies pointed in Kuhnian fashion to “a coming mini-revolution in the field” (Jensen 1978, p. 95).

As Barry Barnes points out, however, anomalies do not speak for themselves. “What one scientist sees as an anomaly,” necessitating a revolutionary new paradigm, “another sees as a puzzle” to be solved without radical conceptual change (Barnes 1982, p. 100). What Jensen envisaged was indeed no more than a “mini-revolution”: one likely to involve “more precise and more general theories of market efficiency and equilibrium models of the determination of asset prices under uncertainty,” but not “abandonment of the ‘efficiency’ concept” (Jensen 1978, pp. 96 and 100).

Others suggested a more radical conclusion. In the 1980s and the 1990s, a “behavioral” approach to finance (to be discussed in chapter 9) drew on work by psychologists on systematic biases in human reasoning to cast market efficiency into doubt.<sup>10</sup> For the advocates of behavioral finance, the growing list of anomalies was evidence against the central tenets of “orthodox” finance theory (see, for example, Shleifer 2000).

Because of the interweaving of the efficient-market hypothesis and the Capital Asset Pricing Model, anomalies such as the small-firm effect could be

interpreted in more than one way. Perhaps they were indeed market inefficiencies pointing to the need for a “behavioral” approach to finance. However, it was also possible that they were symptoms of a more limited problem: deficiencies in the CAPM.

The dominant response of Eugene Fama and his University of Chicago students to the growing list of anomalies was to suggest that the fault lay in the Capital Asset Pricing Model, not in the efficient-market hypothesis. Banz, for example, seemed to toy with the “market inefficiency” interpretation, suggesting that buying small stocks while simultaneously taking a short position in the largest stocks was potentially an opportunity for what he called “arbitrage.” However, in the conclusion of the paper presenting his main findings he preferred the more modest interpretation: “the CAPM is misspecified” (Banz 1981, pp. 14–15).<sup>11</sup>

Fama and French did more than make the empirical criticisms of the Capital Asset Pricing Model noted above: they built work such as Banz’s into a full-fledged alternative to the model. In the Fama-French model, beta plays no role. In its place are two different factors: firm size (the total market value of a corporation’s stocks) and “book-to-market” ratio (the ratio of a corporation’s net assets as recorded in its balance sheet to its market value).

Fama and French argued that firm size and book-to-market ratio “provide a simple and powerful characterization of the cross-section of average stock returns for the 1963–1990 period” (1992, p. 429). The phenomena that appear to be anomalies when the CAPM is applied “largely disappear”—in other words are explained as rational asset pricing—when their model is applied, argued Fama and French (1996, p. 55).

### ***Anomalies and Practical Action***

Finance’s “anomalies” were thus open to contestation and to multiple interpretations, just as in the case of their analogues in the natural sciences. However, the anomalies in finance also became subject to a process with no full equivalent in Kuhn’s discussion of their counterparts in the sciences. In many cases, the identification of finance’s anomalies was the precursor to their attenuation or disappearance. That was so not just in the sense that explanations of them consistent with efficient-market theory were put forward, but because the anomalies were exploited in practice, often by the theory’s adherents, and this exploitation seems to have reduced their size.

As would be expected in the light of the discussion in chapter 1 of the difficulties of econometric testing, the evidence on the persistence of anomalies is not entirely clear-cut.<sup>12</sup> Failure to find the same anomaly in a later data set

might indicate simply that its original identification was mistaken: for example, that it was the result of “data snooping,” the “discovery” of spurious pattern as the same finite data sets were “dredged” repeatedly for anomalies (Fama and French 1996, p. 80).

Nevertheless, there is some consensus that the small-firm and turn-of-the-year effects *were* real, but that the small-firm effect has largely disappeared since its identification (Schwert 2002, pp. 6–7; Jegadeesh and Titman 2001, p. 700), or indeed has possibly even reversed (Gompers and Metrick 2001). The turn-of-the-year effect seems also to have diminished, roughly halving in size since its identification (Schwert 2002, p. 9).

Consider first the turn-of-the-year effect. Paul Samuelson suggests that its diminution was due to the fact that “pretty soon [after the effect’s identification] the assistant professors of finance were beginning to operate on this,” in other words to exploit it (Samuelson interview). The first of the index futures that began to be traded in the 1980s was the Kansas City Board of Trade’s futures on the Value Line index, which were launched in 1982. The Value Line index includes small firms, and each firm is given equal weight in the index’s composition (rather than weighting firms by their total market value), so the performance of small firms has a substantial effect on the level of the index. Every December, the “assistant professors of finance” would “buy the Kansas City futures” and “sell the S&P futures” (in the construction of the S&P 500 index firms are weighted by market capitalization, so the index reflects primarily the behavior of very large stocks). The “professors” would thus benefit from the turn-of-the-year anomaly, while being insulated from overall market fluctuations. Their activities, in Samuelson’s opinion, contributed to the diminution of the anomaly (Samuelson interview).<sup>13</sup>

Matters are more complex in respect to the small-firm effect. As was noted in chapter 3, a former University of Chicago MBA student, Rex Sinquefeld, led the index-fund effort at the American National Bank in Chicago. In 1976 or 1977, Sinquefeld learned of Banz’s as yet unpublished Ph.D. work identifying the small-firm effect. He phoned Banz at the University of Chicago and asked “Could you come downtown for lunch?” The “preliminary numbers” that Banz showed Sinquefeld and his colleagues persuaded them to extend American National’s index funds to encompass stocks outside the S&P 500 (Sinquefeld interview), and Banz’s work informed the original central strategy of Dimensional Fund Advisors, an investment firm set up in Santa Monica in 1981 by Sinquefeld and David Booth (also a Chicago MBA).

Dimensional’s first product was “designed literally on Banz’s [1981] paper,” Sinquefeld said in an interview. “It was the fifth quintile he talked about.” In 1982, Dimensional launched what was almost an index fund covering the



smallest 20 percent of the stocks traded on the New York Stock Exchange. (When Banz did his study, the CRSP data tapes included only stocks traded in New York, so Banz's analysis was restricted to them.) Sinquefield rejected Banz's proposed "arbitrage," which involved holding small stocks and taking short positions on large stocks, because of its high volatility. Pointing to the walls of his office, he explained it as follows: "You look at the standard deviation of that time series and it goes from that wall to that wall. In arbitrage, it's got to have a small standard deviation. . . . We just considered a plain vanilla fully invested long-only strategy"—in other words, one that did not involve short-selling. "Frankly, that's all that could have been sold to the pension community anyway." (Sinquefield interview) After establishing its quasi-index fund for small-capitalization stocks in the United States, Dimensional set up similar funds in 1986 for Japan and the United Kingdom (Rolf Banz himself headed Dimensional's London office), in 1988 for Continental Europe, and in 1989 for the Pacific Rim (Cohen 2002; Jahnke and Skelton 1990).

Sinquefield did not believe that the small-firm effect that had inspired Dimensional Fund Advisors was a market inefficiency. "I'm sort of the Aya-tollah of efficient markets," he jokes, "pretty hard line." (Sinquefield interview) If investing in small-capitalization stocks brought enhanced returns, it must be because it also brought greater risk, though exactly what that risk consisted in remained unclear, even when elaborated into the Fama-French model discussed above. "There's no free lunch here." (Sinquefield interview) Instead of claiming to be exploiting an inefficiency, Dimensional emphasized to prospective institutional investors the virtues of small-stock funds as a way of enhancing the diversification of their portfolios. (See, for example, Sinquefield 1996.)

Dimensional's emphasis on diversification, rather than on exploitation of a putative inefficiency, means that the firm has not been damaged by what appears to be the attenuation, noted above, of the small-firm effect. Dimensional expanded internationally and developed new strategies, some employing Fama and French's asset-pricing model, and the firm became a significant investment presence. By June 2002, it had assets of nearly \$36 billion under management (Cohen 2002, p. 12).

Dimensional Fund Advisors remained closely tied to academia. Its board in 2005 included Fama, French, Myron Scholes, and Robert C. Merton.<sup>14</sup> It provided funding for academic work in financial economics—notably at the University of Chicago's Graduate School of Business, whose research had got the firm started. In 1984, for example, the firm contributed \$180,000 to help enhance the main data source underpinning this research by extending the CRSP tapes to include stocks traded on NASDAQ, the National Association of Securities Dealers Automated Quotation system.<sup>15</sup> Dimensional "encour-

aged academics to work on subjects of interest to the firm by giving any professor a share of profits from investment strategies derived from his or her ideas" (Cohen 2002, p. 1).

There is, however, more to Dimensional Fund Advisors than the exploitation of efficient-market research. Especially in its most distinctive domain—small-capitalization stocks—Dimensional's traders also have to be economic sociologists, so to speak: that is, for example, why its initial products were not quite small-stock index funds. Small stocks are notoriously illiquid, which is one possible reason their prices were low enough to offer such attractive average returns. A buyer such as an index-tracking fund who *had* to buy such stocks faced having to pay what could be a considerable premium. A seller who had to sell (for example, because a firm's market capitalization had grown to such an extent that it was no longer small enough to fall within the fund's remit) might likewise have to offer a substantial discount.

In consequence, Dimensional avoided an exact index-tracking strategy that would have left it little or no discretion over the content and timing of sales and purchases. Instead of being a customer, paying in order to trade, it successfully positioned itself as in effect a market maker, making money out of trading (Keim 1999; Cohen 2002). It encouraged other participants in the market for small stocks to offer it blocks of stock that they wanted to sell, so that it was able to buy at a discount rather than paying a premium.

Encouraging others to sell to Dimensional had obvious potential disadvantages: a seller might have negative information about a stock's prospects that Dimensional did not have, or a seller might go on immediately to sell further blocks of the same stock, depressing the price. To reduce the risk of being exploited by those with whom it traded, Dimensional cultivated relationships of trust. "Preferred sellers were firms (and individuals within those firms) that consistently made full disclosure to [Dimensional] of everything they knew about the stock," including plans they had for further sales (Cohen 2002, p. 7).

Dimensional's traders operated a "penalty box" in the form of "a large board visible from any spot on [Dimensional's] trading floor," into which were placed the names of those who Dimensional's traders reckoned had shown themselves not to be trustworthy. "Depending on the severity of the infraction, a broker-dealer could stay in the penalty box for months or even longer." (Cohen 2002, p. 7) Simultaneously, the firm was careful itself to act in a trustworthy way—for example, by not exploiting information about planned sales by selling ahead of them.

As an increasingly significant presence in the market for small stocks, Dimensional affected that market by adding to its liquidity. If the circumstances

were right and the counterparty was trustworthy, Dimensional would buy blocks of stock that were in relative terms very large, often becoming the owner of 5 percent or more of the firms involved (Cohen 2002, p. 7). Because Dimensional was prepared to handle these large blocks, and because it had a reputation for not making unfair use of private information, the loss of the capacity to trade with it was a serious matter. In consequence, its “penalty box” became a substantial deterrent.

### ***Eliminating Anomalies?***

Dimensional Fund Advisors and Samuelson’s “assistant professors” who exploited the turn-of-the-year effect are not unique cases. In the 1980s and the 1990s, a significant number of specially established firms or trading groups within investment banks set out to build investment strategies around anomalies identified in the literature of financial economics and/or to find anomalies through their own research.

“Statistical arbitrageurs,” as they are often called, vary in how they conceive of the phenomena on which they base their activities. Some statistical arbitrageurs see anomalies as market inefficiencies; other firms see them, as Dimensional does, as proxies for poorly understood risk factors. Nevertheless, it seems as if the aggregate effect of their activities is often a tendency to reduce the size of, or even to eliminate, the anomalies they are exploiting, at least in those cases in which the anomalies are widely known to statistical arbitrageurs.

Consider, for example, the experience of Roll and Ross Asset Management, set up by Richard Roll (whose critique of the Capital Asset Pricing Model was discussed above) and the option and asset-pricing theorist Stephen Ross. The investment strategies pursued by Roll and Ross included the goal of earning enhanced returns by exploiting market anomalies (Ross interview). Richard Roll describes the experience:

... how you beat the benchmark is you have to look for these anomalies ... and that’s ephemeral. There was a period where price:earnings ratios did that. There was a period where size [the small-firm effect] did that. But those kind of things don’t last because people ... take advantage of it and so it goes away. ... The trick is to keep up on the academic literature that’s always discovering new anomalies and be the first person to actually try to take advantage of them. (Roll interview)

Since the start of the 1980s, there has thus been a mechanism whereby the identification of anomalies prompts practical action, often by efficient-market theorists or those close to them, which exploits and therefore tends to eliminate those anomalies. Indeed, if that cannot be done—if there seems to be no way of exploiting an apparent anomaly—there is a sense in which it is not an

anomaly, for what the efficient-market hypothesis rules out is not non-random “structure” of any kind, but structure from which excess risk-adjusted profits can be made.

However, it should not be concluded that we have discovered an entirely closed, self-reinforcing, reflexive circle whereby efficient-market theory first generates and then destroys the evidence against it. Some anomalies have persisted. One example is the “closed-end fund”<sup>16</sup> anomaly, which has been known to financial economics at least since the 1970s (see, for example, Thompson 1978), but has not disappeared.

The anomaly of closed-end funds is that in most cases the total market value of the stock of a closed-end investment fund is less than the total market value of the securities the fund owns. The discrepancy is large—often 10–20 percent (Shleifer 2000, p. 53)—but it is not easy to exploit. For example, a fund’s managers are likely to resist fiercely any attempt to force them to liquidate its assets and distribute the proceeds to shareholders, because that would eliminate their jobs (Gemmill and Thomas 2002).

Another seemingly persistent anomaly is the “momentum effect.” In apparent contradiction to even weak-form market efficiency, it seems as if significant profits can be earned from investment strategies that buy “winners” (“stocks with high returns over the previous 3 to 12 months”) and sell “losers” (“stocks with poor returns over the same time period”). Such strategies “earn profits of about one percent per month for the following year” (Jegadeesh and Titman 2001, p. 699).

Market practitioners had long suspected the existence of a momentum effect. The paper that seems to have convinced even “orthodox” financial economists that such an effect was real was “Returns to Buying Winners and Selling Losers” (Jegadeesh and Titman 1993). One reason the effect is of interest is that among possible explanations is the “behavioral finance” conjecture that market participants “underreact” to new information, so the market “responds only gradually to new information” (Chan, Jegadeesh, and Lakonishok 1996, p. 1681).

The “momentum effect” is exploited widely in trading—it is, for example, a familiar hedge-fund strategy—but that does not seem to have eliminated it (Jegadeesh and Titman 2001). Roll and Ross Asset Management, for example, found that the effect has “been a pretty long-term thing” (Roll interview). In the case of other anomalies, exploitation seems to tend to diminish their size, but that may not be so with the momentum effect, which conceivably may be intensified by the trading that exploits it. For example, buying a “winner” to exploit its momentum may have the effect of making it more of a winner, encouraging other “momentum traders” likewise to buy.<sup>17</sup>

The testimony of arbitrageurs (in, for example, my interview with David Shaw) is that in addition to the anomalies discussed in the literature of financial economics that are exploitable market inefficiencies that are not publicly known. Naturally, arbitrageurs are reluctant to say exactly what they are: others' exploitation of them would very likely reduce or eliminate them. Even in 1965 it was clear to James Lorie, founder of the Center for Research in Security Prices, that such anomalies might exist: "... there is a haunting fear that those with the best arguments [against the random-walk hypothesis] are silently sunning and swimming at St. Tropez" (Lorie 1965, p. 18).

Furthermore, one practical application of efficient-market theory, index funds, seems to have *created* an anomaly, rather than identifying a pre-existing anomaly. The composition of the S&P 500 and similar indices is not fixed. Periodically, firms are added to and deleted from them, because of mergers and bankruptcies and to reflect changes in the market values of corporations and in the overall makeup of the stock market. From an efficient-market viewpoint, the inclusion of a corporation in an index or its deletion from it should not affect the price of its stock: it seems to convey no information about the corporation that was not previously known.<sup>18</sup>

However, Andrei Shleifer, who was to become a leader of behavioral finance, found that from 1976 to 1983 the inclusion of a stock in the S&P 500 index was associated with an average rise in its price on the day of the announcement of its inclusion of just under 3 percent (Shleifer 1986, p. 582).<sup>19</sup> The effect survived its identification: between 1976 and 1996, the average increase was 3.5 percent. To take an extreme case, the price of stock of AOL (America Online) went up by 18 percent when its inclusion in the S&P 500 was announced (Shleifer 2000, p. 22).

At one level, the cause of an increase in price following a stock's inclusion in the S&P 500 or similar indices seems straightforward: index funds, which as noted in chapter 3 have become an extremely large category of investor, do not hold the stock before its inclusion, and when it is included they have to buy it. Again, though, such an explanation is unsatisfactory from the viewpoint of efficient-market theory. It runs directly against a central tenet of finance theory, which goes back to Modigliani, Miller, Markowitz, and Sharpe but which was expressed perhaps most clearly by Myron Scholes on the first page of his Ph.D. thesis: "The shares a firm sells are not unique works of art, but abstract rights to an uncertain income stream for which close counterparts exist either directly or indirectly via combinations of assets of various kinds." (Scholes 1970, p. 1)

If a purchase or sale of stock conveys no information—if it is not, for instance, the result of a corporate insider unloading stock before bad news

becomes public—then even a large purchase or sale should have an effect on price that is “close to zero” (Scholes 1972, p. 182). For example, if a stock is selected for inclusion in the S&P 500 then large purchases by index funds are necessary, but those purchases are “informationless.” Present holders of the stock in question should not need any significant inducement to sell it and to invest instead in one or more of its close substitutes. In consequence, as Scholes wrote (but as Modigliani, Miller, or Sharpe could have written), “the market will price assets such that the expected rates of return on assets of similar risk are equal” (Scholes 1972, p. 182).<sup>20</sup> Inclusion in or exclusion from an index should have no effect on risk level, thus no effect on expected rates of return, and no effect on stock price. That there was an effect could therefore be seen as an anomaly whose existence was evidence against efficient-market theory (Shleifer 2000, pp. 21–23, 48–50). The “investment advice implied by the Efficient Market Hypothesis”—that is, to invest in index funds—“may itself be undermining the efficiency of the stock market” (Morck and Yang 2001, p. 32).

In recent years, however, the “index inclusion” anomaly seems to have diminished considerably (Cusick 2001) as the familiar process of identification, exploitation, and eventual attenuation or elimination takes hold. For example, analysis by the investment bank J. P. Morgan suggests that in the late 1990s and in 2000 it was fairly reliably profitable to buy stock if it seemed probable that a company might be added to the United Kingdom’s leading stock index, the FTSE (*Financial Times*–Stock Exchange) 100, while selling those companies at risk of being deleted. Since 2000, however, the strategy’s profitability “has been eroded as it has become increasingly arbitrated” (Loeys and Fransolet 2004, p. 16).

### ***Mandelbrot’s Monsters***

Beyond the anomalies (and earlier than them in its emergence) lay another kind of problem for efficient-market theory—a problem that seems technical but is deep in its ramifications. If changes in security prices were in some sense “random,” how was that randomness to be characterized mathematically? Was it a “mild” randomness that could be treated by standard statistical techniques, or a “wild” randomness not susceptible to those techniques?

The question of the nature of the randomness manifest in financial markets was eventually to become more than academic. If randomness was “wild,” then markets were more dangerous places than might be imagined, and, as we shall see in chapter 7, some of the emerging practical applications of finance theory might fail disastrously.<sup>21</sup>

“The random walk theory is based on two assumptions,” wrote Eugene Fama (1963, p. 420): “(1) price changes are independent random variables, and (2) the changes conform to some probability distribution.” But *which* probability distribution? As we saw in chapter 2, given the dominance in statistical theory of the normal distribution (the canonical “bell-shaped” curve), a natural first guess—for example, Bachelier’s guess—was the normal distribution and thus a random walk of the form that became familiar to physicists as a model of Brownian motion. A second, modified suggestion—Samuelson’s and Osborne’s suggestion—was a log-normal random walk. At the start of the 1960s, however, another far more disturbing, wildly random set of possibilities began to be canvassed.

The proponent of wildness versus mildness was Benoit Mandelbrot. His challenge to the mathematical foundations of much of modern economics was so fundamental that—unlike most of the topics discussed in this book—it has attracted the attention of historians of economics, notably Philip Mirowski (1989, 1990, 1995) and Esther-Mirjam Sent (1998, 1999).<sup>22</sup>

Mandelbrot, born in Poland in 1924, was educated mainly in France. His family, which was Jewish, moved there in 1936. An uncle, Szolem Mandelbrojt, had become prominent in mathematics in France. After keeping “body and soul together under . . . very dangerous circumstances” during World War II, Mandelbrot took the highly competitive examinations to gain access to the elite École Polytechnique and the ultra-elite École Normale Supérieure. Having won entry to both schools, he entered the latter but “quit the next day. . . . I hated the place because the place was going to be taken over by Bourbaki.” (Mandelbrot interview<sup>23</sup>)

“Nicolas Bourbaki” was the pseudonym of an increasingly influential group of French mathematicians who were seeking to systematize mathematics and to increase its rigor. During the war, Mandelbrot had learned that he had an intuitive flair for geometry. He “realized that [he] could geometrize spontaneously, instantly, a very large amount” of the mathematics he had been taught. However, from talking to his uncle, a founder member of the Bourbaki group, Mandelbrot also knew that geometric intuition did not meet with favor in Bourbaki’s “most formalistic way of doing mathematics” (Mandelbrot interview).

After his graduation from the École Polytechnique, where he was taught analysis by the probability theorist Paul Lévy, Mandelbrot became something of a wandering scholar. He seemed to move from topic to topic, institution to institution, even discipline to discipline: to the California Institute of Technology, where he studied aeronautics (in particular, turbulence); back to the University of Paris, where half of his 1952 Ph.D. thesis was on the Zipf

word-frequency distribution in mathematical linguistics and half on statistical thermodynamics; to the Institute for Advanced Study in Princeton in 1953–54 as the last postdoc of John von Neumann; to Geneva “to attempt a collaboration with Jean Piaget,” the famous developmental psychologist; then to a professorship of mathematics at Lille.<sup>24</sup>

In the summer of 1958, Mandelbrot found a congenial intellectual home: the IBM Research Center at Yorktown Heights in Westchester County, just north of New York City. Soon to occupy the landmark modernist building designed for it by Eero Saarinen, the IBM Research Center was at its peak “the world’s largest campus for computer science research” (Knowles and Leslie 2001, p. 17). It freed Mandelbrot from the constraints of standard academic disciplines without subjecting him to undue pressure to work on pragmatically useful topics. IBM was big enough and successful enough to allow its research “stars” considerable freedom, and their contributions to corporate profits were not yet under close scrutiny.

Talking with Mandelbrot, it is hard not to be reminded of Imre Lakatos’s 1976 book *Proofs and Refutations*, especially as re-interpreted in the light of the work of the anthropologist Mary Douglas (1970a,b) by the sociologist of knowledge David Bloor (1978). The issue on which Lakatos and Bloor focus is reaction to mathematical anomalies (Lakatos calls them “monstrosities” or “monsters”<sup>25</sup>) such as counterexamples to a putative proof.

Mathematicians can simply be indifferent to counterexamples. Alternatively, they can “bar” monsters, for example redefining “polyhedron”<sup>26</sup> so that an anomalous polyhedron isn’t a polyhedron at all. (Lakatos’s main example is a theorem concerning the relationship between the numbers of vertices, edges, and faces of any polyhedron.<sup>27</sup>) Other ways to preserve a theorem in the face of a counterexample are “exception-barring” (for example, restricting the theorem’s domain so that it no longer includes the anomaly) and “monster-adjustment” (that is, redefining terms so that the anomaly is no longer a counterexample).

The approach to monsters that Lakatos recommends, however, involves embracing them as spurs to conceptual innovation: the “dialectical method,” Lakatos calls it (1976, p. 94 ff.). That has been Mandelbrot’s attitude: “I’m always ready to look at anything curious and bizarre.” (Mandelbrot interview)

For example, Mandelbrot became interested in linguistics after reading a review (Walsh 1949) that his uncle had thrown into his wastepaper basket. The review alerted him to Zipf’s law,<sup>28</sup> a regularity in word frequencies that holds across languages. It was apparently a mere curiosity from a field in which Mandelbrot had no training, and others might have disregarded it, but Mandelbrot did not. It was part of the circuitous route that took him to finance.



Though the case of a single individual proves nothing sociologically, it is interesting to note that Mandelbrot's self-perceived and self-chosen social situation is precisely that in which Bloor (1978) predicts the dialectical, monster-embracing method will flourish: one in which both group identities and hierarchies are weak. "For many years, every group I knew viewed me as a stranger, who (for reasons unknown) was wandering in and out," Mandelbrot told a 1985 interviewer (Barcellos 1985, p. 213). In my interview with him, Mandelbrot commented:

... very often people tell me "How come you attract more randomness than anyone else?" In fact, I don't think I do. But most people lead a more organized life. They know where their power can be exerted, who has this power over them. They know their place in society. Also, randomness perturbs, and institutions don't like it, hence correct for random inputs very rapidly. . . . Many of the events which I describe would have been simply dismissed by almost everybody. People would say . . . "this doesn't belong to my field, I must first finish writing some papers. . . ."

The review retrieved from his uncle's wastepaper basket sparked Mandelbrot's interest in variables such as individuals' incomes and word frequencies in languages that can be modeled as following what mathematicians call "power laws."<sup>29</sup> Pondering the distribution of income, Mandelbrot saw the possibility of applying to the topic a more sophisticated mathematical formulation derived from the work of his undergraduate professor Paul Lévy. Among the topics investigated by Lévy was probability distributions that are "stable" in the sense that if two independent variables follow a probability distribution of a given form, their sum also follows that probability distribution (Lévy 1925, pp. 252–277).<sup>30</sup>

For Lévy, and for most of the mathematicians who took up his work, the topic was a theoretical investigation in the foundations of probability and statistics (Zolotarev 1986). Mathematically, all but one member of the family of Lévy distributions are "monsters." Apart from in a few special cases, mathematicians do not even know how to write them down in the standard, explicit way. (See appendix B.)

The parameter that is most important in distinguishing between different members of the Lévy family of distributions is the "characteristic exponent," which Lévy denoted by the Greek letter  $\alpha$  (alpha). Alpha is always greater than zero and no greater than 2. The lower the value of alpha, the more cases fall in the tails of the distribution: in other words, the higher the proportion of events that are extreme, "wild," deviations from the average.

A value for alpha of 2 corresponds to the normal distribution, with its "non-monstrous," well-understood, mathematically tractable properties and rapidly diminishing tails. It is a form of "mild" randomness: the rapidly diminishing

tails mean that extreme events are very infrequent. If  $\alpha$  is less than 2, however, the tails of the distribution are sufficiently “fat” that the statistician’s standard ways of measuring the extent of a distribution’s “spread” (the standard deviation and its square, the variance) no longer exist: the integral expression that defines them does not converge, and so they have no finite value and are in that sense infinite.

Infinite variance plays havoc with standard statistical procedures. Lévy distributions with values of  $\alpha$  other than 2 could thus seem to be theorists’ oddities, of little or no value to practical statisticians. I was taught statistics as an undergraduate in 1970–1972. Lévy distributions were never mentioned explicitly, though one member of the family—the “Cauchy distribution,” for which  $\alpha = 1$ —was described briefly, perhaps as a warning to students not unthinkingly to assume that a distribution’s “moments”—its mean, its variance, and so on—always exist.<sup>31</sup>

Mandelbrot saw in the distribution of income a possible practical application of Lévy distributions. Not only did income have a notoriously fat tail—there are typically far more cases of extremely high incomes than would be the case if income was normally distributed—but overall income is the sum of a number of different components. If those components were independent, and if each of them and overall income were to follow the same probability distribution, the latter must be among Lévy’s family of distributions (Mandelbrot 1960, pp. 85–86).

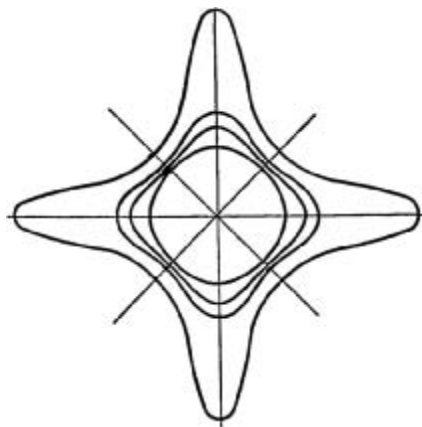
Mandelbrot knew perfectly well that he was invoking monsters: if income followed a Lévy distribution, its fat tail meant that  $\alpha$  must be less than 2, and so the variance was infinite and standard statistical techniques evaporated. Fundamental to those techniques was the “central limit theorem”: that the distribution of the mean of a large number of independent random variables tends toward normality. The theorem requires the variances of those variables to be finite: if they are not, the central limit theorem does not hold. (The theoretical interest of Lévy distributions lies primarily in this area, in the mathematics of “limit theorems for sums of independent random variables.”)<sup>32</sup>

For Mandelbrot, the failure of many existing statistical techniques in a world of Lévy distributions with infinite variances was an incentive, not a deterrent. He relished the challenge of examining situations in which the “causally structural features” of systems “are likely to be very much more hidden by noise” than if the central limit theorem applied. With established analytical approaches failing, one might have to turn, for example, to geometrical investigations and to visual intuition. Standard notions of mathematical rigor might have to be set aside: “. . . when one works in a field where the background noise is [Lévy-distributed], one must realize that one faces a burden of proof

that is closer to that of history and autobiography than to that of physics” (Mandelbrot 1963b, pp. 422 and 433).

In tackling the problem of the statistical form of the distribution of income, Mandelbrot was “coming out of left field and entering a very obscure, but old topic of economics” (Mandelbrot interview). What suddenly moved his work to centrality “was another of these lucky breaks.” Mandelbrot was invited by Hendrik Houthakker—the economist who told Samuelson about Kendall’s work on price changes as random walks—to give a talk at Harvard University. “Before getting there,” Mandelbrot recalled, “I stopped at my host’s [Houthakker’s] office . . . and on his blackboard I saw a drawing I was going to use in my talk. I was very surprised and said “How come you have on your blackboard a drawing from my talk?” He said ‘I have no idea what you’re talking about.’” (Mandelbrot interview)

On Houthakker’s blackboard were data about changes in the price of cotton. What Mandelbrot—champion of the role of the “eye” in science—saw in Houthakker’s data was a sign of his monsters, indications of the joint distribution of two Lévy variables (figure 4.1) that exhibited a particular form of dependence. In the latter, if the two variables indicate the changes in prices



**Figure 4.1**

A joint distribution of two Lévy-stable variables. If the two variables are independent, “they should be plotted along the horizontal and vertical coordinate axes.” If they follow the model of dependence discussed in the text, “they should be plotted along the bisectrices,” in other words the lines at  $45^\circ$ . The roughly circular and cross-shaped contours are “probability isolines”: every point on one of the lines has the same probability of occurrence. Source: Mandelbrot 1963a, p. 403. Courtesy of Benoit Mandelbrot and the University of Chicago Press.

in two successive time intervals, “large changes tend to be followed by large changes—of either sign—and small changes tend to be followed by small changes” (Mandelbrot 1963a, p. 418). If we were in the non-monstrous world of the normal distribution, the “probability isolines” of the joint distribution would be circles or ellipses. Instead, in Houthakker’s data, Mandelbrot saw the cross-shaped isolines of figure 4.1: the mark, to his eye, of his monsters.

The pattern that Mandelbrot glimpsed on Houthakker’s blackboard shifted his focus from the distribution of income (a peripheral problem in the mainstream economics of the 1960s) to the distribution of price changes, which as we saw in chapter 2 was becoming a central topic. Mandelbrot started working with cotton prices, taking Houthakker’s price data back to the IBM Research Center at Yorktown Heights “in a box of computer cards” (Gleick 1988, p. 85), obtaining further prices going back to 1880 from the U.S. Department of Agriculture, and analyzing all these data on the powerful IBM 7090 computer at Yorktown Heights. Soon he was working on the prices of wheat and of other grains, on the prices of nineteenth-century railroad stocks, and on interest rates. Everywhere, he found evidence that price changes in “very active speculative markets” (Mandelbrot 1963a, p. 404) followed Lévy distributions.

The Lévy hypothesis was attractive for four reasons.

First, Lévy distributions had been shown mathematically to occupy a certain privileged position. A generalization of the central limit theorem that was known well enough to be found in the more advanced textbooks of probability theory was that “if a sum of independent random variables with common distribution tends toward any limiting distribution, that distribution will be a member of the [Lévy] class” (Roll 1970, p. 63, summarizing Feller 1966, p. 168). The random-walk hypothesis suggested that price changes were indeed sums of independent random variables, so this theorem might well apply.

Of course, even if the generalization of the central limit theorem did apply, that might mean only that the limit distribution was normal, in other words that  $\alpha$  was equal to two. However, a second reason for the attractiveness of the Lévy hypothesis was that it was gradually being concluded that price changes in a variety of contexts were fat-tailed: the relative frequency of large changes was higher than would be expected if those changes followed a normal or log-normal distribution.<sup>33</sup> That could of course be ignored as a mere empirical anomaly, but Mandelbrot saw it as evidence that what was about to become the standard mathematical model of random-walk price changes was wrong, and that alternatives should be explored.

Third, Lévy distributions have the elegant property that we now call “self-similarity.” Price changes in longer time periods are simply the sum of changes in shorter time periods, and Lévy distributions are invariant under addition.

If changes in short time periods were independent and all followed the same Lévy distribution, then so would changes over longer periods. The “scale” used in the analysis—time periods of a minute, an hour, a day, a week, a month, a year—would therefore not matter: the form of the distribution would be unchanged.

A final reason for the attractiveness of Lévy distributions was specific to Mandelbrot. He was struggling to construct a worldview that would link his interests in the physical sciences and in social sciences like economics: a theory of “roughness,” he now calls it, “a matter of understanding [the] messiness of everything” (Mandelbrot interview). This worldview went beyond standard ways of treating statistical ensembles, for example in the kinetic theory of gases. Mandelbrot’s goal was to understand the “messy” phenomena (such as turbulence or weather) that had evaded standard “first-stage indeterministic theories” (Mandelbrot 1987, p. 121).<sup>34</sup>

Mandelbrot’s emerging worldview became an important component of the theory of “chaos” that became prominent in the 1980s, particularly via his “fractal” geometry (the geometry of intrinsically rough, irregular shapes). Chaos theory’s most celebrated artefact is the “Mandelbrot set,” a geometric configuration of enormous complexity that Mandelbrot calls a “devil’s polymer” (Gleick 1988, p. 228).

In the early 1960s, Mandelbrot’s fractal geometry still lay in the future, and few of his wider ambitions were explicit in his publications from the period.<sup>35</sup> However, even the part of Mandelbrot’s program that was visible generated considerable interest within economics. The young man’s background in mathematics went far beyond that of most of the contributors to the theory of finance, he was using ideas that had not been used before, and his unusual scope was already clear: “He had all kinds of things. He had data on the Nile, the levels of the Nile, the frequency that it overflows, and everything in nature, economics and everything else was characterized by fat tails.” (Fama interview)

Early in 1962, Mandelbrot was in Chicago and took the opportunity to visit the Graduate School of Business. Merton Miller and his colleagues were impressed—“We all wanted to make him an offer [of a professorship]” (Miller interview)—and a productive collaboration, albeit an informal and largely long-distance one, ensued. “Benoit had a great influence on a lot of us, not just the fat tails but the first really rigorous treatment of expectations [Mandelbrot 1966], and so on. . . . I’m, to this day, a Mandelbrotian.” (Miller interview)

The University of Chicago financial economist who took up Mandelbrot’s work most actively was Eugene Fama, still a graduate student in 1962. Among the things that exercised Fama were the “fat tails” of the distributions of

changes in stocks' prices. Fama saw evidence of fat tails in the data used by Kendall and by Osborne (whose work was discussed in chapter 2) and also in those analyzed by his fellow Chicago Ph.D. student Arnold Moore (Moore 1962). Kendall, Osborne, and Moore, however, had not focused on the high frequencies of extreme events. Like Mandelbrot, Fama embraced this anomaly. He worked closely with Mandelbrot, who became an unofficial advisor of Fama's Ph.D. research (Mandelbrot interview; Fama 1965, p. 34).

In his thesis work, Fama analyzed the daily price changes in the period 1957–1962 for each of the thirty stocks that made up the Dow Jones industrial average. In every case, the frequency distribution of the changes in the logarithms of prices was, in the statistician's terminology, "leptokurtic": it had a high peak and fat tails. Had the distribution been normal, price changes greater than five standard deviations should hardly ever have occurred: their frequency should be 0.00006 percent, and such events should be observed in daily price data "about once every 7,000 years." In fact, the frequency in the total sample of these extreme price changes was consistent with them occurring "about once every three to four years," 2,000 times as often as would be expected on the basis of normality (Fama 1965, pp. 48–50). In a relatively small sample, only a few events of such extremity were observed, but Fama found that less extreme fluctuations were also more frequent than they should be on the basis of normality.<sup>36</sup>

Fama concluded that the distributions of stock-price changes were unequivocally fat-tailed.<sup>37</sup> Were they Lévy distributions? Were their variances infinite? Those were harder questions to answer. In any sample of finite size, the sample variance will always be finite even if the underlying distribution has an infinite variance, so these questions cannot be answered by calculating a variance. Nor could one realistically hope to decide simply by plotting sample data in a graph and inspecting its shape: the "actual graphs" of contending interpretations "look very much alike," and which was deemed superior tended to depend on the criteria of goodness of fit employed (Mandelbrot 1973, p. 157).

A further problem was the difficult mathematics of Lévy distributions. As was noted above, apart from in special cases there was not even a simple way of writing down those distributions: as Fama pointed out, "explicit expressions" for their probability density functions (see appendix B) were in general "unknown." How best to estimate their parameters, especially alpha, was not clear. "Sampling theory for the parameters of these distributions," Fama wrote (1965, p. 68), "is practically non-existent." Nevertheless, Fama experimented with various methods of determining the value of alpha, finding that "in the vast majority of cases . . . the estimated values were less than 2," the alpha level that corresponds to the normal distribution. "The Mandelbrot hypothesis fits

the data better than the Gaussian [normal-distribution] hypothesis,” he concluded (*ibid.*, p. 90).

In the second half of the 1960s, Fama and his students, particularly Richard Roll, set to work to reduce the technical difficulties of the estimation of the parameters of Lévy distributions (Fama and Roll 1968, 1971) and to apply the results of this work in the analysis of financial data. Roll, for example, found evidence of the possible applicability of Lévy distributions to the fluctuations in the rates of return offered by U.S. Treasury bills (government bonds with short maturities).<sup>38</sup>

Others, too, turned to Lévy’s and Mandelbrot’s monsters. At MIT, Paul Samuelson investigated how Markowitz’s problem—the selection of an optimal investment portfolio—could be solved if Lévy distributions applied and variances did not exist (Samuelson 1967). At Carnegie Mellon University, Thomas Sargent (later to become famous as a founder of rational-expectations theory) and his graduate student Robert Blattberg noted “the large body of evidence suggesting that many economic variables are best described as being generated by [Lévy] distributions,” and they investigated the impact of this on standard econometric techniques such as regression and “least squares” estimation (Blattberg and Sargent 1971, p. 509).<sup>39</sup>

Nevertheless, despite all this interest, Lévy distributions remained monstrous. The possibility of an entirely different reaction to them was highlighted in 1962, almost at the very beginning of their impact on American economics. At that year’s winter meeting of the Econometric Society, Mandelbrot put forward the Lévy hypothesis, and met with sharp opposition from Paul Cootner (Mandelbrot interview). In preparing his widely read 1964 collection of papers on random-walk models, Cootner was broad-minded enough to include both Mandelbrot’s work and a simplified exposition of it by Fama, but he also included an updated version of his criticism.

Cootner attacked Mandelbrot’s “messianic tone” and expressed his discomfort with what he said was Mandelbrot’s “disturbingly casual” use of evidence, noting that much of the evidence was “graphical and involves slopes of lines which are not given precise numerical values” (Cootner 1964, p. 333). Cootner marshaled his own evidence against the Lévy hypothesis, and ended with an impassioned plea to consider the possible costs (the “loss functions,” as he put it) involved in the acceptance or rejection of the hypothesis:

Mandelbrot, like Prime Minister Churchill before him, promises us not utopia but blood, sweat, toil, and tears. If he is right, almost all of our statistical tools are obsolete—least squares, spectral analysis, workable maximum-likelihood solutions, all our established sample theory, closed distribution functions. Almost without exception, past econometric work is meaningless. Surely, before consigning centuries of work to the

ash pile, we should like to have some assurance that all our work is truly useless. If we have permitted ourselves to be fooled for as long as this into believing that the Gaussian [normal-distribution] assumption is a workable one, is it not possible that the [Lévy] revolution is similarly illusory? At any rate, it would seem desirable not only to have more precise (and unambiguous) empirical evidence in favor of Mandelbrot's hypothesis as it stands, but also to have some tests with greater power against alternatives that are less destructive of what we know. (Cootner 1964, p. 337)

Cootner was not alone in these fears. For example, Clive Granger of the University of Nottingham and Daniel Orr of the University of California at San Diego wrote that it was a "genuine cause for concern" that "the statistical theory that exists for the normal case is nonexistent for the other members of the class of [Lévy] laws." Infinite variances created "profound problems," in Granger and Orr's view, because "many of the classical methods" of statistical analysis "may not apply properly" (Granger and Orr 1972, pp. 275–276).

That unease of this kind was shared more widely is suggested by the rapidity with which support for the Lévy hypothesis evaporated in the early 1970s. One trigger of the loss of support was the work of the University of Chicago Ph.D. student Robert R. Officer, who applied Fama and Roll's improved methods of estimating alpha and found that its value rose as daily stock-price data were aggregated into longer time periods, behavior that was at odds with self-similarity or invariance under addition (Officer 1972).<sup>40</sup>

Fama's own analysis also suggested that distributions became more normal as time periods lengthened (Fama 1976, pp. 26–33), sapping his and others' interest in Lévy distributions:

If they [price changes] were really stable [Lévy-distributed], the way Mandelbrot describes them, they maintained the same properties under addition. So if you start with a stable distribution of log returns, it's going to be of the same type [the same value of alpha] when you look at monthly returns, and that wasn't true. They tend to converge towards normality. . . . I think people just kind of—because they couldn't explain the slow convergence stuff, so they kind of became disinterested in the fat tails. (Fama interview)

If one was dealing with stock or portfolio returns over periods as long as a month, Fama concluded, the tails became slim enough that "the normal distribution is a good working approximation." The "costs of rejecting normality for securities returns in favor of [Lévy] non-normal distributions are substantial," Fama wrote. Although "most of the models of the theory of finance" could be reconstructed on the basis of Lévy distributions, the "exposition" of those models became more complicated and "statistical tools for handling data from [Lévy] non-normal . . . distributions are primitive relative to



the tools that are available to handle data from normal distributions.” If one was working at time scales of a month, the latter tools could justifiably be used (Fama 1976, pp. 26, 33–35).

For Roll, too, the appearance of evidence at odds with self-similarity (in his case, in particular in Hsu, Miller, and Wichern 1974) suggested that further investment of his time in work on Lévy distributions would be mistaken: “I started thinking . . . that maybe I shouldn’t waste too much effort trying to go further on the Lévy laws. I worked a long time trying to derive closed-form density formulas [see appendix B] and stuff like that, and I could have done that the rest of my life, I suppose. So I decided to quit that and do other things.” (Roll interview)

Mandelbrot agrees that Officer’s and similar work showed that “simple scaling did not apply.” However, he questions the conclusions drawn from that work (“They were hasty and indefensible”), and he regards the notion that “somehow things become Gaussian [normal]” as an “arrant” misinterpretation.<sup>41</sup>

For example, Officer’s estimates of the values of alpha rose from around 1.5 for daily data to around 1.7 for 20-day periods. The latter value was still short of the normal distribution’s alpha value of 2, although it was an estimate with quite a substantial statistical error band.<sup>42</sup> “Replacing the observed 1.7 by the desired 2.0 leads to enormous underestimates of the risks,” says Mandelbrot. “As to the bold extrapolation that suggests that in some distant asymptotic regime the distributions become normal, why should we care? Hardly anybody cares about the tails over a century.”<sup>43</sup>

Nor is the failure of an empirical prediction, even a central one, in itself enough to explain the abandonment of the Lévy model. As we saw in chapter 2, the Capital Asset Pricing Model made, at least once Sharpe had the confidence to embrace it, a prediction that was manifestly false—that all investors would hold portfolios of risky assets that were identical in their proportionate composition—but that did not stop the model being adopted widely. What was at least as important as the empirical doubt cast on self-similarity or invariance under addition in explaining the abandonment of the hypothesis of Lévy distributions was that a variety of other ways of modeling distributions with high frequencies of extreme events were becoming available (see, for example, Clark 1973).<sup>44</sup>

Crucially, the alternatives to Lévy distributions also have tails fatter than the normal or log-normal distributions—for example, because they involve fluctuating levels of volatility—but they lack the monstrous feature of infinite variance. They thus preserved the usefulness of the standard statistical and econometric techniques whose loss Cootner, Granger, and Orr had feared.

Finite variances also made it much easier to develop “rational-expectations” models, in which, as noted in chapter 1, economic agents are modeled as being able to perform econometricians’ inferences. (See Sent 1998, 1999.) Econometricians had difficulty making those inferences in a world of Lévy distributions with infinite variances, so if ordinary economic agents were like econometricians they might not be able to form rational expectations.

For all the apparent technicality of the issue of infinite variance, Lévy distributions had aroused strong passions. One mathematically sophisticated interviewee, Oldrich Vasicek, told me that he thought that Mandelbrot “must just have pulled everybody’s legs the whole time. . . . I just cannot believe he would actually take it [Lévy distributions with infinite variance] seriously himself. . . . It has consequences so brutal that nobody would be able to live with that.” If the logarithm of a stock price varied according to a Lévy process with infinite variance, as Mandelbrot had hypothesized, then the expected value of the price itself would be infinite. “So you’d be saying with straight face that you have an infinite expectation for tomorrow’s Xerox price.”<sup>45</sup>

I put Vasicek’s argument to Mandelbrot. He looked at me somewhat sternly and counterattacked, asking “Why not?” In many contexts, what statisticians call “moments” (the expected value, the variance, and so on) have “a concrete meaning,” but in others they “are . . . a totally acquired habit.”<sup>46</sup> The “alternative universe of thinking” he has been developing “requires painfully fresh thinking. . . . For too long have economists been trying to squeeze examples of clear-cut wild variability into the state of mild variability.” (Mandelbrot interview)

Mandelbrot went on to supplement Lévy distributions with other non-standard stochastic processes.<sup>47</sup> However, he came to feel that “in the economics profession of the 1970s [he] wasn’t welcome” (Mandelbrot interview), and he moved on to the other “chaos theory,” “fractal,” and “multifractal” topics referred to above.

When Mandelbrot returned to the study of finance after the 1987 crash, he used more general tools, which allow for both finite and infinite variance and for dependent as well as independent price changes, and which go beyond the issues discussed in this section (Mandelbrot 1997; Mandelbrot and Hudson 2004). He gave no ground to his critics, however, and continues to insist that markets’ “wild” randomness (Mandelbrot 1997, p. 9) can be modeled only using approaches “that not everybody will immediately welcome.”<sup>48</sup>

Within mainstream financial economics, the hypothesis of Lévy distributions with infinite variance largely disappeared after the start of the 1970s. Mandelbrot’s monsters had been banished. They were not quite dead, but their reappearance is much further on in my story. For—just as Lévy distributions

were vanishing—what was in many ways finance theory’s greatest triumph was coming to fruition: the theory of options. It employed the log-normal model, the butt of the criticism by Mandelbrot and those inspired by him. The theory of options is the topic of the next chapter.

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## *Pricing Options*

Options are contracts that give their holder the right, but do not oblige their holder, to buy (or to sell) an asset at a set price on, or up to, a given date. They are old instruments. Puts (options to sell) and calls (options to buy) on the stock of the Dutch East India Company were being bought and sold in Amsterdam when Joseph de la Vega discussed its stock market in 1688 (de la Vega 1957), and subsequently options were traded widely in Paris, London, New York, and other financial centers.

Before the 1970s, age had not brought options respectability. Sometimes they were seen simply as wagers on stock-price movements. Options also seemed the perfect tool for the speculator. Because the cost of an option was and is typically much less than that of the underlying stock, a speculator who correctly anticipated price increases could profit considerably by buying calls, or benefit from decreases by buying puts. Sometimes such speculators themselves caused prices to move in the direction they wanted—by spreading false rumors, for example—so options also became associated with market manipulation.

In Britain, options were banned from 1734 and again from 1834, and in France from 1806, although these bans were widely flouted.<sup>1</sup> Several American states, beginning with Illinois in 1874, also outlawed options (Kruizenga 1956). Although the main target in the United States was options on agricultural commodities, options on securities were often banned as well.

The dubious reputation of options did not prevent occasional serious interest in them. In 1877, for example, the London broker Charles Castelli, who had been “repeatedly called upon to explain the various processes” involved in buying and selling options, published a booklet explaining them, directed apparently at his fellow market professionals rather than popular investors (Castelli 1877, p. 2).

Castelli concentrated primarily on the profits that could be made by the purchaser, and discussed only in passing how options were priced, noting that

prices tended to rise in periods of what we would now call high volatility. His booklet ended—in a nice corrective for those who believe the late twentieth century’s financial globalization to be a novelty—with an example of how options had been used in bond arbitrage between the London Stock Exchange and the Constantinople Bourse to capture the high contango rate prevailing in Constantinople in 1874.<sup>2</sup> (“Contango” was the rate of interest that the sellers of securities charged purchasers who had not yet paid for them.)

Castelli’s “how to” guide employed only simple arithmetic.<sup>3</sup> The first mathematically sophisticated analysis of options was in the 1900 thesis by Louis Bachelier discussed in chapter 2. As noted there, Bachelier modeled movements of the price of the underlying asset (in his case, bonds) as a random walk in continuous time.

Bachelier applied his random-walk model to various problems in the determination of the “strike price” of options (that is, the price at which the option gives the right to buy or sell the underlying asset), the probability of the exercise of options, and the probability of their profitability, showing a reasonable fit between predicted and observed values. (In the French market studied by Bachelier, options usually had a fixed price, and what fluctuated in the market was the strike price.<sup>4</sup> In the American markets discussed in this book, the situation was and is the reverse. Option prices fluctuate, while at least in organized option exchanges the set of strike prices is fixed.)

Another more recently rediscovered early contribution to option theory was by a Trieste actuarial professor, Vinzenz Bronzin (Bronzin 1908; Zimmermann and Hafner n.d.). However, sustained theoretical interest in the topic did not take off until the late 1950s and the 1960s. By then, as we saw in chapter 2, a variant of Bachelier’s random-walk model (the “log-normal” random walk) was becoming standard in the emerging field of financial economics. Attention quickly focused on extending that model to understand the pricing of options: papers on options made up one-fourth of Paul Cootner’s 1964 collection *The Random Character of Stock Market Prices*.

The focus on options did not arise because they were important financial instruments: they were not. The Great Crash of 1929 reignited the traditional hostility to options, which were widely seen as implicated in the excesses and abuses of the 1920s. An outright federal ban on stock-option trading was only just averted (Filer 1959), and such trading continued only in a small, illiquid, ad hoc market based in New York. In general, only brokers’ price quotations could be obtained from that market, not the actual prices at which options were bought and sold. The absence of robust price data was a disadvantage of the options traded in New York from the viewpoint of financial economists.

Instead, the main focus of attention was on warrants, a particular form of call options that were traded in more liquid, organized markets—particularly the American Exchange—and for which market prices were available. Like an ordinary call option, a warrant gives its holder the right to buy stock at a fixed price. However, ordinary call options are written (that is, issued) by investors or traders, and if the holder of such an option exercises it, option writers supply the holder with existing stock that the writer either must already possess or must buy in the market. Warrants, in contrast, are issued by corporations, and if the holder of a warrant exercises it, the corporation creates new stock to issue to the warrant holder.

Even warrants were of limited substantive importance. Rather, developing a theory of option and warrant pricing was a way of elaborating the random-walk model of stock prices: a form of “normal science” in Kuhn’s (1970) terms. As Cootner put it, “options obtain their value from the price of the underlying shares” (1964, p. 373). If one had a well-defined mathematical model of the fluctuations of stock prices (and, the issues discussed in chapter 4 aside, the log-normal random walk seemed to offer such a model), then it appeared as if all one had to do was some relatively easy mathematical analysis to reach a formula for the expected value of a warrant or option. It might then be possible to use such a formula to work backward from option or warrant prices to investors’ expectations about stock-price rises or falls—or so it seemed.

The researcher in whose work the goal of working backward from warrant prices to investors’ expectations and attitudes to risk was most prominent was Case Sprenkle, a graduate student in economics at Yale University in the late 1950s. His interest in options was sparked by a talk at Yale given by Paul Samuelson, whose work on financial economics was discussed in chapter 2. Sprenkle used the log-normal model of stock-price movements—by the late 1950s, that model was “in the air” (Sprenkle interview), and Samuelson in particular was advocating it—to work out the expected value of a warrant’s payoff: see expression 1 in appendix C.

Sprenkle then argued that this expected value would be a warrant’s value to an investor only if the investor was indifferent to risk or “risk neutral.” To get a sense of what this means, imagine being offered a fair bet with a 50 percent chance of winning \$1,000 and a 50 percent chance of losing \$1,000, and thus an expected value of zero. If you would require to be paid to take on such a bet you are “risk averse”; if you would pay to take it on you are “risk seeking”; if you would take it on without inducement, but without being prepared to pay to do so, you are “risk neutral.” A risky investment is worth exactly the expected value of its payoff only to a risk-neutral investor. It is worth less than that to a risk-averse investor, and more than that to a risk-seeking one.

Warrants and other forms of option are riskier than the underlying stock because of their leverage, in other words because “a given percentage change in the price of the stock will result in a larger percentage change in the price of the option” (Sprenkle 1961, p. 189). An investor’s attitude to risk could be conceptualized, Sprenkle suggested, as the “price” the investor was prepared to pay for leverage.

Sprenkle’s formula for the value of a warrant to an investor (equation 2 in appendix C) involved three unknowns: the expected extent of the stock’s appreciation in price by the warrant’s expiry date; the standard deviation of the stock’s anticipated price distribution at expiry; and the “price” the investor was prepared to pay for a warrant’s leverage. The price of leverage would be negative for a risk-averse investor, zero for a risk-neutral investor, and positive for a risk-seeking investor.

Sprenkle (1961, pp. 199–201) assumed that the values of his three unknowns, and thus the value of a warrant, would vary from investor to investor: “Actual prices of the warrant then reflect the consensus of marginal investors’ opinions—the marginal investors’ expectations and preferences are the same as the market’s expectations and preferences.”

Sprenkle examined warrant and stock prices for the “classic boom and bust period” of 1923–1932 and for the relative stability of 1953–1959, hoping to estimate from those prices “the market’s expectations and preferences,” in other words the levels of expected stock prices and their anticipated standard deviations, and also investors’ attitudes to risk as expressed by the price of leverage. His econometric work, however, hit considerable difficulties: “it was found impossible to obtain these estimates.” Only by arbitrarily assuming no expected appreciation in prices and testing out a range of arbitrary values of the price of leverage could Sprenkle make partial progress. His theoretically derived formula for the value of a warrant depended on parameters whose empirical values were hard to estimate empirically (Sprenkle 1961, pp. 204, 212–213).

The most sophisticated theoretical analysis of warrants in the early and mid 1960s was by Paul Samuelson, the MIT economist discussed in previous chapters. Samuelson developed a collaboration with the MIT mathematician Henry P. McKean Jr., a world-class specialist in stochastic calculus (the theory of stochastic processes in continuous time), which in the years after Bachelier’s work had become an important area of probability theory. Samuelson’s model also depended, like Sprenkle’s, on parameters that seemed to have no straight-forward empirical referents—the expected rate of return on the underlying stock, and the expected rate of return on the warrant—and Samuelson offered

no way of directly estimating these parameters (Samuelson 1965a; McKean 1965).

A University of Chicago Ph.D. student, A. James Boness, took a simpler route than Samuelson. Boness made the simplifying assumptions that option traders are risk neutral and that the expected returns on all stocks on which options were available were the same. Boness was then able to estimate that common rate of expected return indirectly by finding the value that minimized the difference between predicted and observed option prices. Unfortunately, the expected return calculated by Boness for the sample he was studying (he analyzed the prices of options in the ad hoc New York market, rather than those of warrants) was 22 percent per year, which seemed to him to be implausibly high (Boness 1964).

Herbert Simon, whose “behavioral” criticism of financial economics was discussed in chapter 2, would no doubt have felt that option theory in the 1960s was a perfect manifestation of his point. The theoretical formulas for the values of options and warrants always involved unobservable parameters, in particular the parameter on whose unobservability Simon had focused, a stock’s expected rate of return. Furthermore, attempts to work backward from observed option prices to estimate expected stock returns and other underlying parameters had proved frustrating: no entirely convincing estimates had been generated.

### ***“The Greatest Gambling Game on Earth”***

An alternative approach to option and warrant prices was to eschew a priori models and to try to find the determinants of those prices empirically. The most significant work of this kind was conducted by Sheen Kassouf. After a mathematics degree from Columbia University, Kassouf set up a successful technical illustration firm. Fascinated by the stock market, he was a keen investor if not always a successful one.

In 1961, Kassouf wanted to invest in the defense company Textron, but could not decide between buying its stock or its warrants (Kassouf interview). He began to examine the relationship between stock and warrant prices, finding empirically that the simple curve expressed by equation 3 in appendix C seemed roughly to fit the observed relationship between warrant price, stock price, and strike price (Kassouf 1962, p. 26).

In 1962, Kassouf returned to Columbia to study warrant pricing for a Ph.D. in economics. His earlier curve fitting was replaced by econometric techniques, especially regression analysis, and he posited a more complex relationship



determining warrant prices (equation 4 in appendix C). However, Kassouf's interest in warrants was not simply academic: he wanted "to make money" trading them (Kassouf interview). He had rediscovered, even before starting his Ph.D., an old form of securities arbitrage (described in Weinstein 1931, pp. 84 and 142–145).

Warrants and the corresponding stock tended to move together: if the stock price increased, then so did the warrant price; if the stock fell, so did the warrant. So one could be used to offset the risk of the other. If, for example, warrants seemed overpriced relative to the corresponding stock, one could sell them short (in other words, borrow and sell them), hedging the risk of stock-price increases by buying some of the stock. Trading of this sort, conducted by Kassouf in parallel with his Ph.D. research, enabled him "to more than double \$100,000 in just four years" (Thorp and Kassouf 1967, p. 32).

In 1965, as a fresh Ph.D., Kassouf was appointed to the faculty of the newly established Irvine campus of the University of California. There, he was introduced to mathematician Edward O. Thorp. Alongside research in functional analysis and in probability theory, Thorp had a long-standing interest in casino games.

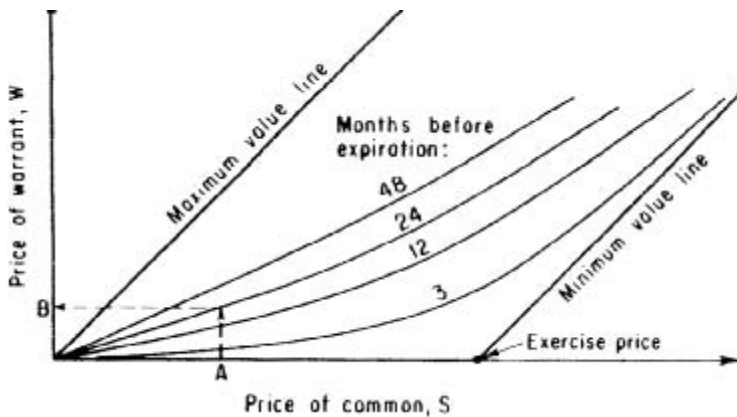
In 1959–1961, while at MIT, Thorp had collaborated with the celebrated information theorist Claude Shannon on a tiny, wearable analog computer system that could predict where the ball would be deposited on a roulette wheel (Thorp interview). Thorp went on to devise the first effective methods for beating the casino at blackjack by keeping track of cards that had already been dealt and thus identifying situations in which the probabilities were favorable to the player (Thorp 1961; Tudball 2002).

Thorp and Shannon's use of their wearable roulette computer was limited by easily broken wires, but card counting was highly profitable. In 1961, during MIT's spring recess, Thorp traveled to Nevada equipped with a hundred \$100 bills provided by two millionaires with an interest in gambling. After 30 hours of blackjack, Thorp's \$10,000 had become \$21,000. He went on to devise, with computer scientist William E. Walden of the nuclear weapons laboratory at Los Alamos, a method for identifying favorable side bets in the version of baccarat played in Nevada.

Thorp found, however, that beating the casino had disadvantages as a way of making money. At a time when American casinos were controlled largely by organized criminals, there were physical risks. (In 1964, while playing baccarat, Thorp was nearly rendered unconscious by "knockout drops" in his coffee.) The need to travel to places where gambling was legal was a further disadvantage to an academic with a family (Thorp interview).

Increasingly, Thorp's attention switched to the financial markets. "The greatest gambling game on earth is the one played daily through the brokerage houses across the country," Thorp told the readers of his hugely successful book describing his card-counting methods (Thorp 1966, p. 182). But could the biggest of casinos succumb to Thorp's mathematical skills? Predicting stock prices seemed too daunting: "... there is an extremely large number of variables, many of which I can't get any fix on." However, he realized that he could "eliminate most of the variables [by thinking] about warrants versus common stock" (Thorp interview). Thorp began to sketch graphs of the observed relationships between stock and warrant prices, and meeting Kassouf provided him with a formula (equation 4 in appendix C) for these curves.

Thorp and Kassouf's 1967 book *Beat the Market* examined the relationship between the price of a warrant and of the underlying stock. (See figure 5.1.) No warrant should ever cost more than the underlying stock, since it is simply an option to buy the latter, and this constraint yields a "maximum value line." If at any time the price of the stock exceeded the warrant's strike or exercise price (the price at which it gave one the right to buy the stock) by more than the price of the warrant, an instant arbitrage profit could be made by buying the warrant, exercising it, and selling the stock thus acquired at its market price. So a warrant can never cost less than the stock price minus the strike or exercise price, and this determines the "minimum value line" in figure 5.1.



**Figure 5.1**

"Normal price curves" for a warrant. From Thorp and Kassouf (1967, p. 31). Copyright Edward O. Thorp and Sheen T. Kassouf. Used by permission of Random House, Inc.  $S$  is Thorp and Kassouf's notation for the price of the stock.

Thorp and Kassouf depicted the relationship between warrant price and stock price as a set of curves between the maximum and minimum value lines. (See figure 5.1.) As expiration approached, those curves dropped toward the minimum value line, because that line was also the warrant's payoff at expiration. (If at the warrant's expiration the stock price is below the strike or exercise price of the warrant, the latter is worth nothing, since exercising it costs more than simply buying the stock on the market. If at expiration the stock price is above the exercise price, the warrant is worth the difference.)

From the viewpoint of a financial economist, the problem of option pricing (at least as far as warrants or call options are concerned) could thus be summarized as the problem of finding an equation for the curves in figure 5.1 that had an economic, and not simply an empirical, justification. As its title indicated, however, that was not the focus of *Beat the Market*. The "normal price curves" generated by Kassouf's empirical formula (equation 4 in appendix C) were to be used by the reader to identify overpriced and underpriced warrants.<sup>5</sup> The former could be sold short, and the latter bought, with the resultant risks hedged by taking a position in the stock (buying stock if warrants had been sold short; selling stock short if warrants had been bought).

The appropriate size of hedge, Thorp and Kassouf explained (1967, p. 82), was determined by "the slope of the normal price curve at our starting position." If that slope were, say, 1:3, as it roughly is at point (A,B) in figure 5.1, the appropriate hedge ratio was to buy one unit of stock for every three warrants sold short. Any movements along the normal price curve caused by small stock-price fluctuations would then have little effect on the value of the overall position, because the loss or gain on the warrants would be balanced by a nearly equivalent gain or loss on the stock. Larger stock-price movements could of course lead to a shift to a region of the curve in which the slope differed from 1:3, and in their investment practice both Thorp and Kassouf adjusted their hedges when that happened (Thorp 2002; Kassouf interview).

Initially, Thorp relied on Kassouf's empirical formula for warrant prices (equation 4 in appendix C). As he says, "it produced . . . curves qualitatively like the actual warrant curves." Yet he was not entirely satisfied: "quantitatively, I think we both knew that there was something more that had to happen" (Thorp interview). He began his investigation of that "something" in the same way as Sprenkle—assuming that the distribution of changes in the natural logarithms of stock prices is normal, and working out the expected value of a warrant's payoff at expiration—reaching a formula equivalent to Sprenkle's (equation 1 in appendix C).

Like Sprenkle's, Thorp's formula (Thorp 1969, p. 281) for the expected value of a warrant's payoff involved the expected increase in the stock price,

which there was no straightforward way to estimate. Thorp decided to approximate it by assuming that the expected price of the stock rose at the riskless rate of interest (the rate paid by a borrower who creditors are certain will not default, for which a usual indicator is the yield of bonds issued in their own currencies by major governments). He had no better estimate, and he “didn’t think that enormous errors would necessarily be introduced” by the approximation. Thorp found that the resultant formula was plausible—“I couldn’t find anything wrong with its qualitative behavior and with the actual forecast it was making”—and in 1967 he began to use it to identify grossly overpriced options to sell (Thorp interview).

Thorp’s expression for the value of a warrant or call option was formally equivalent to that of Fischer Black and Myron Scholes (equation 2 in appendix D), except for one feature: unlike Black and Scholes, Thorp did not discount the expected value of the option at expiration back to the present. (In other words, his value for the option price did not take into account the fact that the seller of an option could earn interest on the proceeds of its sale.)

In the warrant markets in which Thorp traded, the proceeds of the short sale of a warrant were retained in their entirety as collateral by the broker from whom the warrant had been borrowed until the warrant was repurchased and returned. The proceeds were therefore not available immediately to the seller, as Black and Scholes assumed.<sup>6</sup> It was a relatively minor difference: when Thorp read Black and Scholes, he was able quickly to see why the two formulas differed and to add to his formula the necessary discount factor to make them identical (Thorp 2002).

### ***Black and Scholes***

In the background, however, lay more profound differences of approach. Black and Scholes approached the problem of option pricing from a direction quite different to Thorp’s. In 1965, Fischer Black, with a Harvard Ph.D. (Black 1964) in what was in effect artificial intelligence, joined Arthur D. Little, Inc., a consultancy firm based in Cambridge, Massachusetts. There, Black met Jack Treynor, who worked in Little’s operations research group (Treynor interview). Independently from William Sharpe, Treynor had developed, though had not published, his own version of the Capital Asset Pricing Model discussed in chapter 2. It was Black’s (and also Scholes’s) use of this model that differentiated their work from the earlier research on option pricing.

For Black, it was the connection to Treynor that was decisive. Treynor specialized in the analysis of the financial aspects of business decisions such as whether to buy or to lease machine tools (Treynor and Vancil 1956). Those

decisions typically involve working out the present value of projected future income and expenditures. Such calculations depend critically on the choice of interest rate at which to discount future cash flows. (As was noted in chapter 2, a dollar to be received in a year's time is worth less than a dollar received now, because one can earn interest on the latter.)

The main difficulty was how to deal with future cash flows that were uncertain. The guidance Treynor had received from his professors at the Harvard Business School (from which he graduated with an MBA in 1955) seemed to him to fudge the issue. It "wasn't very satisfactory. They were basically saying 'well, choose the discount rate to take risk into account.' It was obvious to anybody that, depending on which risk discount rate you chose, you could get . . . radically different present values and that would either lean you toward going ahead with a project or not, and so that troubled me. It just didn't seem like an adequate answer." (Treynor interview)

Treynor first made a serious effort to solve the problem of how to discount uncertain cash flows in the summer of 1958. His parents owned a cottage in the Rocky Mountains, which they used to escape from the oppressive heat and humidity of the summers in Treynor's native Iowa, and he joined them for a vacation from his job at Little that turned into a working holiday. He drove down from his parents' cottage to the University of Denver to work in its library, and there he read Modigliani and Miller's 1958 paper on the irrelevance of capital structure to total market value. In any problem in operations research it is necessary to know what one is seeking to maximize, and in Modigliani and Miller's paper Treynor found his answer.

Like Modigliani and Miller, wrote Treynor, "we consider the fundamental financial objective of the corporation to be maximizing present market value. . . . The fundamental objective is maximizing the price for which the present owners can sell their interest." Treynor recognized that "this objective needs a carefully reasoned defense," in particular against those who saw it as "immoral," but it enabled him to work productively on the choice of risk-adjusted discount rate (Treynor 1961, pp. 3–4).<sup>7</sup>

What Modigliani and Miller's paper taught Treynor to do, in other words, was to look outside the corporation to the capital markets in order to solve problems in decision making like the choice of discount rate to use in making investment decisions. Mathematically, Treynor based his analysis "on the kind of work that I had seen the senior guys doing in the operations research group" of Arthur D. Little: they would first "try to solve the problem for a single [time] period . . . drive that . . . solution to an infinitesimally small time period, turn it into a partial differential equation and then integrate that thing out over finite time" (Treynor interview). Treynor's work on the later steps in this pro-

cedure hit mathematical difficulties that will be mentioned below, but he successfully solved the single-period problem. Treynor derived essentially the same Capital Asset Pricing Model as Sharpe, who followed the quite different route described in chapter 2.<sup>8</sup>

The Capital Asset Pricing Model provided a systematic account of the “risk premium”: the additional expected return that investors demand for holding risky assets. That premium, Treynor pointed out, could not depend simply on the “sheer magnitude of the risk,” because some risks were “insurable”: they could be minimized by diversification, by spreading one’s investments over a broad range of companies (Treynor 1962, pp. 13–14; Treynor 1999, p. 20).<sup>9</sup>

What could not be diversified away, however, was the risk of general market fluctuations. By reasoning of this kind, Treynor showed—as Sharpe did—that a capital asset’s risk premium should be proportional to its beta (the sensitivity of its returns to fluctuations in the overall level of the market). For example, an asset whose beta was zero—in other words, an asset the returns on which were uncorrelated with the overall level of the market—had no risk premium. (Any specific risks involved in holding it could be diversified away.) As was noted in chapter 2, investors in a zero-beta asset should therefore earn only the riskless rate of interest.

Sharpe’s and Treynor’s Capital Asset Pricing Model was an elegant piece of theoretical reasoning. At Arthur D. Little, Treynor became Black’s mentor in what was for the latter the new topic of finance, and they stayed in touch after Treynor left the firm in 1966 for a post at Merrill Lynch. So it is not surprising that when Black began his own theoretical work in finance it was by trying to apply the Capital Asset Pricing Model to a range of assets other than stock (which had been its main initial field of application).

Also important as a resource for Black’s research was a specific piece of work he had done jointly with Treynor on how companies should value cash flows in making their investment decisions. This was a reformulation of the later, less successful, steps in Treynor’s approach to the mathematics of capital asset pricing (Treynor 1963), and the aspect of it on which Black and Treynor collaborated had involved Treynor writing an expression for the change in the value of a cash flow in a short, finite time interval  $\Delta t$ ; expanding the expression using the standard calculus technique of Taylor expansion; taking expected values; dropping the terms of order  $\Delta t^2$  and higher; dividing by  $\Delta t$ ; and letting  $\Delta t$  tend to zero so that the finite-difference equation became a differential equation. Treynor’s original version of the latter was in error because he had left out “some terms involving second derivatives” that did not vanish, but Black and he worked out how to correct the differential equation.<sup>10</sup>

Among the assets to which Black tried to apply the Capital Asset Pricing Model were warrants. His starting point was directly modeled on his joint work with Treynor, with the stock price replacing the time-dependent “information variables” of the earlier problem. Similar manipulation, and application of the Capital Asset Pricing Model to both the stock and the warrant, led to a differential equation (equation 1 in appendix D) that related the warrant or option price to time, the riskless rate of interest, the stock price, and the stock price’s volatility (a measure of the extent of the fluctuations of the stock price).<sup>11</sup> Black’s analysis assumed that the riskless rate and volatility were constant, so his equation involved three variables: option price, stock price, and time.

However, to get an explicit expression for the price of an option, Black had to find a solution to his differential equation. The latter seemed simple—surprisingly so—but Black found it intractable. “I spent many, many days trying to find the solution to that equation,” Black later recalled: “I . . . had never spent much time on differential equations, so I didn’t know the standard methods used to solve problems like that.” (Black 1989, p. 5)

Black was “fascinated” by the differential equation’s simplicity. In it, apparently essential features of the problem (notably the stock’s beta and thus its expected return, a pervasive feature in earlier theoretical work on option pricing) no longer appeared. “But I was still unable to come up with the formula. So I put the problem aside and worked on other things.” (Black 1989, p. 6)

Black remained at Arthur D. Little until 1969, when he set up a consultancy called Associates in Finance. His links to academic financial economics gradually grew. In 1966, Black met Michael Jensen. When Myron Scholes moved from his Ph.D. work in Chicago in 1968 to join the finance group in MIT’s Sloan School of Management, Jensen suggested that as Scholes was going to Cambridge he should contact Black (Merton and Scholes 1995, p. 1359).

Black teamed up with Scholes and Jensen to conduct the empirical test of the Capital Asset Pricing Model described in chapter 4. Scholes too became interested in warrant pricing, not via Black but through supervising work on the topic by a master’s-degree candidate at MIT (Scholes 1998). While Black had sought directly to apply the Capital Asset Pricing Model, Scholes took a different tack.

Scholes’s Ph.D. thesis had invoked the capacity of arbitrage to ensure that securities whose risks are alike will offer similar expected returns, and he began to investigate whether similar reasoning could be applied to warrant pricing. In particular, he began to consider the hedged portfolio formed by buying warrants and short-selling the underlying stock (Scholes 1998, p. 480).

The hedged portfolio had been the central idea of Thorp and Kassouf's book *Beat the Market*, which Scholes had not yet read (Scholes interview). Scholes's goal, in any case, was different. Thorp and Kassouf's hedged portfolio was designed to earn high returns with low risk in real markets. Scholes's was a desired theoretical artifact. He wanted a portfolio with a beta of zero: that is, with no correlation with the overall level of the market. If such a portfolio could be created, the Capital Asset Pricing Model implied (as noted above) that it would earn, not high returns, but only the riskless rate of interest. It would thus not be an unduly enticing investment, but knowing the rate of return on the hedged portfolio might solve the problem of warrant pricing.

What Scholes could not work out, however, was how to construct a zero-beta portfolio. He could see that the quantity of shares that had to be sold short must change with time and with changes in the stock price, but he could not see how to determine that quantity. "After working on this concept, off and on, I still couldn't figure out analytically how many shares of stock to sell short to create a zero-beta portfolio." (Scholes 1998, p. 480)

Like Black, Scholes was stymied. Then, in "the summer or early fall of 1969," Scholes told Black of his efforts, and Black described the different approach he had taken, in particular showing Scholes the Taylor-series expansion of the warrant price that echoed his work with Treynor (Scholes 1998, p. 480).

Black and Scholes could see how a zero-beta portfolio would have to be constructed. If the stock price  $x$  changed by the small amount  $\Delta x$ , the warrant or option price  $w$  would alter by  $\Delta x$  multiplied by  $\partial w / \partial x$ , the partial derivative of  $w$  with respect to  $x$  and thus the rate at which the warrant or option price changes as the stock price changes. (This partial derivative—"delta," as it was to become known to the option traders discussed in chapter 6—would vary with time and with stock-price changes, but would be calculable if  $w$  could be expressed as a differentiable function of  $x$  and of the other parameters of the problem.) So the necessary hedge was to short-sell a quantity  $\partial w / \partial x$  of stock for every warrant held. That was in a sense the same conclusion Thorp and Kassouf had arrived at:  $\partial w / \partial x$  is their hedging ratio, expressed in their case as the slope of the empirically derived curves of warrant price plotted against stock price as in figure 5.1.

Black and Scholes's hedging ratio was thus equivalent to Thorp and Kassouf's, but it was embedded in quite a different chain of reasoning. Although the precise basis on which Black and Scholes argued the point evolved as they wrote successive versions of their paper,<sup>12</sup> the crux of their mathematical analysis was that the hedged portfolio must earn the riskless rate of interest. The hedged portfolio was not entirely free from risk, they argued



in August 1970, because the hedging would not be exact if the stock price changed significantly and because the value of an option altered as expiration became closer. The change in value of the hedged portfolio resulting from stock-price movements would, however, depend only on the magnitude of those movements and not on their sign (Black and Scholes 1970a, p. 6).

The risk of the hedged portfolio was, therefore, the kind of risk that could be diversified away. Therefore, according to the Capital Asset Pricing Model, the hedged portfolio could earn only the riskless rate of interest. With the expected return on the hedged portfolio known, simple manipulation led to a finite difference equation that could be transformed into a differential equation (again, equation 1 in appendix D), the Black-Scholes option-pricing equation, as it was soon to be called.

As was noted above, Black had not been able to solve the equation. But now he and Scholes returned to the problem. It was still not obvious how to proceed. Like Black, Scholes was “amazed that the expected rate of return on the underlying stock did not appear” in the equation (Scholes 1998, p. 481). This prompted Black and Scholes to experiment, as Thorp had done, with setting the expected return on the stock as the riskless rate of interest. They substituted this rate for the expected rate of appreciation of the stock price in Sprenkle’s formula for the expected value of the payoff of a warrant at expiration (equation 1 in appendix C). To get the option or warrant price, they then had to discount that expected payoff, in other words to work out its present value. How could they do that?

“Rather suddenly, it came to us,” Black later recalled. “If the stock had an expected return equal to the [riskless] interest rate, so would the option. After all, if all the stock’s risk could be diversified away, so could all the option’s risk. If the beta of the stock were zero, the beta of the option would have to be zero too. . . . The discount rate that would take us from the option’s expected future value to its present value would always be the [riskless] interest rate.” (Black 1989, p. 6) These modifications to Sprenkle’s formula led to a formula for the value of a warrant or call option (equation 2 in appendix D). Instead of facing the difficult task of directly solving their differential equation, Black and Scholes were able to complete their analysis simply by showing that their formula (the Black-Scholes call option formula) was a solution to the differential equation.

### ***Merton***

Black and Scholes’s tinkering with the formula for the expected value of an option’s payoff was in one sense no different from what Boness or Thorp had

done. However, Boness's justification for his choice of expected rate of return on stock was empirical—he chose “the rate of appreciation most consistent with market prices of puts and calls” (Boness 1964, p. 170)—and Thorp freely admits that he “guessed” that the right thing to do was to set the stock's rate of return equal to the riskless rate: it was “guesswork, not proof” (Thorp interview). Black and Scholes, on the other hand, could prove mathematically that their call option formula was a solution to their differential equation, and the latter had a clear theoretical, economic justification.

It was a justification apparently intimately bound up with the Capital Asset Pricing Model. Not only was the model drawn on explicitly in both of the derivations of the equation, but it also made Black's and Scholes's entire mathematical approach seem permissible. Like all others working on the problem in the 1950s and the 1960s (with the exception of Samuelson, McKean, and Merton), Black and Scholes used ordinary calculus (Taylor-series expansion and so on). But they used it in a context in which the stock price was modeled as varying stochastically.

Neither Black nor Scholes knew the mathematical theory needed to do calculus rigorously in a stochastic environment, but the Capital Asset Pricing Model provided an economic justification for what might otherwise have seemed dangerously unrigorous mathematics. “We did not know whether our formulation was exact,” says Scholes, “but intuitively we thought investors could diversify away any residual risk that was left.” (Scholes 1998, p. 483)

As was noted above, Black had been a close colleague of the Capital Asset Pricing Model's co-developer, Jack Treynor. Scholes had done his graduate work at the University of Chicago, where in the 1960s and the early 1970s the model was seen as an exemplary contribution to the field. (The “Chicago” criticism of the model discussed in chapter 4 was a later development.) However, at the other main site of financial economics, MIT, the original version of the Capital Asset Pricing Model was regarded much less positively. The model had been developed using a “mean-variance” view of portfolio selection: investors were modeled as guided only by the expected value or mean of the returns on investments, and by investments' risks as measured by the expected standard deviation or variance of their returns.

Unless the changes in prices of securities followed a joint normal distribution (which was regarded as ruled out, because it would imply, as Samuelson had noted, a non-zero probability of negative prices), mean-variance analysis seemed to rest on a specific form of the “utility function” characterizing the relationship between the return,  $y$ , on an investor's portfolio and his or her preferences. Mean-variance analysis seemed to imply that investors' utility functions were quadratic: that is, they contained only terms in  $y$  and  $y^2$ . Special

cases aside, a rational investor with a utility function whose mathematical form differed from this would not be guided by the expected mean and variance alone.

For Paul Samuelson, the assumption of quadratic utility was over-specific—one of his earliest contributions to economics (1938) had been his “revealed preference” theory, designed to eliminate the non-empirical aspects of utility analysis—and a “bad . . . representation of human behavior.” Quadratic utility implied that beyond a certain level of wealth, increasing levels of wealth meant decreasing utility, so investors would “pay you to cart away their wealth” (Samuelson interview).<sup>13</sup>

Seen from Chicago, Samuelson’s objections to the Capital Asset Pricing Model’s assumption of quadratic utility were “quibbles” (Fama interview) when set against the model’s virtues. “He’s got to remember what Milton Friedman said: ‘Never mind about assumptions. What counts is, how good are the predictions?’” (Miller interview).

However, Samuelson was in sharp disagreement with Friedman’s view that the empirical accuracy of a model’s assumptions was irrelevant. Samuelson’s objections to the mean-variance view of portfolio selection and to the original form of the Capital Asset Pricing Model also weighed heavily with his student Robert C. Merton. The son of the social theorist and sociologist of science Robert K. Merton, he switched in autumn 1967 from graduate work in applied mathematics at the California Institute of Technology to study economics at MIT.

Robert C. Merton had been an amateur investor since the age of 10 or 11, had graduated from stocks to options and warrants, and had come to realize that he had “a much better intuition and ‘feel’ into economic matters than physical ones.” In the spring of 1968, Samuelson appointed the mathematically talented young Merton as his research assistant, even allocating him a desk inside his MIT office (Merton interview; Merton 1998, pp. 15–16).

It was not simply a matter of Merton’s finding the assumptions underpinning the standard Capital Asset Pricing Model “objectionable” (Merton 1970, p. 2). At the center of Merton’s work was the effort to replace simple “one-period” models of that kind with more sophisticated “continuous-time” models. In the latter, not only did the returns on assets vary in a continuous stochastic fashion, but investors were modeled as taking decisions about portfolio selection (and also consumption) continuously, not just at discrete points in time. In any time interval, however short, investors could change the composition of their portfolios.

Compared with “discrete-time” models, “the continuous-time models are mathematically more complex,” says Merton. He quickly became convinced,

however, that “the derived results of the continuous-time models were often more precise and easier to interpret than their discrete-time counterparts” (Merton 1998, pp. 18–19). His “intertemporal” Capital Asset Pricing Model (Merton 1973b), for example, did not necessitate the “quadratic utility” assumption of the original.

With continuous-time stochastic processes at the center of his work, Merton felt the need not just to make ad hoc adjustments to standard calculus but to learn stochastic calculus. It was not yet part of economists’ mathematical repertoire (it was above all Merton who introduced it), but by the late 1960s a number of textbook treatments by mathematicians, including Cox and Miller 1965 and Kushner 1967, had been published. Merton used these to teach himself the subject (Merton interview).

Merton rejected as unsuitable the “symmetrized” formulation of stochastic integration by R. L. Stratonovich (1966): it was easier to use for those with experience only of ordinary calculus, but when applied to prices it in effect allowed investors an illegitimate peek into the future. Instead, Merton chose the original definition of stochastic integration put forward in the 1960s by the mathematician Kiyosi Itô; he also employed Itô’s associated apparatus for handling stochastic differential equations (Stroock and Varadhan 1987).

Among the problems on which Merton worked (with Samuelson—see Samuelson and Merton 1969—and independently) was warrant pricing. The resultant work made up chapters 4 and 5 of his five-chapter Ph.D. thesis (Merton 1970). Black and Scholes read Samuelson and Merton 1969 but did not immediately tell them of the progress they had made. There was “friendly rivalry between the two teams,” says Scholes (1998, p. 483). In the early autumn of 1970, however, Scholes did discuss with Merton his work with Black. Merton immediately appreciated that this work was a “significant ‘break-through’” (Merton 1973a, p. 142), and it was Merton who christened their solution (equation 1 in appendix D below) the “Black-Scholes equation.”

Given Merton’s critical attitude to the Capital Asset Pricing Model, however, it is also not surprising that he also believed that “such an important result deserves a rigorous derivation,” not just the “intuitively appealing” one that Black and Scholes had provided (Merton 1973a, pp. 161–162). In a letter, Merton (1972b) told Fischer Black: “I . . . do not understand your reluctance to accept that the standard form of CAPM just does not work.” “What I sort of argued with them [Black and Scholes],” says Merton, “was, if it depended on the [capital] asset pricing model, why is it when you look at the final formula [equation 1 in appendix D] nothing about risk appears at all? In fact, it’s perfectly consistent with a risk-neutral world.” (Merton interview)

So Merton set to work applying his continuous-time model and Itô calculus to the Black-Scholes hedged portfolio. “I looked at this thing,” says Merton, “and I realized that if you did . . . dynamic trading . . . if you actually [traded] literally continuously, then in fact, yeah, you could get rid of the risk, but not just the systematic risk, all the risk.” Not only did the hedged portfolio have zero beta in the continuous-time limit (Merton had initial doubts on this point but they were assuaged), “but you actually get a zero sigma”—that is, no variance of return on the hedged portfolio (Merton interview).

Because it offers returns that are certain, the hedged portfolio can therefore earn only the riskless rate of interest, “not for the reason of [the capital] asset pricing model but . . . to avoid arbitrage, or money machine”: a way of generating certain profits with no net investment (Merton interview). For Merton, then, the “key to the Black-Scholes analysis” was an assumption Black and Scholes did *not* initially make: continuous trading, the capacity to adjust a portfolio at all times and instantaneously. “Only in the instantaneous limit are the warrant price and stock price perfectly correlated, which is what is required to form the ‘perfect’ hedge.” (Merton 1972a, p. 38)

Black and Scholes were not initially convinced of the correctness of Merton’s approach. Merton’s additional assumption—his world of continuous-time trading—was a radical abstraction, and in a January 1971 draft of their paper on option pricing Black and Scholes even claimed that aspects of the world posited by Merton were incompatible with equilibrium in capital markets (Black and Scholes 1971, pp. 20–21). Despite this disagreement, Black and Scholes used what was essentially Merton’s revised form of their arbitrage-based derivation in the final, published version of their paper (Black and Scholes 1973), though they also presented Black’s original derivation, which drew directly on the Capital Asset Pricing Model.

Black, however, remained ambivalent about Merton’s derivation, telling a 1989 interviewer that he was “still more fond” of the derivation based on the Capital Asset Pricing Model: “There may be reasons why arbitrage is not practical, for example trading costs.” (If trading incurs even tiny transaction costs, continuous adjustment of a portfolio is not feasible.) Merton’s derivation, he said, “is more intellectual[ly] elegant, but it relies on stricter assumptions, so I don’t think it’s really as robust.”<sup>14</sup>

Emanuel Derman, who worked closely with Black at the investment bank Goldman Sachs, recalls Black becoming “quite excited” when a simulation with which Derman was experimenting seemed to show that Merton’s continuously adjusted hedge did not yield precisely the Black-Scholes price of an option. According to Derman, Black “said something like, ‘You know, I always thought there was something wrong with the replication method.’” It turned

out that the discrepancy arose because of error in the simulation, but, says Derman, “in his heart . . . Fischer [Black] mistrusted the Merton derivation and preferred his original proof” (Derman 2004, pp. 170–171).

Black, indeed, came to express doubts even about the central intuition of orthodox financial economics: that modern capital markets were efficient, with prices in them incorporating all available price-relevant information. Efficiency held, he suggested, only in a diluted sense: “. . . we might define an efficient market as one in which price is within a factor of 2 of value.” Black noted that this position was intermediate between that of Merton, who defended the efficient-market hypothesis, and that of the “behavioral” finance theorist Robert Shiller: “Deviations from efficiency seem more significant in my world than in Merton’s, but much less significant in my world than in Shiller’s.” (Black 1986, p. 533; see Merton 1987 and Shiller 1989)

Viewed from a distance, however, the differences between Black, Scholes, and Merton diminished—Black’s doubts about Merton’s derivation were not public—and the common core of their arguments became more salient. The modern textbook derivation of the Black-Scholes equation is essentially Merton’s, and it does not invoke the Capital Asset Pricing Model. (See, for example, Wilmott 1998, pp. 71–74.)

The textbook derivation draws on the assumptions made by Black, Scholes, and Merton to show that an option can be hedged perfectly because a “replicating portfolio” can be constructed. (A “replicating portfolio” is a continuously adjusted position in the stock plus borrowing or lending of cash that exactly mirrors the returns from the option.) A trading position consisting of an option hedged with its replicating portfolio is therefore riskless, so it must earn the riskless rate of interest, for otherwise there is an opportunity for arbitrage: for earning profits with no risk and no capital expenditure. This determines the price of an option exactly. If the price deviates from its Black-Scholes value, arbitrage is possible, and efficient markets do not permit such opportunities to persist.

Because the work by Black, Scholes, and Merton (especially Merton) involved relatively advanced mathematics, the novel conceptual core of the Black-Scholes-Merton model—the way in which option prices are determined by arbitrage, and not, for example, by the expected return on the stock in question—can easily be lost among the symbols. In his 1978 textbook *Investments*, William Sharpe put forward a simplified version of this approach to option pricing. (I draw on it in appendix E.) This model—developed by Sharpe in thinking about how to teach option theory (Sharpe interview)—drops the log-normal random walk in continuous time used by Black, Scholes, and Merton in favor of a “binomial” model that is essentially Regnault’s (see chapter 2).

The mathematical complexities vanish, but the main features of the Black-Scholes-Merton analysis remain.

In Sharpe's binomial model, an option can be hedged perfectly, just as in the Black-Scholes-Merton model. The theoretical price of an option is then determined, as in the Black-Scholes-Merton analysis, by the argument that a perfectly hedged portfolio must earn the riskless rate of interest. If the market price of an option deviates from that theoretical price, arbitrage is possible. Investors' attitudes to risk need not be considered, and there is no need to know the probabilities that the stock will move "up" or "down" in price. It is arbitrage, not those probabilities or investors' beliefs about them, that determines the price of an option.

### ***Option Theory after Black, Scholes, and Merton***

It was not immediately clear that Black, Scholes, and Merton had fundamentally re-conceptualized an important problem. As was noted in chapter 3, the *Journal of Political Economy* originally rejected Black and Scholes's paper because, its editor told Black, option pricing was too specialized a topic to merit publication in a general economic journal (Gordon 1970). The paper was also rejected by the *Review of Economics and Statistics* (Scholes 1998, p. 484).

However, the emerging new breed of financial economists quickly saw the elegance of the Black-Scholes-Merton solution. The expected return on the underlying stock and investors' levels of risk aversion did not appear in the Black-Scholes equation: their arbitrage-based arguments did not require knowledge of those. Herbert Simon's jibe that finance was a field dependent on unobservable variables seemed to have been answered decisively, at least as far as expected returns and risk aversion were concerned.

Nevertheless, not everyone, even in financial economics, was fully convinced of the realism of Black's, Scholes's, and Merton's arguments. Paul Samuelson, for example, had traded warrants as well as studying them, and practical experience had taught him "that there is no such thing as a perfect hedge." For example, the hedge might require a short sale (sale of a borrowed security), but that was legal only "on an up-tick" (that is, after a price rise), and "if the market is down all the time for weeks in a row you're just in a queue" (Samuelson interview).

Samuelson's analysis of options (and McKean's mathematical development of it) had come quite close to the eventually successful Black-Scholes solution. "I'm cautious," says Samuelson, "and sometimes in science to be cautious is to be excessively cautious." (Samuelson interview) "What got Black, Scholes and Merton there," Samuelson recalled in 2000, "was their courage to take a

final step that I was squeamish about: namely, I was loathe to accept the idealization of truly *instantaneous* rebalancings; always, I insisted, for *finite* time intervals, *however small*, no perfect hedge was possible.”<sup>15</sup>

Samuelson’s regret (“close to the North pole is not being there”<sup>16</sup>) indicates, however, his admiration for Black’s, Scholes’s, and Merton’s solution. It was a Kuhnian moment. Like the Modigliani-Miller propositions, Markowitz’s analysis of portfolio selection, the Capital Asset Pricing Model, and the efficient-market hypothesis, but in a sense even more thoroughly so, the work on option pricing by Black, Scholes, and Merton became paradigmatic. By this I do not mean that it formed a disciplinary framework or an all-encompassing way of viewing the world: even “orthodox” finance theory never became quite that homogeneous, and, as Peter Galison (1997) and others have argued, such views of “paradigms” are flawed.

Instead, the work by Black, Scholes, and Merton was a paradigm in a sense described by Kuhn as “philosophically . . . deeper.”<sup>17</sup> It was an exemplary problem solution that could be developed and extended imaginatively. A number of financial economists quickly turned to solving the Black-Scholes equation for options more complicated than their initial “European call” (see appendix D), working out analogues of the Black-Scholes equation for stochastic processes other than the log-normal random walk, analyzing other securities and decision problems that had “option-like” features, and developing models in which the prices of assets other than options were imposed by arbitrage.

For example, in the early 1970s Stephen A. Ross was an economist in his first post at the University of Pennsylvania’s Wharton School of Business, and was finding his research specialty, international trade, “dull.” He began attending seminars in other research specialties, “and the first seminar I ever went to in finance was this fellow called Fischer Black, talking about the Black-Scholes model, and I looked at this stuff and I said ‘this is absolutely wonderful’” (Ross interview).

For Ross, the central feature of Black’s, Scholes’s, and Merton’s work was their argument that the only patterns of prices that could be stable were those that permitted no arbitrage opportunities. He developed an “arbitrage pricing theory” (Ross 1976) that was an alternative to the Capital Asset Pricing Model—the theory is described in an endnote<sup>18</sup>—and he teamed up with John C. Cox, who was just completing a Ph.D. thesis in finance, to work on option theory (Ross interview).

In Cox and Ross’s work, the Black-Scholes result became part of a general, elegant account in which option prices were determined by the absence of arbitrage opportunities, and they showed that the determination of option



prices in this way was equivalent to the principle of “risk-neutral valuation” described in appendix E (Cox and Ross 1976). For Ross, models of stock prices and option theory were not separate endeavors: “. . . all this is the same thing. One price. No arbitrage.” (Ross interview) Assets that were substitutes for each other—stocks with the same sensitivity to underlying risk factors; an option and its replicating portfolio—had to have the same price, for otherwise there was an arbitrage opportunity, and “all arbitrage opportunities will be exhausted in markets with open access” (Ross 1978, p. 454).

One particular product of the work of Cox and Ross was to take on a special practical significance, as we shall see in chapter 7. This arose from a collaboration between them and Mark Rubinstein, a finance academic at the University of California at Berkeley who had particular expertise in the applications of computing to finance. The Cox-Ross-Rubinstein model (sketched in appendix E) was an attractive practical alternative to Black-Scholes. It was especially suited to implementation on a digital computer, and if the computer was powerful enough the Cox-Ross-Rubinstein model allowed option-pricing problems that were difficult or impossible to solve by analytical techniques to be solved numerically.

The basic thrust of the work of Cox and Ross, however, was to move to greater mathematical generality in the analysis of option pricing. That was an even more dominant feature of the final piece of theoretical work to be considered in this chapter. J. Michael Harrison was an operations researcher (and essentially an applied mathematician) at Stanford University, and his colleague David M. Kreps was a mathematically sophisticated economist. To Harrison, none of the work in option-pricing theory done before the mid 1970s was sufficiently rigorous.

Harrison and Kreps asked themselves “Is there a Black-Scholes theorem?” From the viewpoint of the “theorem-proof culture . . . I [Harrison] was immersed in” (Harrison interview) there was not.<sup>19</sup> So they set to work to formulate and to prove such a theorem, a process that eventually brought to bear modern “Strasbourg” martingale theory, an advanced and previously a rather “pure” area of probability theory.<sup>20</sup> (See the glossary for the meaning of “martingale.”)

Harrison and Kreps showed that in a “frictionless” market with no opportunities for arbitrage there existed an “equivalent martingale measure,” a way of assigning probabilities to the path followed by the price of an asset such that the value of an option or other derivative contract on that asset was simply the expected value of its payoff discounted back to the present. (The “martingale probabilities” of appendix E are an example.) If the market is complete—in other words, if the securities that are traded “span” all possible

outcomes, allowing all contingent claims (contracts whose payoffs depend on those outcomes) to be hedged or insured against—then the equivalent martingale measure is unique (Harrison and Kreps 1979; Harrison and Pliska 1981).

Harrison and Kreps's 1979 paper applying modern martingale theory to financial markets was both an end and a beginning. By 1979, the basic structure of "orthodox" modern finance theory was essentially complete. At the start of the 1960s, Eugene Fama had felt like "a kid in a candy store." A decade later, Stephen Ross still felt the same sense of intellectual opportunity: "It was a good time. You just knew you would pick up nuggets of gold." (Ross interview)

After 1979 the intellectual opportunities for financial economists seemed sparser. Much important work in finance theory was done after 1979: it was more than the "mopping-up operation" that Duffie (1996, pp. xiii–xiv) contrasts with the pre-1979 "golden age" of asset-pricing theory. Nevertheless, what had come to an end was a period in which it sometimes seemed as if all one had to be was empirically skilled or theoretically talented in order to make a fundamental contribution. In that sense, to work on the economics of finance was harder after 1979. However, "the martingale stuff brought the hyper-mathematicians in" (Ross interview). Building on the work done by Black, Scholes, and Merton, other theorists—Sharpe, Cox, Ross, Rubinstein, and especially Harrison and Kreps—had constructed a bridge between financial economics and martingale theory, an advanced, high-prestige area of pure mathematics. Things that probability theorists within pure mathematics knew about—measure-theoretic notions of probability, the Radon-Nikodym theorem, the martingale representation theorem, the martingale convergence theorem, the Girsanov theorem—became relevant to finance. "There's this whole literature on martingales and stochastic processes so people doing that suddenly said 'yeah, I can get a job on Wall Street.' That's what brought them all into it." (Ross interview) By the 1990s, it had become possible for probability theorists to teach courses that were both rigorous by the standards of pure mathematics and attractive to students looking forward to well-paid jobs in finance.<sup>21</sup>

For all the diversity and elaboration of option theory after 1973, the work of Black, Scholes, and Merton remained paradigmatic. None of what followed contradicted their approach; it was all consistent with it. For example, the Black-Scholes-Merton model is a limit case of the Cox-Ross-Rubinstein model. (See appendix E.) Plug the log-normal random walk and the specific characteristics of an option contract into the Harrison-Kreps martingale model and the results generated are those of the Black-Scholes-Merton model.

The Black-Scholes-Merton model was not transcended, but supplemented. It was brilliant, productive theoretical work. But was it empirically valid? After constructing their formula for the pricing of call options (equation 2 in appendix D), Black and Scholes tested its empirical validity for the ad hoc New York options market, using a broker's diaries in which were "recorded all option contracts written for his customers." They found only an approximate fit: "in general writers [the sellers of options] obtain favorable prices, and . . . there tends to be a systematic mispricing of options as a function of the variance of returns of the stock" (Black and Scholes 1972, pp. 402, 413).

It is not surprising that the predictions of the Black-Scholes-Merton model should have corresponded to reality only roughly. In its mathematical assumptions, the model embodied a world, so to speak. (From this viewpoint, the differences between the Black-Scholes world and Merton's world are less important than their commonalities.)

In their paper on the theory of option pricing, Black and Scholes spelled out their assumptions. These included not just the basic assumption that the "stock price follows a [log-normal] random walk in continuous time," but also assumptions about market conditions: that there are "no transaction costs in buying or selling the stock or the option"; that it is "possible to borrow any fraction of the price of a security to buy it or to hold it," at the riskless rate of interest; and that there are "no penalties to short selling" (Black and Scholes 1973, p. 640).

In the 1960s and at the start of the 1970s, these assumptions about market conditions were wildly unrealistic. Commissions (an important transaction cost) were high everywhere. Investors could not purchase stock entirely on credit (in the United States this was banned by the Federal Reserve's "Regulation T"), and such loans would be at a rate of interest in excess of the riskless rate. Short-selling was legally constrained and financially penalized: stock lenders retained the proceeds of a short sale as collateral for the loan, and refused to pass on all (or sometimes any) of the interest earned on those proceeds (Thorp interview).

In these and other respects, however, the world of the early 1970s was starting to change. Even before it had altered much, Black-Scholes-Merton option theory began to affect the fortunes of options exchanges and even the patterns of prices on them. This—perhaps the strongest form of performativity treated in this book—is the topic of chapter 6.

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## *Pits, Bodies, and Theorems*

That Chicago should be in the vanguard of the changes that swept the world's financial markets in the 1970s and the 1980s was, on the face of it, surprising. The postwar decades were not always kind to the second city of the United States, a city that could sometimes seem to epitomize the troubles of an urban world in the throes of deindustrialization.<sup>1</sup>

Agriculture thrived on the Midwest's endless prairies, but the exchanges that traded futures on the produce of those prairies—the Chicago Board of Trade and the Chicago Mercantile Exchange—did not always benefit. Government guarantees set minimum prices for agricultural products, while surpluses prevented prices from rising much. The need to hedge the risk of price fluctuations diminished, and speculation on those fluctuations became unattractive.

By 1968, traders were “sitting on the steps of the [soy] bean pit [of the Board of Trade] . . . reading newspapers” (Sullivan interview) because trade was so slow. In 1967, with the government shaping the markets, the Board of Trade chose a Washington insider, former presidential aide Henry Hall Wilson, as its president. Wilson in turn hired Joseph W. Sullivan, a *Wall Street Journal* political correspondent, as his assistant. Sullivan began to explore the feasibility of futures on commodities such as plywood, scrap steel, and fishmeal (Sullivan interview).

At the Merc, Leo Melamed had seen the trade in egg futures collapse, and onion futures were banned by Congress in August 1958 after one of the periodic scandals that rocked the commodities futures markets. In 1961, the Merc launched a futures contract on frozen pork bellies, and other futures on meat followed. They formed a viable market—one that was later to come to wider attention when Hillary Rodham Clinton's success in the 1970s speculating in the Merc's cattle futures was scrutinized<sup>2</sup>—but Melamed knew that “a one-product line was a very dangerous way for an exchange to live.” As he rose to leadership of the Merc, he explored the possibility of futures on other commodities such as shrimp, potatoes, apples, and turkeys (Melamed interview).

Plywood and shrimp did not, however, seem to guarantee a genuine revival of the Board of Trade and the Merc, and in the late 1960s a more radical departure began to be canvassed: financial futures. They could serve the same interlocked interests in hedging and speculation as agricultural futures did, but their pursuit encountered barriers that were essentially moral, like the earlier barriers to life insurance (Zelizer 1979).

Stock options and futures were integral to nineteenth-century exchanges (Weber 2000a,b), but the 1929 crash and the subsequent Great Depression reignited hostility to “speculation” in derivatives that looked like wagers on price movements. Even in the 1960s, market regulators such as the Securities and Exchange Commission (founded in response to the excesses and abuses of the 1920s) remained deeply suspicious of derivatives.

The most attractive foundation for a financial derivatives exchange was a futures contract on a stock-market index such as the Dow Jones industrial average or Standard and Poor’s 500. Every day, Melamed used to have coffee with his fellow Merc member, Elmer Falker, “an elderly, cigar-chomping bachelor just under five feet tall, still wearing spats and driving to and from work in his spiffy 1932 Franklin” (Melamed and Tamarkin 1996, p. 100).

Falker was a chartist who had “lost all his money waiting for a gap to be filled on a butter chart,” but he was also the Merc’s philosopher. (In 1987, Melamed was to remember Falker’s warning: “Don’t let our futures market get too successful. . . . Because futures markets tell the truth and nobody wants to know the truth. If the truth is too bad and too loud, they’ll close us down.”)<sup>3</sup> One day, when Melamed was chatting to Falker about his ideas for new futures contracts, Falker told him “Well the ultimate contract, of course, is Dow Jones futures.” (Melamed interview)

The idea of a Dow Jones future also struck Sullivan and his colleagues as a possible salvation for the Board of Trade. However, the idea fell foul of a long-standing cultural, legal, and political problem. Futures trading had always been contentious—farmers, for example, often blamed speculators on futures exchanges for the low price of their produce—and a crucial plank of its opponents’ efforts to ban it was the argument that futures contracts were wagers. If its critics could succeed in making this identification, they would push futures trading outside the law, since gambling was illegal in most of the United States.

For much of the late nineteenth century and the early twentieth century, courts in the United States had struggled to find a clear distinction between legitimate futures trading and gambling. Their efforts were complicated and sometimes contradictory (Swan 2000). However, a test that commanded wide agreement was whether or not a futures contract could be settled by physical delivery of the underlying commodity, for example grain.

If a futures contract could be settled by delivery of the underlying asset, courts generally ruled that it was legal and enforceable. If it could not—if the contract could be settled only in cash—it was in general held to constitute a wager.<sup>4</sup> Hence, as noted in chapter 1, even if delivery was seldom demanded, it had to be possible. If it was not, a futures contract was against the law of Illinois and of any other state in which gambling was prohibited.

Since an index was an abstraction, there was no straightforward way in which an index future could be settled other than in cash. In 1968, Sullivan and two leading members of the Chicago Board of Trade consulted securities lawyer Milton Cohen about the feasibility of a Dow Jones futures contract. Cohen advised against proceeding: the contract would violate Illinois law (Falloon 1998, pp. 209–210).

Melamed, however, had a different idea: futures on currencies. “Currencies had been . . . really dear to my heart,” he says (Melamed interview). He remembered his father explaining to him the differences between the currencies of the countries they traveled through: Poland, Lithuania, Russia, Japan, and the United States. The thousands of often desperately poor Jewish refugees in Japan had been kept from destitution by a currency arbitrage. Japanese citizens were forbidden from owning any foreign currency, and in consequence a black market existed, particularly in dollars.

The Jewish Labor Committee would open bank accounts for refugees in Japan such as Melamed’s family, and deposit yen in them. When refugees received a U.S. visa, they would take the visa to the bank, and they were then allowed to convert the yen in their account to dollars at the official rate. They would take these dollars on board the ship taking them from Japan (Melamed still remembers the “little briefcase” in which his father did this) and register them with the ship’s purser to prove they were not penniless. After doing so, they would then return the dollars to a representative of the Labor Committee, who would take them back onshore and convert them back to yen at the much more favorable black-market rate (Melamed interview).

The currencies of the 1960s did not, however, seem a promising basis for a futures market. Central to the governance of the postwar economic order was an agreement hammered out in 1944 between Britain’s John Maynard Keynes and U.S. negotiator Harry Dexter White in the Mount Washington Hotel at Bretton Woods in New Hampshire’s White Mountains. The Bretton Woods Agreement promoted free trade in goods, but sought to eliminate competitive currency devaluations and to keep exchange rates stable. The exchange rates between the U.S. dollar and other major currencies were fixed, with fluctuations of no more than 1 percent around those rates permitted, while the value

of the dollar itself was anchored by the United States committing itself to sell gold to other governments at a fixed dollar price.

With exchange rates fluctuating only in very limited bands, trading currency futures would be unattractive: there would be no need to hedge against adverse fluctuations, and little point in speculating. To Melamed, however, the University of Chicago free-market economist Milton Friedman was a “personal hero.” Though never a student at the University of Chicago, Melamed used to “sneak in” to listen to Friedman’s lectures (Melamed interview).

The 1953 book that contained Friedman’s essay on “The Methodology of Positive Economics” also contained a chapter titled “The Case for Flexible Exchange Rates.” In it, Friedman argued that “current economic and political conditions” demanded “a system of flexible or floating exchange rates—exchange rates freely determined in an open market primarily by private dealings and, like other market prices, varying from day to day.” Such a regime, he claimed, was “absolutely essential for . . . the achievement and maintenance of a free and prosperous world community engaging in unrestricted multilateral trade” (Friedman 1953b, p. 157).

Friedman was convinced that the Bretton Woods Agreement would unravel, and in the late 1960s there was increasing evidence that this might be so. Were that to happen, a futures market in currencies might be viable. Melamed, who had trained and practiced as a lawyer, believed that such a market could pass the legal test that a genuine futures contract should “contemplate delivery.”

If, for example, Melamed had sold Deutschmark futures contracts, he could then be called upon to “deliver to you Deutschmarks where you want them. You want them in Frankfurt, in your bank, in your bank account. I will then arrange, with my bank here in the United States, to . . . deliver to you in Frankfurt, in your account, in Deutschmarks. So there is no real cash settlement. There’s a delivery process that goes on.” (Melamed interview)

The idea of a currency futures exchange began to obsess Melamed. “I became fanatic on this idea. It wouldn’t let me rest. I mean, I was chairman of the [Mercantile] Exchange. I could literally do this. It wasn’t some dream. I could make this happen.” (Melamed interview)

With index futures blocked, the Board of Trade also began to look for an alternative, but it took a different path: stock options. They were legal: there was an underlying asset that could be delivered, stock certificates, and in New York a small ad hoc market (not an organized exchange) already existed. Among the contributors in the 1960s to the academic literature on options discussed in chapter 5 were the Princeton economists Burton Malkiel and Richard Quandt. They argued that options’ reputation as “the ‘black sheep’ of the

securities field” was undeserved. Their use was “a very desirable strategy for most investors” and “wholly rational,” although the ad hoc New York options market was “relatively inefficient” (Malkiel and Quandt 1969, pp. 6, 163, 165, and 167).

It seems to have been reading about Malkiel and Quandt’s favorable economic analysis of options in an article in the magazine *Institutional Investor* (Malkiel and Quandt 1968) that drew the attention of a leading member of the Board of Trade: the grain trader Edmund O’Connor. Options were desirable but traded only ad hoc. Might the Board not profitably standardize them and trade them in a busy, efficient Chicago pit?

### ***Economics and the Legitimacy of Derivatives Trading***

By the end of the 1960s, the Mercantile Exchange and the Board of Trade thus both had plans to begin trading financial derivatives. Both knew, however, that there was a gap between an idea for a market and a viable reality. Simply launching a financial derivatives contract without attracting customers and getting at least the implicit blessing of the authorities was unlikely to be a path to success. The New York International Commerce Exchange launched a currency futures market in 1970, but it foundered.<sup>5</sup>

“I didn’t have . . . the credentials,” says Melamed. “I was a lawyer and a chairman of a secondary exchange that . . . wasn’t even a distant cousin to a legitimate financial . . . institution and here I was thinking about . . . currency. I needed the stamp of authenticity from someone that counts.” To Melamed, the choice of “stamp of authenticity” was obvious: his “personal hero,” Milton Friedman, whom he had at that point never met (Melamed interview). Melamed and the president of the Mercantile Exchange, E. B. Harris, arranged to talk with Friedman over dinner in the art deco splendor of the Waldorf Astoria Hotel on New York’s Park Avenue.

Friedman was instantly enthusiastic: “He said, ‘That’s a terrific idea. It’s a wonderful idea. You must do this.’” (Melamed interview) Melamed asked “if I [Friedman] would be willing to write a little paper for them on the case” for a currency futures exchange (Friedman interview). Friedman replied “‘I’m a capitalist first,’ and I [Melamed] said ‘How much?’ I immediately knew what he meant and he liked that. He liked that. He said ‘\$5000.’ I said, ‘It’s done.’ Just like that.” (Melamed interview)

“Bretton Woods is now dead,” wrote Friedman in a December 1971 paper, “The Need for Futures Markets in Currencies,” commissioned by the Merc.<sup>6</sup> Huge outflows of capital from the United States had forced President Nixon



to close the “gold window” on August 15, 1971, ending the fixed-rate convertibility of dollars into gold. In response, Germany and Japan had been forced to allow their currencies to float.

As Friedman was preparing his paper, desperate efforts were being made at government level to reconstruct a system of fixed exchange rates, but it was already clear that even if that was successful the allowable bands of fluctuation would be wider than 1 percent permitted under Bretton Woods. Those involved in foreign trade would, therefore, be exposed to far greater risks of currency fluctuation than they had been in the 1950s and the 1960s. Bringing hedgers and speculators together in a currency futures market would make that market large and liquid, making it possible “to hedge at low costs and at market prices that move only gradually and are not significantly affected by even large commercial transactions” (Friedman 1971, p. 5).

On May 16, 1972, the Merc’s International Monetary Market began trading futures on seven currencies (Tamarkin 1993, p. 200). The Merc’s lawyers had advised that it could go ahead without government approval, but Melamed told the lawyers: “I don’t want to do this unless, at least, the powers that be have consented, if not consented at least acquiesced, and if not acquiesced then at least had been given notice so that they don’t do an injunction or I don’t have a legal battle about it.” So Melamed started to make appointments with the appropriate decision makers in Washington, sending them Friedman’s paper in advance (Melamed interview).

Arthur Burns, who chaired the Board of Governors of the Federal Reserve, had been Friedman’s mentor in economics and one of the three economists whom Friedman thanked for their feedback on “The Methodology of Positive Economics” (Friedman 1953a, p. 3). Burns did not oppose the proposal. The critical meeting, however, was with Secretary of the Treasury George P. Shultz. As he and Harris waited outside Shultz’s office, Melamed felt like “the immigrant kid from Poland” (Tamarkin 1993, p. 186).

Shultz, though, had been a colleague of Friedman: he had been a professor in and then dean of the University of Chicago’s Graduate School of Business. “If it’s good enough for Milton,” he told Melamed and Harris, “it’s good enough for me.” (Melamed interview) The currency futures market was launched, and the authorities would not oppose it.

The Chicago Board of Trade faced deeper difficulties. To trade stock options needed not just passive acquiescence but explicit consent from the SEC, and that proved extremely hard to obtain. In the mid 1960s, the term “go-go”—with its connotation of uninhibited, erotic dancing—was transferred to the stock market to describe “rapid in-and-out trading of huge blocks of stock, with an eye to large profits taken very quickly” (Brooks 1973, p. 128).

With even mutual funds engaging in go-go trading in the 1960s, the SEC became increasingly alarmed. The “new fashion contains a serious potential danger to continued public confidence in the securities market,” warned SEC Chairman Manuel F. Cohen in 1968. “The parallels to the problems created by speculative activities of pools and syndicates in the 1920’s and 1930’s are obvious.”<sup>7</sup>

Options aroused particular suspicion. As the Board of Trade began to float the idea of options trading with the SEC in the late 1960s, it encountered what it took to be instinctual hostility, based in part on corporate memory of the role options had played in the speculation and malpractices of the 1920s. Sullivan, for example, was told by one leading SEC official that he had “never seen a [market] manipulation” in which options were not involved. When the Board of Trade invited Manuel Cohen and one of his officials to a meeting with Wilson and Sullivan in the Democratic Club, the official told them that there were “absolutely insurmountable obstacles” to their proposal, and they “shouldn’t waste another nickel pursuing it.” He even compared options to “marijuana and Thalidomide” (Sullivan interview).

Like the Merc, the Board of Trade sought legitimacy from economics. In 1969, it sought an assessment of the proposal for an options exchange from a leading economic consultancy firm, Nathan Associates, with which the Board would have had personal contact because the firm had studied the grain futures market for the Department of Agriculture. While working for the U.S. Department of Commerce in the 1930s and the 1940s, the economist Robert Nathan (1908–2001) had, along with Simon Kuznets, developed the modern framework of “national accounts” for the United States, including the crucial measure, “Gross National Product.” The consultancy firm that Nathan set up in 1946 quickly became prominent. After the Korean War, for example, Nathan Associates drew up on behalf of the United Nations the plans for economic reconstruction in Korea.<sup>8</sup>

For its report on options, Nathan Associates turned for assistance to the burgeoning new field of financial economics: MIT’s Paul Cootner, the University of Chicago’s James Lorie and Merton Miller, and, especially, the Princeton economists whose work on options had sparked the initial interest at the Board of Trade. Malkiel, Quandt, and their colleague William Baumol provided Nathan Associates with an analysis of the impact of an options exchange on “the public interest.”

In their contribution to the Nathan Report, Baumol, Malkiel, and Quandt argued that options “enrich the investor’s repertoire of strategies by allowing him to realize a different set of payoffs than he would have realized in their absence.” Just as the possibility of carrying an umbrella was an advantage to

the pedestrian, “the more strategies are available to the investor, the better off he is likely to be” (Nathan Associates 1969, vol. 2, pp. 14, 20).<sup>9</sup>

The Nathan Report, in turn, made it possible for the proposal for an options exchange to gain its single most crucial recruit, Milton Cohen. The Chicago lawyer had originally not wished to be associated with the proposal, but the report made it “legitimate enough” that he agreed to become Special Counsel to the Board of Trade and to lead its negotiations with the SEC (Sullivan interview). Cohen had been a senior official at the SEC and was arguably the pre-eminent securities lawyer in the United States. No one was better placed to “make a record” with the SEC: putting proposals and getting responses.

Even with Cohen’s support, the proposal to trade options made slow progress. After two years, the record of Cohen’s exchanges with the SEC formed a stack of documents four feet high (Sullivan interview), but with no approval forthcoming. However, the Nixon administration was changing the climate in Washington. In 1971, Nixon appointed William Casey—a venture capitalist and a tax lawyer—to chair the SEC.

Casey’s attitude to “speculation” was quite different from that of his predecessor Manuel Cohen, and he held Milton Cohen in high regard, trying to lure him back from private practice to become his “personal mentor” (Sullivan interview; Rissman interview). Casey’s respect for Milton Cohen meant that the latter was able to secure a meeting with him to go through the arguments in favor of an organized options exchange. Although members of his staff were still skeptical, Casey was convinced, saying to Cohen: “Tell me what kind of order you need from the Commission to get started.” (Milton Cohen, in CBOE/OCC 1998b) On April 26, 1973, the Chicago Board Options Exchange opened.

### ***Culture, Community, and Collective Action***

The time-consuming, expensive lobbying, planning, and preparatory work that established the Chicago Board Options Exchange had many of the characteristics of collective action. Its parent, the Board of Trade, was not a hierarchical corporation; it was a membership organization that elected its officials and voted on important decisions.

Board employees were paid for their work, but expenses were ultimately born by the members of the Board as a whole, and some members—Edmund O’Connor (the original proponent of options), Irwin “Corky” Eisen (who chaired the subcommittee that designed the trading-floor procedures for the new options exchange), and others such as David Goldberg, Patrick Hennessy, and Paul McGuire—took on substantial unremunerated commitments, which

continued after the options exchange opened. O'Connor, Eisen, and Goldberg, for example, would sometimes lend newcomers the money (initially \$10,000, but soon more) to buy a membership, with no certainty that the recipient would succeed and be able to pay them back.

On the Mercantile Exchange, similarly, Leo Melamed devoted large amounts of time he could otherwise have spent on profitable trading to leading its move into financial derivatives. He continued to trade in the pits (part of his authority came from the fact that he led the Merc from there), but his duties chairing the Merc often kept him from paying full attention to trading, and he suffered losses.

However, Melamed rejected all proposals to pay him until the need to respond to the crisis of 1987 made clear that “the demand on my time was such that I could no longer trade effectively” (Melamed and Tamarkin 1996, p. 281). Furthermore, although the Merc’s financial derivatives trading became profitable and self-sustaining, there were—as discussed below—periods in which Melamed had to encourage the Merc’s members to take part in it even if their profits were likely to be low. He could not instruct them to do so: he was their elected representative, not their employer. All he could do was to seek to persuade them to act for the collective good of the exchange.

As Olson (1980) famously argued, collective action cannot satisfactorily be explained simply by the fact that it was in the collective interest of all those involved, in this case the memberships of the Board of Trade and the Merc. *All* their members benefited, even those who stood aside from it entirely. In such a situation, rational egoists—the individuals posited in much of orthodox economic theory—will free ride, leaving it to others to bear the costs of collective action, which will therefore not take place, even if it would foster the interests of all involved. Hence the possibility of a delightful paradox: the very markets in which *Homo economicus*, the rational egoist, appears to thrive cannot be created (if they require the solution of collective action problems, as in Chicago) by *Homines economici*.

Certainly, the accounts of their motivation provided by central actors in the move to financial derivatives are not of rational egoism. Leo Melamed’s admiration for Friedman sat alongside a political inheritance of quite a different sort. When I asked him why he devoted effort to collective projects, Melamed cited the influence of his father, a member of the Jewish revolutionary-socialist Bund (Melamed interview). Even as a child, Melamed recalls, he was “able to sense the intense feelings [the Bund] generated” in his parents (Melamed and Tamarkin 1996, p. 22). “I will never forget,” he says, “being wedged between them standing rigidly erect at Bund meetings in Bialystok that opened with the *Shvue*, the Bund’s anthem—an oath of allegiance. My mother

held my hand tightly as she and my father sang, and I could feel the emotional choking that gripped both of them as they fervently swore never to forsake the battle on behalf of the working class. There among the mass of people with the decibels ringing in my ears, I knew something was going on, something big, something awe-inspiring, something eternal.”

Had it not been for the escape route offered by Sugihara’s letter of transit, Melamed’s parents would have joined the Bundist partisans in the forests of Lithuania in armed resistance: it was, for example, members of the Bund who led the heroic, doomed uprising of the Warsaw ghetto in April 1943. Melamed disavowed his parents’ socialism—“Adam Smith taught me that you serve society best by caring for yourself” (Melamed interview)—but he never abandoned their sense of the moral demands of the collective.

“My father had instilled in me [the] idea that you gain immortality by tying yourself up with an idea, or a movement, or an institution that transcends mortality” (Melamed interview). The very name of the world’s first successful modern financial derivatives exchange, the Merc’s “International Monetary Market,” was a private homage to a socialist. It shared its initials with Melamed’s father, Isaac Moishe Melamdovich.

The Bundist lineage of collection action was specific to Melamed. Others, though, acted in analogous ways for reasons that were not wholly dissimilar. “We . . . never thought of even asking for reimbursement [of expenses involved in creating the options exchange],” says Corky Eisen of the Board of Trade. “This was part of the concept that was inculcated into all of us: ‘You owe it to your community.’ We had all done very nicely, thank you . . . and we felt that we had an obligation to the exchange and this is how you pay your obligations.” (Eisen, interviewed by Millo<sup>10</sup>)

Of course, avowals of altruism sometimes mask self-interest, but for a rational egoist to embark on a project like the creation of the International Monetary Market or the Chicago Board Options Exchange, in a context in which all others are rational egoists, is implausible. A risk-neutral egoist would need to be confident that the expected personal benefits of the creation of the new markets exceeded the total costs of establishing them (others must be expected to free-ride, so the founder must expect to bear all these costs); for a risk-averse egoist, the excess might have to be considerable. Given the considerable *ex ante* uncertainty whether the new markets would prosper—many members of the Board of Trade opposed the options exchange as a likely waste of money—these conditions are unlikely, and my interviews give no suggestion that they were met.

The interviews do, however, provide evidence that the Chicago exchanges provided the kind of context that the extensive experimental evidence that has

accumulated since Olson's work suggests promotes the solution of collective action problems: a context of ample face-to-face interaction and one that fosters "conditional co-operators" and "willing punishers" (see, for example, Ostrom 2000). Although the exchanges had and have large memberships (ranging from around 500 to 3,500), they are not anonymous places. Like many exchanges, they have some of the characteristics of what the sociologist Max Weber called "status groups" (Weber 2000a,b). The division between insiders and outsiders is strict. One can trade on exchange floors only by purchasing or leasing a membership (the number of which is strictly limited), and memberships of the Board of Trade and Merc were often passed from father to son.

In an "open outcry" exchange such as those in Chicago, contracts involving large sums are entered into on the basis of verbal agreement or hand signals and eye contact. Although there are systems for the written, and now computerized, recording of contracts, each participant enters details separately, when prices may move substantially in seconds. Misunderstandings and mistakes ("outtrades") are not unusual, and opportunism is obviously possible.

Outtrades often have to be settled on a rough-justice, split-the-difference basis: to discover what "really" has been agreed in the tumult of a pit may be impossible. Widespread opportunism would render open-outcry trading infeasible, but a simple sanction is available: refusal to trade further with an offender. At any given time, several traders will usually be offering the same prices, and while formal rules require trading with the first "heard" or "seen," participants do in practice have a degree of discretion as regards whom they "hear" or "see."<sup>11</sup>

Trading-floor interaction often spills over into off-floor socializing and elaborate membership committee structures. One should not idealize: antagonistic encounters on trading floors are common, and sometimes physical fights break out; exchange politics is sometimes bitterly divided. The occasional intensity of hostile interaction, however, points to the importance of *interaction*. Day after day, year after year, members of open-outcry exchanges trade with each other face to face. They have the incentive to monitor each other's conduct, and (because so much of this conduct occurs in a public arena) have the capacity to do so closely. Infractions are remembered, sometimes for decades. The result is a moral economy as well as a financial one.

Thus William R. Power joined the Chicago Board Options Exchange after having been a trader in New York. "I was an over-the-counter dealer doing business over the telephone rather than face to face as on the exchanges. My experience in that market was that no one helped anyone." When Power moved to Chicago he was "shocked and delighted" to discover people who

would help him, “including Ed O’Connor (my first ‘godfather’) and Corky Eisen (my second).”<sup>12</sup>

Gradually, Power came to see that a sense of morality was to be found in Chicago’s apparently pell-mell, egoistic trading pits: “This [Chicago] is a place where people think very simple in terms of people and markets. Black. White. Good. Bad. There’s an invisible sheet with an invisible line down the middle of it. This is a good guy. This is not a good guy. Nobody’s on that line. They’re either a good guy or a bad guy. Very long memories.” (Power interview)

Actions that are seen as unduly opportunistic are punished by “freezing out,” and actions that are perceived as in the common good are rewarded with “respect” (Melamed interview). Generosity gives rise to obligation, sometimes between generations rather than in the form of mutual reciprocity. “Corky” Eisen, Power’s second “godfather,” reports:

I . . . came to the Board of Trade as a clerk at the age of twenty. . . . One of my mentors was an Irishman by the name of Jim McKerr. . . . He brought me in as a clerk, financed me, loaned me . . . the money to buy the membership. . . . When I came back from the service (Korea), things were tough then. I went to work for another Irishman, Bill Haggerty senior. . . . They were wonderful to me and when I got my membership back he wanted me to clear through him. I told him “. . . I have an obligation to Jim McKerr, and I’m going to clear and be his customer, not yours.” I expected him to say “That’s very nice, here’s the door, goodbye.” Instead he said “I can understand that. As a matter of fact, I respect you for it, your job here is secured.” . . . So I remained McKerr’s customer. I felt obligated. In about two years I developed into a pretty good trader . . . but I realized the only way I could survive and compete was to become a [clearing] member. I still didn’t have any money because in those two years I was paying lots of debts. I was now even. I went to Jim McKerr (that was in 1956) and said “I’m going to start clearing.” He said “That’s wonderful . . . but what are you going to do for money?” . . . My share was \$15,000 in order to start a clearing firm and Jim said “Okay” and reached in his pocket and wrote me a check for \$15,000. I didn’t even ask. I said “How in the world can I ever thank you?” And he said “You have a debt, but the debt is to youngsters that come on after you. You can repay me by helping other kids.” . . . Many years later, when the Options Exchange was in existence, I was a clearing member and I was taking a lot of young floor traders as customers. One day Jim McKerr came visiting from Florida . . . he was 80 years old. . . . One of the kids jumped and gave him a big hug. Jim looked at him—he was rather reserved—and said “What’s that for?” And the guy said “Mr. McKerr, I owe my career to you. Whenever I came to Corky [Eisen] to thank him, he told me about you and he said that he was returning your help.” When I took him down to lunch there must have been twenty people who shook his hand, people he had never seen or heard of. But this was his legacy and we have passed it on. (Eisen, interviewed by Millo)

“Respect” and “obligation,” in their turn, are resources for moral suasion. The need for collective action did not cease once financial derivatives began to be traded. Members of the Board of Trade and of the Merc had to be persuaded to spend some time, and devote some money, to trading the new

products, even though they might not at first offer the same opportunities as more familiar commodities: as the new markets were established, trade in agricultural futures was reviving, for example with the beginning of grain sales to the Soviet Union.

On the Chicago Board Options Exchange's first morning of trading, its failure seemed an alarming possibility. "There were two traders on the floor," recalls Eisen. "Eddie [O'Connor] and I were running all over the floor making markets." They returned to the floor of the Board of Trade: "we went around and we convinced everybody to come in at least when grains got a little slow, around 11.15, 11.30 . . . and then they'd go back and trade the close in grains." (Eisen in CBOE/OCC 1998b)

Similarly, the success of the Merc's International Monetary Market initially seemed precarious. "Once the novelty wore off, the market liquidity completely dried up. . . . For most of the day . . . we just sat around playing chess and backgammon." (Randy McKay, quoted in Schwager 1992, p. 82)

Like Eisen and O'Connor, though, Melamed was able to exercise his influence. "I became an obsessed one-man enforcer," he says, "coercing, cajoling, admonishing, pleading with our . . . members to trade the currency markets. We needed liquidity, I begged. Everyone had to lend a hand. And for the most part, the floor responded to my pleas. These were, after all, my guys." (Melamed and Tamarkin 1996, p. 198)

When the Merc launched the S&P 500 index futures discussed below, Melamed was similarly active in pressuring and persuading its members to trade the new product. Other traders would show him their time-stamped trading tickets to demonstrate that they had done the 15 minutes of trading per day on the nascent market that Melamed demanded. They would be "ashamed not to" do this minimum for the collective good, says Melamed (interview).

Nor did the social structure of the Chicago exchanges cease to matter once the International Monetary Market and Chicago Board Options Exchange became large and successful (by 1978, more than 100,000 options contracts, each corresponding to 100 shares, were traded on an average day on the latter).<sup>13</sup> Baker (1981; 1984a,b) examined the pattern of trading and the behavior of prices in two options trading "crowds," one large and one small. After taking account of the volatility of the underlying stocks, Baker found that contrary to standard economic predictions, option prices were more volatile in the larger crowd, an effect he explained by the tendency of this crowd to fragment into subnetworks when trading was intense.

"In the really large crowds that are really active," one trader told Baker, "it's possible to get trading in very different prices. . . . It's noisy; you can't hear." (quoted by Baker 1984a, p. 786) The small crowd, in contrast, tended to



remain stable in membership, and always small enough for easy communication. Prices in it tended to remain stable, even as trading became more intense. A trader active during the late 1970s, when Baker was studying options trading, explained to me that the cause was essentially collective action in the smaller “crowds” (see also Baker 1984b):

... the larger crowds were ... really competitive and ... sometimes egos would get in the way and ... some guy would get a trade and the next guy would say “Well, I would have paid an eighth better for twice the amount,” and there’d be screaming and shouting. But in some of the slower pits ... there wasn’t as much competition, then there would [be] more of a sharing basis, which was always a problem to some of the firms because they viewed them ... somewhat as cliques and nobody would ever break rank in terms of pricing. If an order came in, and the market would be [bid]  $\frac{1}{8}$ , [ask]  $\frac{1}{2}$ , or something ... nobody would ever sell it at  $\frac{3}{8}$ , nobody would ever break rank.<sup>14</sup>

There were, furthermore, ways in which small crowds could keep themselves small, for example, by always seeing or hearing an existing member “first” (Hull interview).

### ***Black-Scholes on the Trading Floor***

Viewed from the galleries from which (before September 11, 2001) visitors could watch, Chicago’s pits could appear to be places of frenzied chaos. Those who trade in them, however, do so day after day, and, especially in the smaller pits, they are normally trading repeatedly with the same people. Traders often stand in the same spot every day, and where one stands is consequential. There is a spatial hierarchy in Chicago’s pits, as Caitlin Zaloom (2004) found in the pits of the Board of Trade and as Cari Lynn (2004) found at the Mercantile Exchange. Novice traders begin at the bottom, in what is sometimes called “the soup,” and as they gain in seniority, in capital, and in ability to take on larger trades, they are allowed gradually to move up the steps of the pit.

The top rung of a pit—traditionally reserved for brokers who handle large customer orders rather than trading primarily on their own account—is a particularly prized place. At the Board of Trade, for “locals” who traded on their own account to stand there was a breach of spatial hierarchy, albeit one that eventually was accepted once those locals showed they were able to take on large trades. “The brokers didn’t like it at first,” one local told author William Falloon (1998, p. 255), “but we stood our ground.”

The experience of trading in a pit is an intensely bodily one.<sup>15</sup> The stories pit traders tell are often bodily: of the voice coach who taught them to shout all day without becoming hoarse; of the showers of spit from other people shouting; of the heat; of sweating bodies inside polyester trading jackets; of

the need for surreptitious sips of water (but not too much, because no one wants to have to leave a pit at a crucial moment for urgent bodily reasons); of feeling worried about being stabbed in the face by one's neighbor's pencil (and the realization that the onset of a worry such as that signals the time to retire from pit trading); of one's knees beginning to give way in middle age from standing all day; of being able to read fear in others' eyes and faces. Being tall, for example, is an advantage to a pit trader: the Mercantile Exchange had to impose a limit on the extent to which traders could add to their bodily height by wearing platform heels, because the latter were causing too many accidental falls on the pits' steps.

Although there are successful female pit traders (Koppel 1998, Lynn 2004), the pits are primarily places of male bodies.<sup>16</sup> The need to protect one's chosen spot in the pit, and the desire to be surrounded by traders who would take on, or brokers who would offer, suitably large trades, mean much jostling among those male bodies and occasional fights. One Board of Trade broker told Zaloom:

I've had guys stand next to me and I've bumped them literally two or three hundred times a day with my elbow. . . . I can do it and not even blink an eyelash like I'm not even doing it. And they just don't like that. They are gone. They're standing somewhere else. (Zaloom 2004, p. 376)

Another trader, in this case on the Mercantile Exchange, told me:

You get people who's trying to position themselves closer to the brokers and there is jostling there, who gets better position. And then you get certain people there who are just . . . bullies and that's how they make money. . . . They intimidate other people, whether it be physical or financial and every once in a while you just got to put them where they belong. So whether you do it in their face or knock the shit out of them from behind, by accident of course, just every once in a while. . . . There's all kind of people there and that's what the fights are all about. . . . Now you're paying attention to the market and this idiot's sitting in front of your face and screaming at you to give him a five-lot [a relatively small contract]. "Get out of my face." And every once in a while they just don't. Well if they don't, you just have to shove them back. If he doesn't, if you nudge him a little bit and he doesn't move, you just have to hit him. That's the way it goes. It's unfortunate, but that's the way it is.

This, then, is the environment into which Black-Scholes-Merton option theory was launched in 1973 (I will return below to the simpler theory of futures prices). The theory's abstract mathematics might seem to inhabit a world quite different from the deeply social, intensely bodily interaction of trading pits. The Black-Scholes-Merton model, however, began to have effects on the Chicago Board Options Exchange almost immediately. Even more effectively than the economics of Baumol, Malkiel, and Quandt, the

Black-Scholes analysis enabled the Options Exchange to rebut the charge that options were morally disreputable.

The former counsel of the Chicago Board Options Exchange, Burton R. Rissman, told me that even after it opened it still faced the stigma of the association with gambling. For example, he visited a senior official of the Federal Reserve to try to persuade her to interpret its Regulation T, which restricts the extension of credit for the purchase of stocks, in a way that would facilitate the hedging of options by the exchange's market makers. On her desk was spread out a *Wall Street Journal* article on options, "and I could see, as I sat there, that some places were underlined in red . . . Everywhere that was underlined was the word 'gambling.'" (Rissman interview)

As the work of Black and Scholes became well known, it undermined the long-standing cultural association between options and gambling:

Black-Scholes was really what enabled the exchange to thrive. . . . It gave a lot of legitimacy to the whole notions of hedging and efficient pricing, whereas we were faced in the late 60s–early 70s with the issue of gambling. That issue fell away, and I think Black-Scholes made it fall away. It wasn't speculation or gambling, it was efficient pricing. I think the SEC very quickly thought of options as a useful mechanism in the securities markets and it's probably—that's my judgment—the effects of Black-Scholes. I never heard the word "gambling" again in relation to stock options traded on the Chicago Board Options Exchange. (Rissman interview)

That the theoretical work of Black and Scholes could effectively be deployed to defend options' legitimacy did not, however, mean that the theory was adequate as an empirical description of prices. As we saw in chapter 5, when Black and Scholes tested their formula against prices in the ad hoc New York options market, they found only approximate agreement (Black and Scholes 1972).

Nor did the opening of the Chicago Board Options Exchange immediately improve the fit between the Black-Scholes-Merton model and market prices. Mathew Gladstein of the Donaldson, Lufkin & Jenrette Securities Corporation contracted with Scholes and Merton to provide theoretical prices ready for its opening: ". . . the first day that the Exchange opened. . . . I looked at the prices of calls and I looked at the model and the calls were maybe 30–40 percent overvalued! And I called Myron [Scholes] in a panic and said 'Your model is a joke,' and he said 'Give me the prices,' and he went back and he huddled with Merton and he came back. He says 'The model's right.' And I ran down the hall . . . and I said 'Give me more money and we're going to have a killing ground here.'" (Gladstein interview)

As was noted in chapter 1, Black-Scholes prices are extremely sensitive to the value chosen for volatility: although a stock's past volatility can be meas-

ured statistically, its expected future volatility is what affects the price of an option on it. So a discrepancy such as that noted by Gladstein could in principle have been caused by Scholes's and Merton's estimate of future volatility being too low.

However, Scholes's student Dan Galai systematically tested whether it was possible to use the Black-Scholes model to earn excess profits from the patterns of prices in the first seven months of the Chicago Board Options Exchange. If price patterns followed the model, it should not be possible to earn such profits. Galai (1977, p. 195) found that "some above-normal profits could have been made." These profits "were even greater than those found in our original tests," says Scholes (1998, p. 486), indicating a poorer fit of the model to the Options Exchange than to the earlier ad hoc market.

Galai had, of course, all the advantages of the academic. He was investigating hypothetical trading strategies using past price data supplied on punched cards by the Chicago Board Options Exchange. He had available to him programming assistance and the University of Chicago's powerful computers. Could the Black-Scholes-Merton model be used not in these favorable circumstances but in the bodily hubbub of a trading floor?

The Black-Scholes-Merton model's core was a differential equation that would have been opaque to anyone without college-level training in mathematics. Even in the simplest case, a call option on a stock that pays no dividend (appendix D, equation 2), an unaided human being cannot realistically be expected to calculate a Black-Scholes price. At the very least, a table of natural logarithms and of the distribution function of a normal distribution are needed.

Furthermore, the options being traded in Chicago were not the "European" options analyzed by Black and Scholes; they were "American" options. (The latter be exercised at any point until they expire, not just at their expiration.) To value an American call on a dividend-bearing stock, one needed a correction procedure that was not yet in the published literature. When the U.S. option exchanges were allowed to begin trading puts, in June 1977, things got worse: it was far from clear how to find a theoretical price for an American put.<sup>17</sup>

In the 1970s and for most of the 1980s, it would have been against the rules of the Chicago Board Options Exchange to use a computer on the trading floor, and to my knowledge no one tried: the computers of the period were too cumbersome. The programmable calculators that were becoming available in the mid 1970s were permissible and easily portable, and could be used to find values of the simplest Black-Scholes case: a call on a non-dividend-bearing stock. Calculators pre-programmed with the necessary algorithm were

sold, but they were not used much on trading floors, despite a widespread impression to the contrary in sources such as Passell 1997.

Most traders seem to have regarded calculators as too slow for use in a pit. Even the few seconds it would take to input parameter values and wait for a solution could mean a loss of profitable trading opportunities. Furthermore, making the necessary adjustment to take into account the payment of dividends was “difficult and time-consuming.” In consequence, few “use [programmable calculators] regularly for option evaluation after the initial novelty wears off” (Gastineau 1979, pp. 269–270).

Instead, an old technology—paper—was the most important mediator between the mathematics of the Black-Scholes-Merton model and what was sometimes the scrum of human bodies on trading floors. Away from the hubbub, computers were used to generate Black-Scholes prices. Those prices were reproduced on sets of paper sheets which floor traders could carry around, often tightly wound cylindrically with only immediately relevant rows visible so that a quick squint would reveal the relevant price. While some individual traders and trading firms produced their own sheets, others used commercial services.

Perhaps the most widely used sheets were sold by Fischer Black himself. (See figure 6.1.) Each month, Black would produce computer-generated sheets of theoretical prices for all the options traded on U.S. options exchanges, and would have them photocopied and sent to those who subscribed to his pricing service. In 1975, for example, sheets for 100 stocks, with three volatility estimates for each stock, cost \$300 per month, while a basic service with one stock and one volatility estimate cost \$15 per month (Black 1975b).

Black incorporated dividend corrections, using a technique he had worked out. When puts began to be traded, he drew on the work of Michael Parkinson (1977) of the University of Florida, one of the first physicists to

### **Figure 6.1**

One of Fischer Black’s sheets. The numbers on the extreme left hand side of the table are stock prices, the next set of numbers are strike prices, and the large numbers in the body of the table are the Black-Scholes values for call options with given expiry dates (for example, July 16, 1976) on the Fridays of successive weeks (for example, June 4, 1976). Because U.S. exchange-traded options contracts correspond to blocks of 100 shares, the value of one contract was 100 times the option value listed in Black’s sheets. The smaller numbers in the body of the table are the option “deltas” (the amount an option contract changes in value if the stock price changes by a dollar). The data at the head of the table are interest rates, Black’s assumption about stock volatility, and details of the stock dividends. Courtesy of Mark Rubinstein and the estate of Fischer Black.

I UNITED STATES STL CORR

REGISTRATION ANN INT ANN DEV DIV INT EX DATE  
JLT 16 76 5.4507 21.008 5.4700 8/ 4/76  
OCT 15 76 6.2804 21.008 5.4700 11/ 3/76  
JAS 21 77 6.5395 21.008

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	06/02/76			06/11/76			06/18/76			06/25/76			07/02/76		
	JLT 76	OCT 76	JAN 77	JLT 76	OCT 76	JAN 77	JLT 76	OCT 76	JAN 77	JLT 76	OCT 76	JAN 77	JLT 76	OCT 76	JAN 77
45															
40	5.31	5.85	6.50	5.25	5.78	6.43	5.19	5.72	6.36	5.18	5.67	6.29	5.09	5.56	6.22
45	1.33	2.32	3.39	1.29	2.33	3.39	1.18	2.34	3.39	1.17	2.23	3.17	1.07	2.16	3.09
46.7	7.27	1.77	2.63	6.09	1.69	2.56	6.08	1.61	2.49	5.52	1.53	2.42	2.13	1.44	2.35
50	1.27	1.76	1.51	0.83	0.73	1.45	0.86	0.67	1.39	0.89	0.67	1.33	0.84	0.59	1.21
53.3	0.13	1.13	1.80	0.06	0.27	1.76	0.02	0.24	1.72	0.00	0.21	1.62	0.00	0.18	1.61
55	0.03	0.18	0.30	0.01	0.12	0.54	0.00	0.13	0.50	0.00	0.11	0.45	0.00	0.09	0.42
60	0.00	0.03	0.19	0.00	0.02	0.18	0.00	0.02	0.16	0.00	0.01	0.17	0.00	0.02	0.12
66.7	0.00	0.03	0.00	0.00	0.02	0.15	0.00	0.01	0.10	0.00	0.01	0.06	0.00	0.00	0.02
70	0.00	0.01	0.07	0.00	0.00	0.15	0.00	0.00	0.12	0.00	0.00	0.10	0.00	0.00	0.08
46															
40	6.28	6.73	7.33	6.23	6.66	7.27	6.18	6.59	7.20	6.13	6.52	7.13	6.05	6.45	7.06
45	2.08	3.12	4.00	1.90	3.03	3.92	1.76	2.94	3.85	1.60	2.84	3.77	1.42	2.75	3.69
46.7	1.18	2.26	3.15	1.00	2.17	3.08	0.88	2.08	3.01	0.83	1.99	2.93	0.68	1.90	2.86
50	1.24	1.08	1.87	0.75	1.01	1.80	0.71	0.93	1.74	0.67	0.87	1.68	0.58	0.79	1.61
53.3	0.31	0.41	1.08	0.16	0.46	1.00	0.08	0.36	1.04	0.01	0.31	0.92	0.00	0.28	0.93
55	0.09	0.27	0.75	0.04	0.28	0.71	0.01	0.24	0.65	0.00	0.18	0.64	0.00	0.15	0.58
60	0.00	0.06	0.27	0.00	0.06	0.25	0.00	0.07	0.22	0.00	0.05	0.20	0.00	0.04	0.19
66.7	0.00	0.05	0.09	0.00	0.03	0.05	0.00	0.02	0.04	0.00	0.02	0.04	0.00	0.01	0.02
70	0.00	0.01	0.02	0.00	0.01	0.02	0.00	0.01	0.01	0.00	0.00	0.01	0.00	0.00	0.01
47															
40	7.27	7.66	8.20	7.22	7.58	8.14	7.17	7.51	8.07	7.13	7.45	8.00	7.03	7.38	7.94
45	2.78	3.78	4.66	2.63	3.64	4.58	2.50	3.49	4.50	2.37	3.51	4.42	2.22	3.41	4.35
46.7	1.67	2.81	3.73	1.52	2.72	3.65	1.37	2.43	3.58	1.20	2.33	3.50	1.00	2.44	3.42
50	0.30	1.02	2.28	0.33	1.34	2.21	0.23	1.26	2.14	0.17	1.10	2.07	0.05	1.10	2.00
53.3	0.08	0.30	1.31	0.04	0.37	1.26	0.03	0.32	1.20	0.00	0.27	1.14	0.01	0.24	1.08
55	0.02	0.40	1.97	0.01	0.36	1.92	0.00	0.39	1.87	0.01	0.38	1.82	0.00	0.34	1.78
60	0.00	0.08	0.36	0.00	0.07	0.33	0.00	0.05	0.33	0.00	0.04	0.28	0.00	0.03	0.24
66.7	0.00	0.00	0.05	0.00	0.00	0.06	0.00	0.00	0.07	0.00	0.00	0.05	0.00	0.00	0.05
70	0.00	0.00	0.03	0.00	0.00	0.03	0.00	0.01	0.02	0.00	0.01	0.02	0.00	0.00	0.01
48															
40	8.26	8.56	9.10	8.22	8.52	9.03	8.17	8.45	8.97	8.13	8.39	8.90	8.08	8.33	8.88
45	3.57	4.50	5.36	3.46	4.41	5.29	3.35	4.32	5.21	3.23	4.22	5.13	3.14	4.14	5.05
46.7	2.31	3.43	4.36	2.17	3.34	4.28	2.03	3.24	4.20	1.87	3.15	4.12	1.69	3.05	4.04
50	1.07	1.82	2.75	0.84	1.74	2.67	0.68	1.65	2.60	0.52	1.56	2.52	0.38	1.47	2.45
53.3	0.13	0.87	1.63	0.08	0.79	1.57	0.04	0.72	1.50	0.00	0.66	1.44	0.01	0.63	1.38
55	0.09	0.56	1.23	0.07	0.51	1.17	0.03	0.46	1.12	0.00	0.41	1.06	0.00	0.36	1.01
60	0.01	0.13	0.48	0.00	0.15	0.43	0.00	0.16	0.42	0.00	0.14	0.39	0.00	0.12	0.36
66.7	0.00	0.04	0.12	0.00	0.01	0.10	0.00	0.00	0.09	0.00	0.00	0.08	0.00	0.00	0.07
70	0.00	0.00	0.07	0.00	0.00	0.06	0.00	0.00	0.04	0.00	0.01	0.04	0.00	0.00	0.03

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become interested in option theory, to provide his subscribers with theoretical prices of puts (Gastineau 1979, p. 269).

At first sight, Black's sheets look like monotonous arrays of numbers. They were, however, beautifully designed for their intended role in "distributed cognition" (Hutchins 1995a,b). Black included what options traders using the Black-Scholes-Merton model needed to know, but no more than they needed to know. There was virtually no redundant information on his sheets—hence the sheets' easy portability. Black devoted particular care to the crucial matter of the estimation of volatility.<sup>18</sup>

Even the physical size of Black's sheets was well judged. They had first to be printed on the large computer line-printer paper of the period, but they were then photo-reduced onto standard-size paper, differently colored for options traded on different exchanges.<sup>19</sup> The resultant sheets were small enough for easy handling, but not so small that the numbers became too hard to read. (The reproduction in figure 6.1 is smaller than full-scale.)

### ***Why Black-Scholes-Merton?***

The care that Black put into his sheets does not, of course, explain why they were bought and used. Not all options traders believed using sheets was necessary. The options trader Blair Hull reports that he was mocked for using them. Fellow traders "would laugh at you and try to intimidate you out of the pit, saying 'You're not a man if you're using those theoretical value sheets.' They'd take your sheets and throw them down on the floor and say 'Be a man. Trade like a man. . . . You shouldn't be here. You're not a trader. You can't trade without those.''" (Hull interview)

Even if a trader found sheets useful, Black's were not the only sheets available. Gastineau's *Stock Options Manual* (1975) listed three options advisory services; his book's second edition (1979) listed 15. Of the latter, six did not offer option values, so they were not directly comparable with Black's service. Five services, including Black's, offered theoretical prices generated from the Black-Scholes-Merton model or variants thereof. The remaining four services, however, used a different approach, offering option values based not on theoretical reasoning but on econometric analyses of observed patterns of option prices. These analyses seem mainly to have been variants of Sheen Kassouf's econometric work, discussed in chapter 5.

Why might a participant in the options market in the 1970s have chosen to use Black's sheets or another material implementation of the Black-Scholes-Merton model? One factor could have been the authority of economics. Financial economists quickly came to see the Black-Scholes-Merton model as



superior to its predecessors. It involved no non-observable parameters except for volatility, and it had a clear theoretical basis, one closely linked to the field's dominant viewpoint: efficient-market theory.

However, while there were a number of participants with links to academia, Chicago floor traders in general were and are not in awe of professors. From their viewpoint, however, the model had the advantage of "cognitive" simplicity. The mathematics of the solutions to the Black-Scholes equation might be off-putting (especially when one had to consider dividend-bearing stocks and American puts), but the model could be talked about and thought about relatively straightforwardly.

The Black-Scholes-Merton model's one free parameter (volatility) was easily grasped, discussed, and reasoned about. Other models—including some of the variants of and modifications of Black-Scholes-Merton that were offered by other financial economists—typically involved a mental grasp of, and estimation of, more than one free parameter—often three or more. As *The Stock Options Manual* put it, "the user of these complex models is called upon to deal with more unknowns than the average human mind can handle" (Gastineau 1979, p. 253).

Another factor underlying the success of the Black-Scholes-Merton model was simply that it was publicly available in a way many of its early competitors were not. As U.S. law stood in the 1960s and the 1970s, it was unlikely that an options pricing model would be granted patent or copyright protection, so there was a temptation not to disclose the details of a model. Black, Scholes, and Merton, however, did publish the details, as did Kassouf (whose model was described in his Ph.D. thesis, and thus was available for use by options pricing services). Keeping the details private may have been perfectly sensible for those who hoped to make money from their models, but it was a barrier to the adoption of those models by others.<sup>20</sup>

For example, Gary Gastineau (author of *The Stock Options Manual*) developed, with Albert Madansky of the University of Chicago, a model that Gastineau believed remedied what he felt were the over-idealized assumptions of the Black-Scholes-Merton model. (For instance, the Gastineau-Madansky model used "an empirical stock price distribution rather than the lognormal distribution.") However, not only did Gastineau publish only "an outline of the general form" of his model; he used its results "solely for the benefit of certain individual and institutional clients," rather than making them available more widely in the form of an options pricing service (Gastineau 1979, pp. 203, 253, 269).

Gastineau was thus in the paradoxical situation of being a critic of the Black-Scholes-Merton model who nevertheless felt compelled to recommend



Black's sheets to the readers of his *Stock Options Manual*, because nothing he considered better was publicly available. "Until another weekly service incorporates Black's service, his tables . . . are the best evaluation data available to the average investor." (Gastineau 1979, p. 269)<sup>21</sup>

The situation was perhaps akin to the triumph of the publicly available IBM PC architecture over rival architectures, especially Apple's. The architecture of IBM personal computers may well not have been better than Apple's proprietary architecture, but IBM's design (like the Black-Scholes-Merton model) was available for others to adopt while Apple's was not.

### ***Barnesian Performativity***

The options traders who used Black's sheets or other implementations of the Black-Scholes-Merton model hoped, of course, to make money. The most obvious way of doing that was to put into practice the arbitrage strategy inscribed in the model's derivation. One could use the model to identify options that were overpriced (or underpriced) relative to their theoretical values, sell them (or buy them), and hedge the risk by taking and adjusting a position in the underlying stock. Black's sheets included not only theoretical prices but also the "delta" values that told subscribers how big that hedging position in stock should be. (See figure 6.1.)

Two practical difficulties stood in the way of this strategy. First, one had to be confident in one's own estimate (or Black's estimate) of the volatility of the underlying stock. Second, taking a position in stock was expensive, especially for Chicago floor traders and other options-market participants who did not belong to firms that were members of the New York Stock Exchange. In particular, stock purchases and sales incurred steep commissions. Firms such as Goldman Sachs or Gladstein's Donaldson, Lufkin, & Jenrette could operate successfully on both the Chicago Board Options Exchange and the New York Stock Exchange, but many others could not.

However, any floor trader on the Chicago Board Options Exchange could practice "spreading" (Black 1975a, pp. 39–40; Galai 1977, pp. 189–194). This operation—which appears to have been practiced widely—involved using the model to identify pairs of options on the same underlying stock, in which one option was, according to the model, underpriced relative to the other. Traders could then buy the underpriced option and sell its overpriced counterpart, and applying some simple arithmetic to the numbers on Black's sheets showed how to minimize exposure to the risk of fluctuations in the price of the underlying stock: one made the sizes of purchases and sales inversely proportional to the deltas of the options in question.

The “Introduction” to his options service that Black circulated gave simple instructions how to identify and to exploit opportunities for spreading (Black 1975b, p. 7). Spreading avoided both the difficulties of the more basic Black-Scholes arbitrage strategy described above. It was “less sensitive to the estimated volatility of the stock” because “an increase in the volatility estimate will increase the value of all the options on a stock” (Black 1975a, p. 40), and it did not require purchases or sales of stock: it required only purchases and sales of options.

At first, the correspondence between the Black-Scholes-Merton model and patterns of prices on the Chicago Board Options Exchange was fairly poor. Soon, however, it began to improve. The most thorough tests of fit were conducted by Mark Rubinstein (1985), using a subset of a huge data base of nearly all Chicago Board Options Exchange price quotations and transactions between August 1976 and August 1978. (As was noted in chapter 1, the subset was created by eliminating trading close to the start and finish of the trading day, trading in situations of low liquidity, and so on.)

By constructing from matched pairs of observed option prices the estimate of volatility that minimized deviations from Black-Scholes values, Rubinstein judged the fit of the Black-Scholes-Merton model without independently estimating volatility. He calculated the maximum deviation from the Black-Scholes prices implied by that volatility, finding (in the case of options on the same stock with the same time to expiration but different strike prices) typical deviations of around 2 percent—by any social-science standards, a good fit.

A fundamental aspect of what Rubinstein did was, therefore, to check the empirical validity of a basic feature of the Black-Scholes-Merton model: “that all options on the same underlying asset with the same time-to-expiration but with different striking prices should have the same implied volatility” (Rubinstein 1994, p. 772). In other words, Rubinstein checked whether the graph of implied volatility against strike price was a flat line, as it should be on the model. There was thus a homology between the econometric testing of the Black-Scholes-Merton model and the trading-floor use of the model in “spreading.” When spreaders used the model (for example, by following the instructions that accompanied Black’s sheets) to look for discrepancies, it would be precisely deviations from that flat line that they would have identified and that their activities would have tended to “arbitrage away.” It seems, therefore, that the model may have been helped to pass its central econometric test “with remarkable fidelity” (Rubinstein 1994, p. 772) by the market activities of those who used it.<sup>22</sup>

If my conjecture is correct (and though it is plausible, the available evidence does not permit certainty), it would constitute a form of performativity that is,

in the terminology of chapter 1, Barnesian. There, I quoted Barry Barnes's succinct summary of his viewpoint: "I have conceived of a society as a distribution of self-referring knowledge substantially confirmed by the practice it sustains" (Barnes 1988, p. 166). The Black-Scholes-Merton model was used in the practice of arbitrage—especially, but not exclusively, in "spreading"—and the effects of that arbitrage seem to have been to move patterns of prices toward the postulates of the model, in particular on Rubinstein's test. The "practice" that the Black-Scholes-Merton model sustained helped to create a reality in which the model was indeed "substantially confirmed."

### ***A Black-Scholes World***

The effects of the use of the Black-Scholes-Merton model in arbitrage thus seem to have formed a direct performative loop between "theory" and "reality." Markets were, however, also changing toward greater conformity to the model for reasons in which the model was not directly implicated. In 1973, Black, Scholes, and Merton's assumptions were wildly unrealistic. Stock, for example, could not be bought entirely on credit: it had been the constraint this placed on market makers' capacity to hedge using stock that Rissman had been trying to have removed during his unsuccessful visit to the Federal Reserve.

If an options trader did not belong to a member firm of the New York Stock Exchange, stock transactions incurred, as noted above, significant commissions. The information on stock prices needed to price an option and to create and adjust a hedge was often not available quickly: at first, the electronic "feed" conveying prices from New York to Chicago was relatively slow. Purchases and sales of stock took time, and "to place a stock order, a marketmaker must leave the options trading crowd (or at least momentarily divert his attention from options trading activity), and, as a result, may lose the opportunity to make an advantageous options trade" (SEC 1979, pp. 139–140).

Gradually, though, many of the Black-Scholes-Merton model's assumptions gained greater verisimilitude. Stock borrowing became easier, for example as the trust departments of U.S. banks began to be prepared to lend out stocks owned by their clients (Faulkner 2004, p. 47), and the balance of market power began to shift, with borrowers obtaining increasing proportions of the interest on the proceeds of short sales (Thorp interview). A protracted struggle over the New York Stock Exchange's fixed commissions ended with their abolition in May 1975. The speed of transmission of stock prices from New York to Chicago was increased, and better communications and increasing automation made it quicker and easier to adjust the replicating portfolio.

It was even proposed that if the limit on the extension of credit to purchase stocks laid down by the Federal Reserve were to be removed for hedging by market makers on options exchanges, the Black-Scholes model would be used, performatively, to determine the quantity of stock purchases that constituted a bona fide hedge and that were therefore eligible for unrestricted credit. Had this proposal been accepted, it would have been another delightfully direct loop of performativity—the Black-Scholes model deployed to make one of its assumptions a reality—but the Federal Reserve rejected the idea.<sup>23</sup> The Federal Reserve nevertheless permitted options market makers unrestricted use of credit for their options positions and, at least in some cases, 75 percent credit for stock bought as a hedge.<sup>24</sup>

Gradually, too, the culture of the Chicago Board Options Exchange changed. Initially, it was (as noted above) allowed to trade only call options, and it did so in a bear market, in which calls with high strike prices seemed always to expire unexercised, because the stock price remained below the strike price. A good income could therefore apparently be earned by selling those calls without bothering with theory or hedging. “Sell the 280s and drive a Mercedes” became the motto of at least some traders, referring to high-strike-price (\$280) calls on IBM, then the stock on which options were most heavily traded (Doherty interview).

One manager, Michael Greenbaum, tried to teach option theory to market makers: “He’d stay late to give seminars and two people would show up, and it would be Joe Doherty [Sullivan’s deputy, who became an options trader] and somebody else, and they were already fully sold disciples. . . . He’s trying to bring in some new technology [option-pricing theory] and nobody would use it or pay for it.” (Carusillo and Struve interview 1)

However, a sudden surge in stock prices in April 1978 caused huge losses to market makers who had sold large numbers of insufficiently hedged calls, and some were forced out of the market: those who had sold the 280s lost their Mercedes, so to speak. Gradually, Chicago options market makers began to develop a distinct self-identity in which careful pricing and hedging were important. On the agricultural futures exchanges, the stereotypical belief was “I got the trade ’cause I’m faster than you, buddy.” In New York, it was “I got the trade ’cause I’m here”—that is, because I am the designated specialist. In the Chicago Board Options Exchange’s growing self-perception, it was “I got the trade ’cause I thought it out” (Doherty interview).

The Chicago Board Options Exchange grew and prospered, and other exchanges also began to trade options. The American Stock Exchange in New York began to do so in January 1975, the Philadelphia Stock Exchange in June 1975, and the Pacific Stock Exchange in San Francisco in April 1976 (Cox and

Rubinstein 1985, p. 24). With options trading becoming big business, individual market makers and small firms were gradually displaced by larger firms (such as O'Connor and Associates, set up in 1977 when Michael Greenbaum, whose poorly attended classes on options theory were described above, persuaded Edmund O'Connor to support him in establishing a firm to trade on all the U.S. options exchanges).

In the 1970s, options on a given stock were traded on only one exchange. If one wished to take a position on an entire industrial sector, it had to be implemented across all the exchanges on which relevant options were traded. Seat-of-the-pants trading could not suffice when implementing a position across several markets and carrying dozens or hundreds of such positions. Pricing models were necessary for risk management, and, crucially, they offered a way of communicating and coordinating activities—a way of *talking* about options.

Central to the way in which the Black-Scholes-Merton model or its variants facilitated communication was the notion of “implied volatility.” This is calculated by running the Black-Scholes model “backwards,” using observed option prices to infer, by iterative solution, the stock volatilities they implied. “Implied volatility” reduced the complexity of option trading (different stocks with different, changing, prices; puts and calls; different expirations and strike prices) to a simple common metric. For example, O'Connor traders in the different options exchanges used their “sheets” to calculate implied volatilities and reported them by hand signals to the O'Connor booths beside the trading floors and thus to the firm's headquarters—e.g., “I can buy Arco [the oil company Atlantic Richfield] on a 15” (in other words, purchase options the price of which implied a 15 percent per annum volatility of Atlantic Richfield stock). There would be “two or three people sitting upstairs saying ‘Mickey can buy Arco on a 15. Someone in San Francisco can buy Santa Fe on a 13.’ They're both big oil companies. . . . If you thought all oil stocks were similar . . . you'd certainly rather buy one on 13 than a 15. . . . So they'd say ‘don't buy any.’” (Carusillo and Struve interview 1)

Gradually, what was being bought and sold in an options market was re-conceptualized: it was the Black-Scholes-Merton model's free parameter, *volatility*. If stock volatility increased, options became more valuable; if it decreased, they became cheaper (see, for example, Dunbar 2000, p. 167). Strategies involving a multiplicity of different transactions could be talked about very simply: “. . . we would have a morning meeting, and Greenbaum would say ‘The book isn't long enough volatility. We're looking to buy some,’ or ‘We bought too much yesterday. We're looking to be less aggressive.’” (Carusillo and Struve interview 1)

As the options markets grew, and the SEC relaxed, options were traded on more stocks, some no longer “blue chip” corporations but instead highly volatile newcomers. More expensive errors made pricing models seem indispensable: “I’ll stand in a pit with [options on] 16 stocks that each trade two [expiration] months and five strike prices and I’ll take anybody on: turn off the lights, I want to trade with no electronics. But when you get to multiple expirations, strike prices, higher volatility stocks . . . volatility changes by 10 percent. Arco goes from a 15 to a 13.5 [annualized percentage implied volatility]. I can do that math in my head. [With a highly volatile stock] 150 to 135 seems like it ought to be similar, but . . . I’ve got too much money at risk if I’m wrong in my mental calculations.” (Carusillo and Struve interview 1)

Gradually, theoretical models became not just private resources for traders and their firms but the public property of the entire Chicago Board Options Exchange trading floor. This began with the introduction of options on stocks listed not on the New York Stock Exchange but on NASDAQ (the National Association of Securities Dealers Automated Quotation system, set up in 1971).

NASDAQ has no trading floor: stock trading on it is dispersed among large numbers of dealers buying and selling via computer screens and telephones. Chicago market makers soon learned that stock prices on NASDAQ screens were, in practice, indicative only. One could not be sure of a genuine price for a large transaction until one telephoned a dealer, and there were suspicions that at crucial moments telephones were left unanswered for critical seconds.

Black, Scholes, and other options theorists had implicitly assumed (the matter was never expressly discussed) that the price of the underlying stock was known. With NASDAQ, that pervasive assumption failed. Hence, in 1986, the Chicago Board Options Exchange launched its first “Autoquote” system. In the crowds within which NASDAQ options were traded, Exchange employees would feed in the prices of the most liquid options, those with a strike price close to the current stock price.

Autoquote software implementing the Black-Scholes equation would then generate the price of a “synthetic underlying”—that is, calculate the stock price compatible with those option prices. From that it would generate and make public to traders the Black-Scholes prices of the full range of options being traded—including the less liquid ones, for which preceding market prices might be a poor guide (Knorrning interview).

The gradual “mathematicization” of options trading did not mean abstraction from the dense, bodily, spatial social structures of Chicago pit trading: mathematicization took place through those structures, not despite them. Consider, for example, the basic mathematical relation of put-call parity (Stoll

1969). The possibility of a simple arbitrage ties together the prices of put and call options with the same underlying stock, expiration, and strike price.<sup>25</sup> Traders on the Chicago Board Options Exchange and other options exchanges constantly monitor the relative prices of puts and calls, watching for violations of put-call parity, and exploiting them—and thus eliminating them—if they occur.<sup>26</sup>

The socio-spatial structures of different exchanges, however, affected the ease with which the mathematical relation between put and call prices was maintained. The first exchange to join Chicago in trading options was the American Stock Exchange (Amex). The social structure of trading on the Amex differed from that in Chicago. Like the New York Stock Exchange, the Amex had “specialists” who maintained “books” of buy and sell orders and matched those orders. When options trading began at the Amex, the specialist system was extended to it, while in Chicago the privileges of the specialist were anathema because of the tradition of competitive market makers with equal formal rights.

In Chicago, market makers quickly learned how to profit from discrepancies between put and call prices, and violations of put-call parity were typically evanescent. In busy classes of options at the Amex, however, it was found necessary to have two specialists (both members of the same firm), one taking responsibility for trading in calls and the other for trading in puts.

Apparently there were many more breaches of put-call parity on the Amex,<sup>27</sup> and these could be exploited by traders who stood between the two specialists monitoring the prices at which each would deal. “What a dream world that was. . . . They [the specialists] didn’t know . . . what [the other specialists were] doing. They were doing okay on their own, but they didn’t coordinate. . . . He’s the call specialist, you’re the put specialist. And you don’t even stand real close to each other.” (Carusillo and Struve interview 1)

### ***Markets and Politics***

During the 1970s, the range of derivatives traded in Chicago was gradually expanded. Having spun off the Chicago Board Options Exchange, the Board of Trade itself became involved in financial derivatives. Richard L. Sandor, who moved to the Board of Trade as its chief economist after teaching applied economics at the University of California at Berkeley, played a major role in designing and standardizing the Board’s new products. In October 1975, the Board of Trade began trading futures on the mortgage-backed bonds issued by the Government National Mortgage Association. In August 1977, trading in futures on Treasury bonds began (Falloon 1998).

In 1976, the Mercantile Exchange began trading futures on Treasury bills, Leo Melamed having again turned to Milton Friedman to assist in getting the approval of Secretary of the Treasury William E. Simon: "I didn't know Bill Simon and I didn't know who knew Bill Simon. It turned out Milton Friedman knew Bill Simon. So, having discovered that, I called up Milton Friedman and I said 'Look, we want to list Treasury bills. I know you know that's a good idea.' He said 'Yes, it's a good idea.' I said 'Well, will you tell Bill Simon that it's a good idea?' . . . By the time I walked into Bill Simon's office . . . Simon said 'I got a letter here from Milton Friedman. Where do you want me to sign?'" (Melamed interview)

However, the "ultimate contract," index futures, still seemed out of reach because of the problem of cash settlement. It remained the case that, in the words of Judge George H. Painter in 1976, "the possibility of delivery on the exchange is the single element distinguishing futures trading from wagering" (Tamarkin 1993, p. 172). So an index future that could be settled only in cash was still illegal. Melamed had, however, gone against the traditional Chicago preference for light regulation by supporting the creation of a new federal agency, the Commodity Futures Trading Commission, to regulate the activities of the Merc, the Chicago Board of Trade, and the other futures exchanges in the United States.

"Not a popular thing to create a federal agency around here," says Melamed. One reason for supporting its creation was "to legitimize what we were doing. Anyone that has a federal agency over it is a legitimate thing." Melamed's main motivation, however, was that a federal agency could "give that edict about cash settlement" (Melamed interview). The original 1974 charter of the Commodity Futures Trading Commission preempted the laws of individual states, so if it approved cash-settled futures contracts they would no longer conflict with state gambling laws (there was no federal prohibition on gambling).

Beyond cash settlement, however, lay another barrier to index futures: rivalry between regulators. The 1974 amendments to the Commodity Exchange Act that set up the Commodity Futures Trading Commission broadened the definition of "commodity" from agricultural products to "all other goods and articles . . . services, rights and interest in which contracts for future delivery are presently or in the future may be dealt in" (quoted by Brady Commission 1988, p. VI-76).

Philip McBride Johnson, who in 1974 was Counsel to the Chicago Board of Trade, and who later chaired the Commodity Futures Trading Commission, said that this broad, apparently vague phrasing was deliberate: "To have offered the word 'securities' would have set off alarms through the SEC



[Securities and Exchange Commission], which was at best a casual observer through most of the legislative process. Instead we offered a definition of ‘commodity’ that would include the phrase ‘services, rights and interests,’ believing one or more of those words would capture securities (to which the courts have subsequently agreed). This seemingly benign phrase was readily adopted.” (Johnson, quoted by Falloon 1998, p. 247)

The careful wording kept the financial derivatives traded by the Merc and the Board of Trade in the 1970s under the jurisdiction of the Commodity Futures Trading Commission, and away from that of the SEC. But despite having failed to notice the threat to its “turf” contained in the 1974 wording, the SEC could not fail to be aware of and unhappy about the emergence of new financial derivatives outside its jurisdiction. Allowing its regulatory rival authority over futures on *stock* indices would have been yet a further incursion into its “turf.”

“There was going to be a court battle, and it was going to delay us forever,” says Melamed. However, “as all things in Washington, you can compromise” (Melamed interview). The problematic feature of stock index futures from the viewpoint of the gambling laws—cash settlement—turned into a resource from the viewpoint of negotiating the jurisdictional dispute between the Commodity Futures Trading Commission and the SEC. “We aren’t delivering stocks, you know,” argued Melamed. “We are never going to deliver stocks. This is cash settlement. . . . It’s not necessarily a security. It isn’t anything, in fact.” (ibid.)

If what was about to be traded could be viewed not as a security nor even as the derivative of a security but (in the words of one SEC official) as “a figment of Melamed’s imagination,” agreement was possible (Melamed interview). In December 1981, John Shad of the SEC and Philip McBride Johnson of the Commodity Futures Trading Commission agreed that a cash-settled futures contract on a sufficiently broad stock index fell within the latter’s jurisdiction, and the Shad-Johnson Accord was enacted into law by the U.S. Congress in 1982 (Brady Commission 1988, pp. vi-77, vi-78).<sup>28</sup>

At last the path to the “ultimate contract” was clear. One question remained: On which stock index should futures be offered? The obvious choice was the Dow Jones industrial average, the index that was by far the best known to the wider public. However, the Mercantile Exchange had interviewed fund managers, and “everyone said ‘Obviously our benchmark is the S&P 500’” (Melamed interview). In 1980, with the possibility of index futures trading still a couple of years away, the Mercantile Exchange had, at Melamed’s insistence, forged an agreement with Standard & Poor’s for the exclusive right to trade futures on the S&P index.

The Merc's lawyers had told Melamed that no agreement was needed: ". . . they [the Standard & Poor's Corporation] don't own the index. Nobody owns an index. . . . It's . . . public domain." (Melamed interview) Indeed, when Melamed went to see Brenton W. Harries, the president of Standard & Poor's, "he looked at me as if I was . . . from Mars," asking Melamed "You want to pay me for something we're giving away for free?" (Melamed interview)

The Merc's apparently unnecessary expense turned out to be a wise investment. The Board of Trade decided to offer futures on the Dow Jones industrial average, and took the stance that the index was in the public domain, but then lost a court battle with Dow Jones. So it ended up trading futures on the Amex's much less well known Major Market Index, which was only a partial proxy for the Dow Jones.

There was one last nuance. The Chicago Mercantile Exchange had been the first to apply to the Commodity Futures Trading Commission to trade cash-settled stock index futures, and it could have pushed for the right to be the first to trade them. The Kansas City Board of Trade had prepared a proposal to trade futures on the Value Line index, settling the contracts not in cash but by delivery of stock. When it became clear that cash settlement would be permitted, it hurriedly revised its proposal.

Kansas City was a peripheral market that needed a boost, so Susan M. Phillips of the Commodity Futures Trading Commission (CFTC) requested that the Merc not insist on its priority over it and over the New York Stock Exchange's newly established futures exchange. After a decade plotting the path to the "ultimate contract," Melamed must have been tempted to insist on being the first to trade it, but he knew that "a favor of this kind to the CFTC would pay many dividends in the years ahead" (Melamed and Tamarkin 1996, p. 295). So Kansas City, not the Merc, was the first to trade index futures.

A market, says Melamed, "is more than a bright idea. It takes planning, calculation, arm-twisting, and tenacity to get a market up and going. Even when it's chugging along, it has to be cranked and pushed." (Melamed and Tamarkin 1996, p. 295) Even once the Merc's S&P index futures were launched (on April 21, 1982) the political work of market construction did not end. Financial derivatives exchanges are subject to virtuous and vicious cycles: if trading volumes are high, exchanges are liquid and attractive places to trade, further enhancing volume; if volume starts to slip, liquidity can dry up and exchanges become fatally unattractive.

Melamed worried in particular about the competition from the futures-trading offshoot of the New York Stock Exchange, the world's most powerful exchange: "I tell you that I was scared of them." So "all chits were . . . called in" by Melamed to ensure the success of the Merc's S&P futures. All those for

whom he had done favors over the years were asked to return them by trading the new contract. Every member of the Mercantile Exchange was asked to spend at least 15 minutes every day trading in the S&P pit, and Melamed led the way by spending as much of every day as he could in the pit himself. “I used to meet with them in groups. . . . I would meet with them one on one. Whatever it took. Get ’em in the pit. Get ’em in the pit.” (Melamed interview)

### ***Enacting Theorems***

A cash-settled index future contract worked—and, some details aside, still works—as follows. (I draw my example from Martin 1988, pp. 139–140.) Suppose the S&P 500 index level was 285.00, and a future on the index, maturing in three months, can be bought at 286.40. The “contract multiplier” in the 1980s was \$500, so a single contract had a notional value of the index level (285) multiplied by \$500, or \$142,500. The purchaser of such a contract, however, had to deposit with the Mercantile Exchange’s clearinghouse only an “initial margin” of \$10,000 (which could, for example, be in the form of U.S. Treasury bills, so one does not have to forgo interest on the margin payment). The seller also had to deposit a similar margin.

Every day, at the close of trading, the amount of “margin” deposited with the clearinghouse was and is adjusted according to the closing price of the future. For example, if the future price fell two points to 284.40, the purchaser had to deposit an additional sum equal to twice the contract multiplier—in other words, \$1,000. (The clearing problem on the Mercantile Exchange described at the start of chapter 1 was the need for extra deposits of this kind from those who had bought index futures.) If, in contrast, the future rose by a point, the purchaser could reduce the amount on deposit by \$500. (The seller of the index future would have to make equal but opposite adjustments to his/her margin account.) This process continues day after day until the contract matures, when the final day’s adjustment is determined by the level of the S&P 500 index at the start of the following day’s trading (Hull 2000, p. 33).

The purchaser of index futures thus receives returns similar to those received by a holder of the underlying stocks, profiting if the stocks that make up the index rise in price and losing money if they decline. The main differences are that the futures contract does not require one to put up the cash to buy the stocks (so one can therefore earn interest on the cash), but one forgoes dividends from the shares. In consequence, the theoretical value of an index futures contract is given by a simple equation<sup>29</sup> that can be written informally (Martin 1988, p. 139) as follows:

Futures value = Index price + Interest on index price – Index dividends.

The theoretical value of a future was not a “discovery” of finance theory; the analogous relationship between the value of a future, the price of the underlying asset, interest rates, and storage costs had long been understood by participants in commodity futures markets.<sup>30</sup> The relationship between futures prices and index levels was, nevertheless, a relatively strict mathematical relationship—a “theorem,” so to speak.

If the price of an index future deviates from the theoretical value given by the above equation, arbitrage profits can be made. For example, if the price is higher than the theoretical value, one can sell index futures contracts, cancel out the risk of index level fluctuations by buying an equivalent amount of the underlying stocks, and realize a sure profit when the future expires.

Index arbitrage thus has the potential to create an *objective* link between index futures and stocks. The theoretical value of an index future has nothing to do with opinions as to whether the price of stocks will rise or fall; like the theoretical value of an option, it is imposed by arbitrage. Arbitrage, therefore, had the potential to stop index futures being “a figment of Melamed’s imagination,” yoking them firmly to the prices of the underlying stocks.

In the early months of index futures trading, futures tended to be below their theoretical value. Stock prices were rising—in retrospect, “[19]82 . . . was the beginning of the big 80s bull market”—but nobody, futures traders included, believed it. “As a result, the index futures, which weren’t yet pegged to their correct value by lots of people doing arbitrage . . . were cheap.” (Weinberger interview)

When trading on Value Line futures began in Kansas City, David Weinberger, who worked for the arbitrageur (and future Secretary of the Treasury) Robert E. Rubin at Goldman Sachs, quickly pulled together an ad hoc but effective index arbitrage operation. Weinberger constructed a list of 30 stocks which collectively formed a reasonable proxy for the Value Line index and set up a link to a Goldman Sachs broker in Kansas City. He talked to the Goldman traders who handled small stock orders—who luckily liked Weinberger, and often felt despised by those who traded large blocks of shares—preparing them quickly to buy or to sell “baskets” of the thirty stocks on Weinberger’s shouted instructions (Weinberger interview).

With futures prices often still far different from theoretical values, Weinberger found the arbitrage highly profitable.<sup>31</sup> He moved from Goldman Sachs to O’Connor and Associates, where he set up a similar index arbitrage operation (Weinberger interview). Weinberger was soon joined in index arbitrage by others in investment banks and in hedge funds such as Princeton

Newport Partners, co-founded by Edward O. Thorp, whose work on option pricing was discussed in chapter 5.

Soon, the result of the activities of the expanding number of arbitrageurs was that departures of futures prices from their theoretical values were much more limited. Though transaction costs create a price zone within which arbitrage profits cannot be earned, the growing presence of arbitrageurs in the early and mid 1980s kept discrepancies between futures prices and theoretical value limited in size. For example, the average such discrepancy for three-month S&P index futures between June 1983 and mid-August 1986 was 0.32 percent (Hill, Jain, and Wood 1988, p. 24), and index arbitrageurs would typically move in whenever discrepancies grew to 0.5 percent (Anonymous 1988b).

What is even more striking, because of the much greater complexity of the mathematical relationship involved, is the closeness with which the index options that also began to be traded in the early 1980s clustered around the Black-Scholes flat-line relationship between strike price and implied volatility. The establishment of the Commodity Futures Trading Commission had freed the futures exchanges from the grip of state anti-gambling laws, but the options exchanges were still subject to them.

In 1978, for example, the SEC rejected a proposal from the Philadelphia Stock Exchange to trade index options, despite an “economic justification” written by Mark Rubinstein, “because of the gambling aspect” (Rubinstein interview). However, the options exchanges were able to get Congress to add a provision preempting state gambling laws to the 1982 legislation enacting the Shad-Johnson Accord (Rissman interview). In consequence, the Chicago Board Options Exchange started trading index options in 1983.

Crucially, because arbitrage tied the value of index futures to the level of the index, futures could be used to hedge index options. The world posited by the Black-Scholes-Merton model was more true of futures than it was of stock. Hedging using the Merc’s index futures incurred much lower transaction costs than hedging using stock. While buying stock on credit was still restricted by the Federal Reserve’s Regulation T, index futures equivalent to a large stock position could be bought by paying only the Merc’s modest margin deposit. The cost of selling stock short had come down; however, it remained expensive, and was sometimes it was not possible. In contrast, constructing a short position on an index in order to hedge an options position was inexpensive and straightforward: one simply sold index futures. The residual deviations of around 2 percent from theoretical prices that Mark Rubinstein had found for the stock options of 1976–1978 fell by 1986 to around 1 percent for index options (Rubinstein 1994, p. 774)—a trivial discrepancy.

By 1987, then, it could with some justice be said that “when judged by its ability to explain the empirical data, option-pricing theory is the most successful theory not only in finance, but in all of economics” (Ross 1987, p. 332). Along with the even closer fit between the prices of index futures and their theoretical values, it was a remarkable development: a transubstantiation. As we saw in chapter 5, option pricing had moved from practitioners’ rules of thumb to the more ethereal mathematics of stochastic differential equations and of martingales. In Chicago, that mathematics was being performed in flesh and blood. The shouting, gesticulating, sweating, jostling bodies in Chicago’s pits were enacting theorems.



Black-Scholes-Merton option-pricing theory was enacted at the Chicago Board Options Exchange and at similar exchanges elsewhere in the United States and overseas. The theory's other important early application was portfolio insurance: the use of option theory to guide trading so as to set a floor below which the value of an investment portfolio will not fall. The idea came to the finance scholar Hayne E. Leland of the University of California at Berkeley in September 1976. During a sleepless night, he was pondering how to boost his income via consultancy (Leland interview). The next day, he recruited the help of his Berkeley colleague Mark Rubinstein. An idea similar to Leland's was also developed, independently and slightly earlier, by Michael J. Brennan of the University of British Columbia and his student Eduardo S. Schwartz, who were considering the investment strategy that should be followed by insurance companies that sold investment products with a guaranteed minimum value (Brennan and Schwartz 1976). It was Leland and Rubinstein, however, who played the critical role in starting the process that led to the widespread adoption of portfolio insurance. Black, Scholes, and Merton had shown that, given certain conditions, it was possible to mirror perfectly the payoff on an option by continuously adjusting a position in the underlying stock and cash (or bonds). Because the position had the same payoff as the option, it was what we now call a "replicating portfolio." Black, Scholes, and Merton had used the idea of the replicating portfolio to work out what options ought to cost. Leland and Rubinstein (and also Brennan and Schwartz) focused on the replicating portfolio itself.

A floor below which the value of an asset cannot fall is, in effect, a put option on the asset: an option to sell the asset at the guaranteed price level. In principle, therefore, the value of a portfolio can be insured by buying a put on the portfolio with a strike price equal to the desired floor.

However, the options traded in the 1970s and the early 1980s on the options exchanges of the United States were relatively short-term, there were limits on



the size of position in them that could be accumulated, and they were unsuitable in other ways for the insurance of the value of large, diversified portfolios. “Insuring” a portfolio by buying an exchange-traded put for every stock in it would be very expensive. A put option on a stock index such as the Standard and Poor’s 500 would be a good alternative, but, as noted in chapter 6, the options exchanges were not allowed to trade index options until 1983.

What Leland had seen, however, was that although suitable actual puts were not available, a pension fund or other investor that desired portfolio insurance could use option theory to “manufacture” a synthetic put. Qualitatively, what was needed was to shift between stocks and cash (or government bonds) as stock prices fluctuated, buying stocks as prices rose and selling them as prices fell. If the value of the stock portfolio fell toward its floor, more and more of it would be sold, so the overall impact on the portfolio of further declines in stock prices became less because fewer stocks—and perhaps eventually no stocks—were held. Option theory could provide quantitative guidance as to how to do that in such a way that the desired put would be replicated precisely.

Leland’s idea did not meet quick or easy acceptance. From 1976 to 1978, Leland and Rubinstein set portfolio insurance largely aside. They feared it would be redundant if the SEC approved index options (Rubinstein was involved in the unsuccessful proposal from the Philadelphia Stock Exchange mentioned in chapter 6), and they were wary of disseminating the idea too widely because it seemed in the 1970s as if the design of a financial product could not be protected by a patent (Rubinstein interview).<sup>1</sup>

In 1979, Leland gave talks on portfolio insurance at several banks, “went home[,] and eagerly waited for the phone to ring. It never did.” (Leland and Rubinstein 1988, p. 6) In 1980, however, the idea sparked the enthusiasm of John O’Brien, whose extensive experience in performance measurement meant that his many contacts knew that he “wasn’t just another flim-flam man” (O’Brien interview). In February 1981 the trio established Leland O’Brien Rubinstein Associates, Inc. (LOR) with “two part-time secretaries, one computer, and no clients” (Leland and Rubinstein 1988, p. 7).

More was involved in turning portfolio insurance from an idea to a product than recruiting a credible product-champion, critical as O’Brien was. As noted in previous chapters, Black-Scholes-Merton option theory was based on the assumption that the probability distribution of changes in the logarithms of stock prices was normal. Short-selling (selling borrowed stock) was assumed to be possible without financial penalty, and cash could be borrowed or lent at an identical riskless rate of interest. The volatility of the underlying stock was taken to be known and constant, and it was also assumed that both stocks and

options can be traded without incurring transaction costs (Black and Scholes 1973, p. 640).

Leland, O'Brien, and Rubinstein knew they did not live in what Leland called "a Black-Scholes world" (Leland 1980, p. 580), a world in which market conditions were as posited by option theory. As portfolio insurance was molded from idea to product, the underlying theory was developed to incorporate some of reality's imperfections. Rubinstein was already involved in the collaboration with John Cox and Stephen Ross, referred to in chapter 5, that led to an approach to option theory (Cox, Ross, and Rubinstein 1979) that could be used to model price distributions other than the log-normal.

Leland provided a mathematical analysis of the replication and pricing of options in a world with non-zero transaction costs (Leland 1985) and found a practical solution to the problem that "even a cursory familiarity with the behavior of stocks, as well as stock indexes" showed that constant volatility was "not a realistic assumption" (Leland and Rubinstein 1988, p. 5). Instead of trying to insure a portfolio for a fixed period of time (which was unrealistic because of fluctuations in volatility), LOR offered to insure it for a given number of stock-price moves, for example "five moves (any combination of ups and downs) of 5 percent" (Leland and Rubinstein 1988, p. 6).

During the early 1980s, a growing number of institutional investors contracted with LOR to provide them with instructions to buy or to sell stock in such a way as to replicate a put and thus provide portfolio insurance. The strategy was not always popular with those investors' individual fund managers, who sometimes resented these instructions as outside interference with their investment strategies.

However, as described in chapter 6, in April 1982 the Chicago Mercantile Exchange launched a futures contract on the S&P 500 index, the benchmark most widely used by portfolio managers. The introduction of index futures provided LOR with a far simpler way of implementing portfolio insurance, one that did not require interference with fund managers' holdings.

The close, arbitrage-imposed link between the price of futures and the level of the underlying index made it possible to implement portfolio insurance by buying and selling futures, rather than by buying and selling stocks, at least so long as the portfolio to be insured was highly correlated with the S&P 500, as well-diversified U.S. stock portfolios would be. As noted in chapter 6, a futures position equivalent to a huge position in stock could be constructed by making margin deposits that were a small fraction of the value of the underlying stock.

LOR's customers provided it with access to capital typically amounting to around 4 percent of the value of the portfolio to be insured (Mason et al. 1995, p. 772), and in return for a management fee LOR used this capital to create

and then to adjust a position in the futures market designed to produce the desired synthetic put. LOR would begin by instructing its brokers on the Chicago Mercantile Exchange to sell the quantity of index futures necessary to create the desired initial hedge.

If index levels then fell, LOR would telephone instructions to Chicago to sell more futures; if they rose, it would buy futures. Futures traded in large volumes and could readily be bought and sold with low transaction costs, making “the protection of very large [portfolios] feasible for the first time. . . . As of the end of 1986, roughly 80 percent of the dollar value of LOR accounts was protected using futures.” (Leland and Rubinstein 1988, p. 8)

During the mid 1980s, portfolio insurance became big business. By the autumn of 1987, the portfolio-insurance programs of LOR and its licensees covered \$50 billion of stock (Mason et al. 1995, p. 786) with perhaps almost as much again covered by firms not affiliated with LOR (Voorhees 1988, p. 57). This success, however, began to cause Leland and Rubinstein to have misgivings. Although they had coined the term “portfolio insurance,” they had reservations about it, preferring the broader and more neutral phrase “dynamic asset allocation.” They knew that for all their technical innovations they had not freed themselves completely from the assumptions of a Black-Scholes world.

“The analogy with insurance breaks down,” they warned, if stock prices “gapped” downward, plunging discontinuously: there would not be “sufficient time to adjust the replicating portfolio” (Rubinstein and Leland 1981, p. 72). Such discontinuities were excluded, mathematically, from the Black-Scholes log-normal random walk, but could not be ruled out in practice.

LOR therefore added an “override” check to the Black-Scholes strategy. “Every day we would say ‘if we were to take all the money out of the market and put it in cash and hold it through the expiration date, would we be able to deliver the floor?’” (Rubinstein interview). For certain clients—such as the Aetna Life Insurance Company, which was literally, not just figuratively, insuring portfolios—LOR added “jump protection,” working out whether their positions would meet the conditions of the above override check if markets fell by a set amount (around 6 percent) so quickly that the portfolio could not be adjusted at all (Rubinstein interview).

It was accepted by all involved that, at least in the absence of jump protection, portfolio insurance would fail if a dreadful external event caused the market to fall discontinuously—if, as Leland warned pension fund officials, “one morning, we learn that the Russians have invaded Iran and all the Mideast oil supplies are being cut off” (Leland interview). What gradually

became more salient, however, was a risk “internal” to the markets. In a Black-Scholes world, adjustment of the replicating portfolio does not affect the price of the underlying stock, the market for which was implicitly taken to be large, liquid, and efficient: the Black-Scholes option-pricing equation “assumes that you can’t affect either stock or options prices, by placing orders,” wrote Fischer Black (1990, p. 13). Would that still be the case as portfolio insurance became big business?

Sales by a portfolio insurer as prices fell were an “informationless,” mechanical response to changing prices, so in an efficient market they should not affect stock prices, which should be determined by the tradeoff of risk and expected return discussed in chapter 2. That prices were governed by that tradeoff was the reasoning of Myron Scholes’s Ph.D. thesis, but it was not peculiar to Scholes; it was a fundamental tenet of finance theory.

When portfolio insurance was small-scale, the assumption that the stock and futures markets were external “things” in which prices would not be affected significantly by the insurers’ purchases or sales was plausible enough. But what if portfolio insurance was adopted widely? In January 1983, after attending an LOR presentation, Bruce Jacobs of the Prudential Insurance Company of America wrote in a memo to his employers (reproduced in Jacobs 1999, pp. 301–304) that “if a large number of investors utilized the portfolio insulation technique, price movements would tend to snowball. Price rises (falls) would be followed by purchases (sales) which would lead to further price appreciation (depreciation).”

Jacobs was to become portfolio insurance’s most persistent critic. As portfolio insurance’s scale grew, however, the fear about portfolio insurance’s possible positive feedback effect, its amplification of price movements, started to affect the three men at its heart: Leland, O’Brien, and Rubinstein. “From the very first day I thought of portfolio insurance I said ‘Well what if everyone tries to do it?’ I didn’t like the answer I came up with.” (Leland interview)

By June 1987, the portfolios “insured” by LOR and its licensees were sufficiently large that Leland was pointing out that “if the market goes down 3 percent, which, in those days, would have been a very large one-day move, we could double the volume [of trading] in the New York Stock Exchange” (Leland interview). (Although by then LOR’s portfolio insurance was implemented primarily with futures, index arbitrage would transmit selling pressure from the futures to the stock market.) With sales on that scale, would informationless selling really have no effect on prices?

“We had one client come to us who had a huge pension plan,” says Rubinstein. “We wanted to tell that client that was too much money for us to handle. We were just too worried about the impact that the trading would have

on the markets.” If LOR refused the client’s business, however, “he’d go somewhere else”—to one of the growing number of other firms also offering portfolio insurance (Rubinstein interview).

“It was as if Pandora’s box had been open[ed]” (Rubinstein interview): “we could shut our doors, but that wasn’t going to stop anything” (O’Brien interview). LOR’s principals did not envisage a catastrophic crash—they assumed, as had Jacobs, that “savvy investors” (Jacobs 1999, p. 303) would step in to exploit and thus limit the mispricings induced by positive feedback—but they suspected that market volatility could be increased. “If that’s what people want to do [purchase portfolio insurance],” they thought, “then the market *should* be more volatile. There’s nothing necessarily bad about it” (Rubinstein interview).

Rubinstein’s concerns were, however, brought into focus by a sharp market decline on September 11 and 12, 1986. On September 11, the Dow Jones industrial average fell 4.6 percent, its largest one-day fall for nearly 25 years (SEC 1987, p. 1). The Securities and Exchange Commission (SEC) investigated, and “concluded that the magnitude of the September decline was a result of changes in investors’ perception of fundamental economic conditions, rather than artificial forces arising from index-related trading strategies.”<sup>2</sup>

Rubinstein believed (but could not prove) that the SEC was wrong, and that the pressure of futures selling by portfolio insurers had been critical. He told the SEC, but did not publish his concerns: “for the first time in my career I had a conflict of interests. . . . I wasn’t sure about it and I didn’t want to stick my neck out and do a thing that would have hurt the business.” (Rubinstein interview)

### ***October 19 and 20, 1987***

The events of September 1986 were followed by another sharp decline on January 23, 1987, but those falls seemed minor reversals in a prolonged international bull market that saw the S&P 500 index almost triple between 1982 and September 1987, with a similar rise in London and an even greater rise in Tokyo. The demons of the 1970s—high inflation, oil shocks, bitter labor disputes, stagnation—seemed to be receding, banished by liberalized markets, monetarism, Reaganism, Thatcherism, and by the vogue for aggressive financial management, exemplified by the audacious acquisitions of big, staid corporations by raiders with good access to the market in “junk” (lower-than-investment-grade) bonds.

By the autumn of 1987, however, doubts were growing as to whether the apparent successes of “Reaganomics” (Brady Commission 1988, p. I-11) were sustainable. The trade deficit of the U.S. had ballooned, as had its public debt,

the dollar was under pressure, and there were fears that interest rates would have to rise. On Wednesday October 14, disappointing data on the trade deficit and moves by the Ways and Means Committee of the House of Representatives to remove tax advantages that had contributed to the mergers and acquisitions boom led to what was then the largest number of points ever lost by the Dow Jones average in a single day.

Thursday October 15 was again highly volatile, and Wednesday's fall was exceeded on Friday October 16, when the Dow fell 4.6 percent. That Friday "was living history," one trader told the *Financial Times*. "We have young traders out here with their eyes popping out of their heads at this sight," said another (Anonymous 1987a). Markets internationally also fell, and in Britain even nature seemed to echo the human turmoil. On the night of October 15–16, the worst storm in more than 100 years caused widespread damage across southern England, leaving British markets effectively shut on Friday.

Friday's falls were, however, quickly to pale into relative insignificance. On Monday October 19 the London market fell some 11 percent (Anonymous 1988a, p. 52), and in New York—where the trading day is several hours later than London's, because of the time zone difference—the Dow fell 22.6 percent. It was its largest one-day fall ever, worse even than its worst individual days in the Great Crash: the 12.8 percent fall on October 28, 1929, and 11.7 percent fall on October 29, 1929 (Schwert 1990; Brady Commission 1988, p. 1).

As alarming as the size of the crash were the breakdowns in markets that accompanied it. The printers at the specialists' booths on the New York Stock Exchange could not keep up with the waves of sell orders arriving through the semi-automated DOT (Designated Order Turnaround) system, and there were also serious network delays and software problems (Brady Commission 1988, pp. 48 and VI-47).

Those who tried to sell via telephones often found they could not get through. Some brokers simply left their telephones to ring unanswered; others tried to respond but could not cope with the volume of calls. One NASDAQ broker-dealer reported that "his phone board looked like a disco with every light flashing all day long and even after bringing in additional help from off the trading desk it was just impossible to answer them all" (Brady Commission 1988, p. VI-15).

In Chicago, extraordinarily sharp declines in index futures prices began. Brokers acting for external customers, notably for portfolio insurers, had to implement large "sell orders." As they did, index futures prices plunged far below the theoretical values implied by the levels of the index. Nassim Taleb, in 1987 a trader on the Mercantile Exchange, recalls it this way:

... the crowd detected a pattern of a guy who had to sell [as] the market went lower. So what do you do? You push lower ... and you see him getting even more nervous. ... It's chemistry between participants. And here's what happened. You understand, these guys are looking at each other for ten years. ... They go to each other's houses and they're each other's best friends and everything. Now one of them is a broker. He has an order to sell. They can read on his face if he's nervous or not. They can read it. They're animals. They detect things. So this is how it happened in the stock-market crash. They kept selling. They see the guys sell more. ... (Taleb interview)

Normally, if the futures price fell substantially below its theoretical value, the discrepancy would be corrected by index arbitrage. However, the trading disruptions in New York broke the link that arbitrage established between the stock and futures markets. The S&P 500 and other indices were recalculated virtually continuously: as each New York stock traded, exchange employees completed cards and fed them via optical character recognition readers into the exchange's Market Data System, and computer systems at firms such as Bridge Data and ADP updated index values (Blume, MacKinlay, and Terker 1989).

If significant component stocks in the index were not trading, the calculated index value rapidly became "stale": its relationship to market conditions became indeterminate. Even under normal circumstances, gaps between successive trades of individual stocks and delays in data entry and processing meant that the S&P 500 was "typically about five minutes old" (Rubinstein 1988, p. 39), and that could be consequential economically for an index arbitrageur. On October 19 and 20, however, the disruption of trading meant that the gap between the index and the market it was meant to represent grew dauntingly large.

Furthermore, even if one had the confidence to perform index arbitrage it was not clear on October 19 and 20 that one actually could. S&P 500 futures arbitrage required trading not just individual stocks but large baskets of them. By 1987, Weinberger's technique of trading only a small sample of the stocks making up an index was no longer adequate. (Most of the time, arbitrageurs were seeking to exploit small price discrepancies and so their hedging had to be more precise.) If one had simultaneously to create or to adjust positions in 500 stocks, reliance on human beings alone was problematic. So S&P 500 index arbitrage was normally implemented via the automated DOT system, which allowed member firms of the New York Stock Exchange to identify in advance a basket of up to 500 stocks and then enter buy or sell orders for the entire basket.

The network delays on October 19 hampered index arbitrage, and at 9:30 A.M. on October 20 the New York Stock Exchange imposed what was in effect

a prohibition on use of DOT for arbitrage (Brady Commission 1988, p. III-22). In addition, the ends of the automated chain were human beings: the specialists on the floor of the exchange. It was they who had to turn an index arbitrageur's DOT order, arriving on printers at all the specialists' posts at which S&P 500 stocks were traded, into completed transactions. Many were unable or unwilling to do so. They could not find buyers to match with sellers and, with their own capital evaporating, they feared bankruptcy if they stepped in to remedy the imbalance (as their regulatory obligations said they should).

The breakdown in arbitrage permitted a substantial gap to open between the prices of index futures and their theoretical values. As noted in chapter 1, on October 19 the S&P 500 index fell 20 percent, while the price of S&P 500 two-month index futures fell 29 percent (Jackwerth and Rubinstein 1996, p. 1611). The discrepancy should have led arbitrageurs to buy futures and to short-sell the underlying stocks. It was, however, quite unclear whether that arbitrage could successfully be completed.

For example, Edward O. Thorp, whose work on option pricing was discussed in chapter 5, was involved in the 1980s in index arbitrage via the firm he co-founded, Princeton Newport Partners. He recalls great difficulty in getting his firm's trader even to attempt arbitrage on October 20. He was able to persuade him only by threatening to do it on his own account and telling him "I'm going to hang you out to dry" because the firm would then get no share of the profit. The trader was able to make only around 60 percent of the short sales Thorp had instructed, but Thorp had anticipated this by telling him to attempt twice the theoretical quantity (Thorp interview).

The fact that futures prices plunged far below even the huge falls on the stock market exacerbated fears on the latter, because they were taken as indicative of further declines yet to come. It also caused portfolio insurers to face a difficult dilemma. The price discrepancy could imply that the price of futures was artificially low because of the failure of arbitrage, and insurers should therefore not attempt the enormous sales demanded by put replication. Alternatively, it could mean that the index itself was not an accurate reflection of the state of the stock market, that the even greater fall in Chicago was the more valid measure, and huge sales of futures were the correct response.

Different portfolio insurers reacted differently to the discrepancy. On the morning of Monday October 19, Leland and Rubinstein flew down from their Bay Area homes to LOR's Los Angeles headquarters. The New York Stock Exchange had opened just as Leland boarded his early morning flight, and the flight crew told the passengers that the Dow Jones industrial average had fallen by a serious, but less than catastrophic, 60 points. After the short flight, Leland "asked the cab driver to put on the stock report and the market was down like



300 points at that time. I just said ‘Oh God’” (Leland interview). That fall would be the equivalent of a drop of nearly 1,400 points at the market levels at the time of this writing (July 2005).

At LOR’s offices, the trader who handled its sales of futures warned his bosses “if I try to put on all the contracts . . . I’m convinced the market will go to zero.” The fear of “driv[ing] the markets to closure” (Leland interview), together with the growing discrepancy between the price of futures and their theoretical value, led LOR to slow futures sales. In contrast, Wells Fargo Investment Advisers, another leading portfolio insurer and LOR licensee, ignored the price discrepancy and kept selling futures aggressively (Voorhees 1988, p. 58).

### ***Crisis and Recovery***

Although it was the price falls on Monday October 19, 1987 that hit the headlines, the hours of greatest systemic danger came that night and on Tuesday morning. The events with which I began this book were not public knowledge at the time, but enough leaked out for “rumors about the financial viability” of the Mercantile Exchange’s clearinghouse to sweep the markets on the Tuesday morning (Brady Commission 1988, p. 40). Since the clearinghouse stood behind all the billions of dollars of futures traded by the Merc, it was a central part of the financial infrastructure of the United States, and the possibility of its failure was a source of deep alarm.

As was noted in chapter 1, banks had begun to cut back on credit to securities firms, threatening the latter with bankruptcies. The specialists on the New York Stock Exchange were in a particularly precarious position, and on the Tuesday morning many either did not open trading on the stocks for which they were responsible, or opened it and then quickly suspended it, overwhelmed by orders to sell.

Normally the world’s most liquid private securities, the stocks of America’s great “blue chip” corporations—Du Pont, Sears, Eastman Kodak, Philip Morris, Dow Chemical—could not be traded. At 11:30 A.M. on October 20, trading even in the mighty IBM ceased. In Chicago, S&P 500 futures were in free fall: in  $2\frac{1}{4}$  vertiginous hours, S&P index futures prices fell 27 percent, and at one point they were 18 percent below the theoretical value implied by the index.<sup>3</sup>

With so many of the New York stocks that underlay options not trading, the Chicago Board Options Exchange decided it had to close at 11:45 on Tuesday morning. At 12:15 P.M., Leo Melamed telephoned John J. Phelan, chairman of the New York Stock Exchange, and learned that the NYSE’s directors were

meeting. The details of Melamed's conversation with Phelan are contested, but Melamed inferred that Phelan and his colleagues were contemplating closure. Melamed felt he had to act to protect the Merc from the uncontrollable panic that might ensue if the world's most important exchange shut down, and the Merc closed trading in S&P 500 futures (Brady Commission 1988, p. III-26; Stewart and Hertzberg 1987, p. 23).

Then, at 12:38 P.M., what participants later described as a "miracle" happened (Stewart and Hertzberg 1987, p. 23). Only one index future was still trading: the Chicago Board of Trade's Major Market Index future, its inferior substitute for its desired Dow Jones future, which had been blocked by the legal ruling that an index level was private property, not a public fact. One or more market participants—their identities are still unknown—had begun to buy Major Market Index futures. Their purchases were modest, the equivalent of stock worth no more than \$60 million, but in an illiquid market they forced the sharpest ever rise in the price of those futures.

Later, there was speculation that the purchases were "part of a desperate attempt to boost the Dow and save the markets" (Stewart and Hertzberg 1987, p. 23). If that were so, the choice of instrument was shrewd. Like the other stock-index futures, Major Market futures had been trading far below their theoretical values. Their rapid rise took them to a premium, to prices above theoretical values. As the news reached New York, it was a much-needed fillip to morale. Within a few minutes, at around 12:45 P.M., it was followed by some orders to buy stocks, as arbitrageurs began to restart trading to lock in the profits that the premium offered—despite, as one of them later put it, feeling "terrified of the market" (Stewart and Hertzberg 1987, p. 23). 17 of the Major Market Index's 20 stocks were also in the Dow Jones 30-stock industrial average, so the purchases were largely in the stock of corporations whose prices shaped the publicly most salient market index.

As arbitrageurs' purchases began, the programs, noted in chapter 1, that corporations were announcing to buy back their stock also started to have effects. Gradually, New York's specialists received enough "buy" orders to resume trading. Phelan had wanted to keep the New York Stock Exchange open. That morning he had received a private appeal from the White House not to shut the exchange, and he also feared that closure might be permanent. "If we close it," he later recalled thinking, "we [will] never open it." (Stewart and Hertzberg 1987, p. 23)

Phelan telephoned Melamed and the heads of other exchanges to say that New York would indeed remain open. The Mercantile Exchange then felt confident enough to restart S&P futures trading just after 1:00 P.M. In New York, IBM resumed trading at 1:26 P.M., and by 2:00 P.M. on Tuesday October 20,

with the main “blue chips” all trading again and the Dow stocks rising sharply, it was clear that the immediate crisis had passed. On Wednesday October 21, the recovery broadened into a near-record overall rise: by the close of trading that day, about half of Monday’s losses had been recovered.<sup>4</sup>

Portfolio insurers performed quite credibly in protecting their clients’ “floors” in the exceptional conditions of October 19 and 20. Although slow futures sales meant LOR was “underhedged by 50 percent” on October 19, it “still met its floor for 60 percent of its clients” with the rest of its accounts suffering “floor violations that ranged between 5 and 7 percent” (Voorhees 1988, p. 57).

The clients of other portfolio insurers typically were down “6% or 8% if the maximum targeted loss is 5%” (Anders 1987). Given the huge falls in the overall market, “It was better to have it [portfolio insurance] than not to have had it,” said one client, Robert Mall of the Honeywell pension fund (quoted in Voorhees 1988, p. 57).

The problem, though, was that many portfolio insurance accounts were then “stopped out.” They were in effect completely in cash (the futures sales needed to try to protect “floors” had been equivalent to the entire insured portfolio), and the only way in which LOR or the other portfolio insurers could continue to guarantee the “floor” was to leave them in that condition (see, for example, Rubinstein 1988, p. 40). Unless insurers’ clients were prepared to accept an ad hoc downward revision of their floors, they thus had entirely to forgo the benefits of subsequent stock-price rises. Given the extent to which stock prices recovered, that turned out to be a significant cost.

### ***Explaining the Crash***

The question of the benefits and costs to the clients of portfolio insurers was quickly joined by a more fundamental question: was portfolio insurance implicated in the crash of October 19? The most authoritative of the clutch of official inquiries into the crash was by a Presidential Task Force on Market Mechanisms led by investment banker Nicholas Brady, who was soon to serve under both Ronald Reagan and George H. W. Bush as Secretary of the Treasury. The task force’s report placed considerable weight in its account of the crash on “mechanical . . . selling” by portfolio insurers (Brady Commission 1988, p. v).

Claims that portfolio insurance exacerbated the crash persist (for example, Jacobs 1999). Were the claims correct, the 1987 crash would indeed be an instance of counterperformativity, given that portfolio insurance was an application of Black-Scholes-Merton option theory. The crash was a grotesquely

unlikely event on the log-normal model of stock-price movements underpinning that theory. In terms of the log-normal model, the fall on October 19 of “the two month S&P 500 futures price . . . is a  $-27$  standard deviation event with probability  $10^{-160}$ ” (Jackwerth and Rubinstein 1996, pp. 1611–1612).

Furthermore, the crash involved substantial, discontinuous, downward price movements, not the continuous random walk of the log-normal model. Indeed the crash fits poorly with any standard model of the stochastic dynamics of stock prices: “No study so far has been able to explain the [crash] as a ‘reasonable’ draw from a distribution that also describes the price dynamics during more normal times.” (Timmermann 1995, p. 19)

More generally, the crash is a frequently cited counterexample to finance theory’s claim that stock-price movements are the result of the impact of new information on rational expectations of future returns from those stocks. The 22.6 percent fall in the Dow Jones industrial average on Monday October 19, 1987 was the equivalent of about 2,300 points at its levels at the time of this writing. It is extremely hard to identify “new news” over the previous weekend that would rationally justify such a huge, sudden reevaluation of stocks.

Certainly, the crash took place against a background of deteriorating economic conditions, but knowledge of those conditions was not new. Both efficient-market theory and the “event studies” it spawned suggest that capital markets react almost instantaneously (within minutes, and often within seconds) to relevant news, so earlier events—even events during the previous week—cannot from an efficient-market viewpoint explain Monday’s crash: such information would already have been incorporated into Friday’s prices.

The questions of whether portfolio insurance exacerbated the crash, and if so to what extent, are immensely hard to answer conclusively. The Brady Commission and critics of portfolio insurance could point to a plausible set of mechanisms: initial price declines causing portfolio insurers to sell stocks and futures; index arbitrage transmitting sales pressures from the futures market to the stock market; an “overhang” of uncompleted portfolio insurance sales over the weekend of October 17–18; well-informed traders realizing further sales were inevitable and anticipating them by selling ahead of them; price declines causing further sales by portfolio insurers, and so on.

Detailed analysis by the task force led by Nicholas Brady found that on October 19 portfolio insurers directly sold almost \$2 billion in stocks (nearly 10 percent of that day’s volume of trading on the New York Stock Exchange). They also sold S&P 500 index futures equivalent to stocks worth \$4 billion, more than 40 percent of externally generated trading in those futures (Brady Commission 1988, p. 36).

In the absence of a model of the underlying economic processes, it is hard to assess the significance of the sale of stock and stock index futures worth \$6 billion. The total market value of the stocks of American corporations before the crash was about \$3.5 trillion (Gennotte and Leland 1990, p. 999), so portfolio insurers' sales on October 19 amounted to less than 0.2 percent of the total holdings of stocks. It might seem a tiny proportion, incommensurate with generating such a huge drop in prices. Overall, though, only just over 1 percent of the U.S. market's total capitalization (stocks worth \$39 billion) changed hands during the crash, and that small percentage change in ownership *was* associated with a price decline of more than 20 percent.

If positive feedback had taken place—in other words, if price declines had been amplified by insurers' mechanical sales—one might have expected prices to rebound as investors realized that an “artificial” mechanism had led stocks to be undervalued. A brief rebound on the morning of Tuesday October 20 was overwhelmed by another wave of selling and by the serious market disruption described above, but as already noted prices did rebound in a more sustained fashion on the afternoon of October 20 and on October 21. (By coincidence, the programs announced by corporations to buy back their stock, which were widely seen as significant in the rebound, amounted to the same total—\$6 billion—as the Monday's portfolio insurance sales.<sup>3</sup>)

Since about half of Monday's decline had been reversed by the close on Wednesday, perhaps positive feedback accounts for roughly 50 percent of the crash? That argument is, however, inconclusive: it is, for example, greatly affected by whether one includes the previous week's falls as part of the crash and whether one takes somewhat later, lower prices, rather than Wednesday's rebound, as the benchmark. Both these alternatives would considerably reduce the proportion of the crash that was later “corrected.”

Another way of examining the role played by portfolio insurance in the crash is international comparison. By 1987, the technique was beginning to be adopted outside the United States, but nowhere else had it achieved anything like its scale in the United States.<sup>6</sup> Since all of the world's major stock markets crashed, by amounts often similar to or in some cases even worse than in the United States (Roll 1988), specific features of the U.S. market such as portfolio insurance might seem to be incidental.

The problem, however, is that during the crash price movements in stock markets internationally were highly correlated. “Eyeballing” charts of price movements to see whether the United States led other markets down does not produce unequivocal results,<sup>7</sup> and formal tests of causality only partially disentangle the chain of events.<sup>8</sup> The crash was an international event, but the available evidence does not rule out the possibility (though equally does not

demonstrate) that its extent outside the United States was exacerbated by what happened in the United States

Portfolio insurance is not the only possible candidate cause of the 1987 crash. "Behavioral" finance scholar Robert Shiller conducted a mail survey of investors directly after the crash: his first pilot survey was dispatched before 5 P.M. on Monday October 19. He found that while 5.5 percent of institutional investor respondents employed portfolio insurance, almost as many again were using simpler forms of "stop-loss" strategy in which stocks are sold when prices fall below a set threshold, and 10 percent of wealthy individual investors also had stop-loss strategies (Shiller 1988, p. 291). The effects of such strategies would have been similar to those of portfolio insurance: investors would have tried to sell stocks as prices fell.

There had also been discussion before October 19 of the possibility that the stock-price rises of the 1980s would end in a crash akin to that of 1929. John Kenneth Galbraith contributed an article titled "The 1929 Parallel" to the *Atlantic Monthly* (Galbraith 1987). The October issue of the *Atlantic Monthly*, on American newsstands as the crash began to unfold, warned: "America has let its infrastructure crumble, its foreign markets decline, its productivity dwindle, its savings evaporate, and its budget and borrowing burgeon. And now the day of reckoning is at hand." (Peterson 1987, p. 43; see Shiller 1988, p. 292)

On the morning of October 19, the *Wall Street Journal* published a chart with the movements of the Dow Jones industrial average in the 1980s superimposed on those of the 1920s. The article's text was reassuring: "Wall Street analysts . . . argue that much has changed in the intervening decades to make the stock market—and the economy—more stable." (Anonymous 1987b) However, any reader that Monday morning who ignored the reassurance and extrapolated 1987's prices using the 1929 graph would have been led to expect a crash remarkably similar to what was going to take place in the hours to come (Koning n.d.).

To the extent that fears of a crash had been widespread in the months preceding October 1987—and Shiller's survey suggests they were—they would help to explain the growing popularity of portfolio insurance, and also would add an element of self-fulfilling prophecy to the October events: large numbers of investors who feared a crash responded to price declines by all running for the exit and finding "it was large enough to accommodate only a few" (Brady Commission 1988, p. 57). The Brady Commission concentrated its attention on large sales by big investors, notably by portfolio insurers, but the breakdown of the technical mechanisms of the markets—swamped telephone lines and the failures of the DOT system, for example—"came from urgent selling by

the large number of smaller investors rather than from the small number of larger investors” (Bernstein and Bernstein 1988, p. 176).

Given the lack of conclusive evidence, and the presence alongside portfolio insurance of other strategies and of a broader mindset that would have been similar in their effects, it is therefore difficult to improve on Hayne Leland’s admirably candid and properly tentative judgment. Portfolio insurance “certainly didn’t start a crash,” he says, “because we were a reactive strategy, but we may well have contributed in some degree to the size of the fall. The ‘some degree’ was a 3 percent contribution or a 60 percent contribution. I’m not sure.” (Leland interview)

It is also perfectly possible that to inquire into the causes of the *crash* is to ask the wrong question. The fundamental challenge posed by the October events to efficient-market theory is to explain why such large price movements took place in the absence of major new information about economic prospects and the situation of America’s corporations. From this viewpoint, it may be the rebound in the afternoon of October 20 and on October 21 that is more challenging to explain than the price declines on October 19, for which reasonably plausible explanations, broadly compatible with economic orthodoxy, can be found.<sup>9</sup>

Wednesday October 21, however, has received almost no analytical attention (the Brady Commission’s analysis, for example, stops with October 20), presumably because it is sharp stock-market declines, not sharp rises, that are regarded as undesirable and thus in need of explanation. It is, for example, quite unclear that the announced programs for the re-purchasing of stock were sufficient to account for the rise.

The question of what caused the crash of October 19 was important as events unfolded, not just retrospectively: how participants behaved on the Tuesday and Wednesday was affected by their beliefs as to the cause of Monday’s events. Thorp, for example, “went home that night [Monday October 19] to think about it . . . thinking through the numbers, knowing how much portfolio insurance was on and how much selling had to be due to portfolio insurance being adjusted. It was a very large number. . . . I realized what had happened. . . . It was portfolio insurance.” This analysis gave him the confidence to undertake the arbitrage, described above, that others did not attempt. “When the disconnect [between the futures and stock markets] was understandable it wasn’t quite so fearsome. You could see it was going to go away again” (Thorp interview).

It is also possible that the search for the *causes* of the crash is mistaken. For example, Timmermann’s verdict, quoted above, on the difficulty of explaining October 19 “as a ‘reasonable’ draw” may reflect exploration of too limited a

class of stochastic processes. Perhaps “wilder” forms of randomness account for sudden, huge price discontinuities interrupting prolonged periods of limited fluctuation. Benoit Mandelbrot was moved to return to the study of finance, after many years on other topics, by the 1987 crash (Mandelbrot interview). “Think of a ruler held up vertically on your finger,” the geophysicist-turned-finance-scholar Didier Sornette suggests. To ask which hand movement or gust of air causes its collapse is to miss the point. “The collapse is fundamentally due to the unstable position; the instantaneous cause of the collapse is secondary” (Sornette 2003, p. 4).

### ***Reconstructing a Black-Scholes World***

Given the ambiguity of the evidence and the depth of the underlying issues, it is interesting that the developers of portfolio insurance do not take the easy option of denying that it had a significant role in the 1987 crash. Leland and Rubinstein are both broadly “orthodox” economists and efficient-market theorists (see, for example, Rubinstein 2001). How do they reconcile this with their acceptance that it “isn’t ridiculous to say that portfolio insurance was a significant factor in the market crash” (Rubinstein interview)?

From the viewpoint of this book, the most relevant explanation is Leland’s, first presented in a December 1987 typescript (Leland 1987)<sup>10</sup> and then developed with postdoctoral researcher Gérard Gennotte (Gennotte and Leland 1990).<sup>11</sup> Its bearing on the issues discussed in this book is that it is an analysis directly tied to the question of how, in Leland’s words, to “design the market” so that “crashes can be avoided” even in the presence of large-scale portfolio insurance (Leland 1987).

Apart from in one feature to be discussed below, Gennotte and Leland’s is a “rational-expectations” model: it posits investors who have a correct understanding of the price dynamics presumed in the model. The model is of the determination by supply and demand of the price,  $p_o$ , of a single risky asset, which can be “interpreted as the stock market portfolio” (Gennotte and Leland 1990, p. 1006). Part of the demand for the asset comes from “uninformed investors . . . who observe only  $p_o$ ”—in other words, whose only source of information about the future price of the asset is inference from its current observed price (Gennotte and Leland 1990, p. 1002). Part of the supply of the asset results from the activities of portfolio insurers, who sell increased quantities of it as its price falls. That supply is  $\pi(p_o)$ , a deterministic, decreasing function of  $p_o$ .

Because portfolio insurance can create positive feedback (as the price of the risky asset falls, portfolio insurers will sell more of it), price discontinuities or



‘crashes’ can occur” (Gennotte and Leland 1990, p. 1008). The main determinant of their likelihood in Gennotte and Leland’s model is whether or not  $\pi(p_o)$ , the function describing the extent of portfolio insurers’ sales at different price levels, is known to the economic agents posited by the model. (The possibility that  $\pi(p_o)$  is known to no participant is the key departure from a full rational-expectations model.)

Sales of the asset by portfolio insurers will lead to lower prices, but if  $\pi(p_o)$  is not known—in other words, if investors do not know the proportion of sales that are “mechanical” responses to lower prices—price falls will be greater “as a consequence of investors inferring information from prices. A supply shock leads to lower prices, which in turn (since the shock is unobserved) leads uninformed investors to revise their expectations downward. This limits these investors’ willingness to absorb the extra supply and causes a magnified price response” (Gennotte and Leland 1990, p. 1001).

When Gennotte and Leland set the parameters of their model to values roughly corresponding to the U.S. stock market in 1987, the effects of whether or not  $\pi(p_o)$  is observed were dramatic. “The unobserved hedging which created a 30 percent crash in market prices, would have less than a 1 percent impact on prices if it were observed by all investors” (Gennotte and Leland 1990, p. 1016).

For Leland, then, the key factor in the 1987 crash was not portfolio insurance per se but lack of awareness of the true extent of portfolio insurance’s “mechanical” sales. “If everybody knows that we’re uninformed traders, then people don’t revise their expectations downward when the price falls. They just say things are on sale. Then they will take the other side [i.e. buy] more willingly. If everyone thinks the price is falling because somebody has information, then they won’t take the other side.” (Leland interview) The price fall will then be much larger.

The explanation of the 1987 crash posited by Leland and Gennotte is thus that mechanical sales were misinterpreted as implying that “something terrible was happening . . . that there was something fundamentally wrong” (Leland interview). That process could account for developments in the United States and could also explain the extent to which price falls were transmitted from the U.S. market to other markets, as investors in those markets inferred gloomy economic prognoses from declines in the U.S.

Leland and Gennotte’s explanation is consistent with widespread reports of investor fear in October 1987. Shiller’s questionnaire asked respondents whether during the crash they experienced “symptoms of anxiety” such as “sweaty palms” or “tightness in chest.” A fifth of individual investors, and two-fifths of institutional investors, reported experiencing such symptoms on

October 19 (Shiller 1989, pp. 388–389). Almost all were aware of falling prices during that day—Shiller’s institutional respondents “checked the prices of stocks” an average of 35 times on October 19 (1989, p. 388)—and those falls provoked emotion consistent with there being “something fundamentally wrong.”

“There was panic,” reports John O’Brien: “[In] my observation . . . people were as panicked in brokerage houses as they were two weeks ago [September 11, 2001].” (O’Brien interview) Had investors understood that sales were mechanical and known how large they were going to be—LOR heard rumors that “suggested portfolio insurance trading was going to be three or four times larger than in fact it was” (Norris 1988, p. 28)—they might have felt less afraid.

I quoted above Thorp’s testimony that the “disconnect” between the futures market and stock market “wasn’t quite so fearsome” once he understood the price moves of October 19 to have been the effect of portfolio insurance. However, investors in general did not have the knowledge Thorp had of the likely proportion of sales that had been by portfolio insurers. Such investors may well have believed “the big, bad wolf was there . . . that some catastrophe would befall America that the smart people were on to” (O’Brien interview).

Unlike many other discussions of the 1987 crash (for example, by the Brady Commission), Leland and Gennotte’s explanation of the role of portfolio insurance in the crash involves an explicit economic model. Ultimately, however, it is no more provable (or disprovable) than other explanations of the crash. It has, however, an important aspect: its explicit link to “sunshine trading,” a means of repairing the rational market.

The idea of sunshine trading predated the crash. It emerged from a conversation between John O’Brien and the futures broker Steven Wunsch of Kidder Peabody, who were discussing the fact that LOR’s futures trades were large and that they *had* to be made. O’Brien puts it this way: “If the market drops this much, we have to trade. It’s not a matter of us saying ‘Well, gee, this is not a good time to trade.’” The solution O’Brien and Wunsch came up with “was something [for which] we then came up with the name ‘sunshine trading.’ We said, what we’ll do, we’ll go down to the floor of the futures exchange, we’ll announce an hour beforehand that we’re going to sell \$10 million of futures at 11 o’clock.” (O’Brien interview<sup>12</sup>)

The obvious objection was that a sunshine trader would be “front run”: others would sell futures ahead of them, in order to profit from a decline in price brought on by the pre-announced sale. However, the proponents of sunshine trading reckoned that if news of the intended sale was disseminated widely, competition among would-be front runners would tend to eliminate the adverse price effects of front running.

Indeed, sunshine trading can in a sense be seen as an attempt to free portfolio insurers from the ways in which the bodily social structures of Chicago's open outcry pits create channels of information flow such as that described by the Chicago Mercantile Exchange trader Lewis J. Borsellino:

As I walked into the S&P pit [on October 22, 1987] a few minutes before the opening bell, I noticed the brokers who filled customer orders seemed nervous and edgy. I had been an order-filler myself. . . . I remembered well the nervous anticipation of having a big order to fill at the opening. That's what I saw across the pit that morning. I could see it in the way their eyes darted around them and the uneasy fidgeting. . . . They were sellers, I decided at that moment. (Borsellino 1999, p. 6)

In the instance described by Borsellino, it took to the following day for the seller's identity to become known via the Chicago rumor mill: it was the speculator George Soros. However, the activity of regular, predictable customers such as portfolio insurers quickly becomes identifiable to traders in an open-outcry pit via local knowledge of which brokers act for them. Observing the behavior of those brokers—when they enter and leave the pit; their bodily demeanor within the pit; their trading—can give floor traders “an advantage because they know something that nobody else does” (Leland interview).

LOR's pre-crash experiments with sunshine trading were successful: “We made 13 large trades this way . . . and it was our belief that, on average, we got better prices.” (Rubinstein interview) The goal at that point was to reduce LOR's transaction costs, but after the crash Leland and O'Brien saw sunshine trading as attractive for another reason. Instead of pre-announcing just one trade, portfolio insurers could make known “the table of trades that we would make at various market levels” (O'Brien interview), in effect publishing Gennotte and Leland's  $\pi(p_o)$ .

If Gennotte and Leland's account of the crash were correct, disseminating knowledge of portfolio insurers' sales in advance would greatly reduce the risk of those sales destabilizing the market. With  $\pi(p_o)$  not known, large sales produced “shock . . . nobody knowing how big . . . how much more would be coming.” In contrast, with  $\pi(p_o)$  known the market could “prepare itself” for portfolio insurers' informationless trades and “clear . . . just fine” (O'Brien interview; see also Grossman 1988, pp. 278–279).

Sunshine trading could, in other words, be seen as an attempt to reconstruct a Black-Scholes world, to recreate a world in which the mere placing of “informationless” orders did not affect prices. Another attempt at repair—a more ambiguous one—was advocacy of a shift away from continuous stock trading via New York's specialists, trading which had broken down to an almost disastrous extent on October 20, and of a move to discrete stock auctions, held perhaps four times daily.

In a stock auction, “all the buy and sell orders [would] be congregated in one place and adjusted by the people who are putting these orders in to a point that they can be cleared at a single price, and all buyers and sellers receive that price” (John O’Brien, quoted on p. 26 of Norris 1988). The main advocate of single-price auctions was Wunsch, but the suggestion was also supported by Leland and O’Brien. Its ambiguity in relation to a Black-Scholes-Merton world is that this world assumes trading that is continuous in time, but what was more important to portfolio insurers was continuity in price: the avoidance of large, discontinuous gaps.

New York’s specialists were supposed to provide price continuity (no large gaps between successive prices), but had failed to do so. The moment that seems to have triggered the near-disaster of Tuesday October 20 was when a sudden, brief rebound that morning gave way to another precipitous fall.

The initial rebound was based on very partial information, says Leland: “there were very thin orders left. If the [specialists’] book [of buy and sell orders] had been public, I am convinced those prices would not have opened up with as much of a gap as they actually did.” In contrast, a “single price auction essentially allows everyone to see the full sets of supply and demand.” From a portfolio insurer’s viewpoint, “it is sort of like sunshine trading, in the sense that we would put in our entire order demand, and everybody would see that” (Leland, quoted by Norris 1988, p. 28).

However, neither sunshine trading nor single-price stock auctions were successful reforms in the United States.<sup>13</sup> Sunshine trading foundered on the objections of Chicago floor traders, who claimed that its pre-announced trades would be “prearranged” trades, which were illegal. Despite the failures of 1987, the basic structure of the New York Stock Exchange remained unchanged (although at the time of writing in 2005 it was under threat following allegations of malpractice), but small specialists’ firms were generally taken over by investment banks and other bigger, better-capitalized institutions. Wunsch helped to set up the Arizona Stock Exchange, based around electronically conducted, discrete auctions rather than continuous trading via specialists, but the Arizona Exchange was eventually unable to compete successfully with its entrenched rivals.

So LOR’s proposals to redesign markets so as to minimize the unwanted effects of portfolio insurance came to little. That mattered less than it might have, because after October 1987 the market for the type of portfolio insurance sold by LOR, its licensees, and its competitors dwindled rapidly. In part that was because the costs of portfolio insurance in situations of high volatility (forgone gains and risks of being “stopped out”) became evident, but it may also have been because the managers of “respectable” institutions such as

pension funds wished to avoid overt pursuit of a strategy that was “tainted” by its association with the crash.

It was not that the desire for what portfolio insurance promised—a floor to losses—vanished. Instead, those who wished such a floor seem to have turned from portfolio insurance’s synthetic puts to actual puts. Such puts were purchased either “over-the-counter” (by direct institution-to-institution negotiation) from investment banks or bought on organized options exchanges, notably the Chicago Board Options Exchange.

The use of real rather than synthetic puts might seem to make little difference, because the vendors of such puts have to hedge the risks involved in their sale, and this may involve constructing the same replicating portfolio as needed for a synthetic put. However, if the puts in question are traded on an organized, public exchange, their purchase makes visible the demand for portfolio insurance. Furthermore, both investment banks and options traders may need to hedge only a portion of their apparent exposures, because some of those exposures “cancel out” others.

In addition, the price of actual puts has to be paid explicitly and “up-front,” rather than primarily by forgone gains, as in portfolio insurance. As we shall see, after 1987 the price of the index puts most likely to be attractive as a floor to losses has consistently been high—higher than it would be in a Black-Scholes world. These “up-front premium” costs could cause “sticker shock” (Voorhees 1988, p. 58), limiting the scale of the purchase of puts.

On October 13, 1989, U.S. markets again crashed. Though the 7 percent fall was only one-third of that two years previously, it sparked renewed anxious analysis. This analysis, however, showed that the “insurance” uses of over-the-counter puts had been small compared to portfolio insurance in 1987. Portfolios worth only about \$2 billion had been protected by over-the-counter puts, a tiny fraction of portfolio insurance’s \$60–\$90 billion coverage (SEC 1990b, p. 25; Voorhees 1988, p. 57).

### ***The Volatility Skew***

The overall consequences of the 1987 crash for the U.S. economy were remarkably limited. Central parts of the financial system broke down, but only briefly. The rise in stock prices of the 1980s was reversed only temporarily (the 1990s were to see an extraordinary boom), and even in the short run the effects of the reversal on the wider economy were not huge. The crash did have its effects, and those effects have persisted, but they are not evident on the surface.

Consider, for example, the Chicago Board Options Exchange in 1999–2000 (the period of the Chicago part of the fieldwork for this book). Much of what

was to be seen then on the exchange's trading floor was a continuation and intensification of the developments discussed in chapter 6.

In the 1980s, human beings had been still at the center of the market, and technical systems were their aids. By 2000, the balance had shifted. The Chicago Board Options Exchange traded 104,000 classes of option. Inevitably, trading in some was quiet—in 2000, despite record overall trading volumes the average class was trading only 13 contracts per day—but the exchange's Autoquote system, now pervasive, continuously generates prices for all classes of options, no matter how sporadic the actual transactions in those classes, and distributes those prices to the vast array of electronic displays on the trading floor and through worldwide computer networks.

Autoquote prices are firm, at least for public orders of modest size. If those orders can be filled at these prices, the contract can be executed without human intervention: the other side is assigned at random to "a market maker who has volunteered to accept such orders" (Options Institute 1999, p. 241). Most market makers now carry hand-held computers, linked by infrared and microwave communication links to the Chicago Board Options Exchange's central systems. These register the automated fill of an order, and if the market maker wishes to hedge the resultant position (as now most would wish to do), the difficulties described in chapter 6 are no more: the hand-held computer calculates the requisite hedge, and the trader can make the necessary stock purchases or sales simply by punching buttons.

Such was the array of screens, computers, and communication systems on the Chicago Board Options Exchange's trading floor at the end of the 1990s that other heating was needed only when the Chicago temperature dropped below  $-10^{\circ}\text{F}$  (Options Institute 1999, p. 232). Dotted inconspicuously among all this automation were the touch screens used to set Autoquote working. Once these screens were used to set parameters such as volatility, the Autoquote system received stock prices and index levels from other markets, and, as these inputs changed, continuously updated option prices.

Two words at the top of each screen revealed how Autoquote updated prices: "Cox-Ross." The Cox-Ross-Rubinstein model discussed in appendix E was used to generate prices. Human beings remained in ultimate command: by pressing on the section of the screens marked "lean," traders could manually override model values, and, furthermore, the ebb and flow of orders in actively traded option classes did move prices. Nevertheless, such human interaction was now merely one aspect of a larger technosystem.

It could reasonably be said of this technosystem that it performed theory. That the model employed was Cox-Ross-Rubinstein, rather than Black-Scholes-Merton, was not in itself significant: as noted in appendix E, the latter

is a limit case of the former. By 2000, however, a crucial aspect of the Black-Scholes-Merton model was almost never present. As we saw in chapter 6, in that model the relationship between the strike price and the implied volatility of options on the same underlying asset with the same time to expiration is a flat line. In the fall of 1987, however, the flat-line relationship, empirically manifest in the late 1970s and the early 1980s, disappeared, and was replaced by a distinct “skew.”

Figure 7.1 illustrates the shift in typical pattern for index options. The earlier flat line has not returned subsequently (see, for example, Jackwerth 2000); indeed, the skew continued to grow at least until 1992 (Rubinstein 1994, pp. 772–774). The direction of the skew has been stable: puts with strike prices well below current stock index levels have higher implied volatilities—are relatively more expensive—than puts with higher strike prices, and the put-call parity relation discussed in chapter 6 means that the same is true of calls. Similar, but less intense, skews are also to be found in options on individual stocks (Toft and Prucyk 1997, p. 1177).

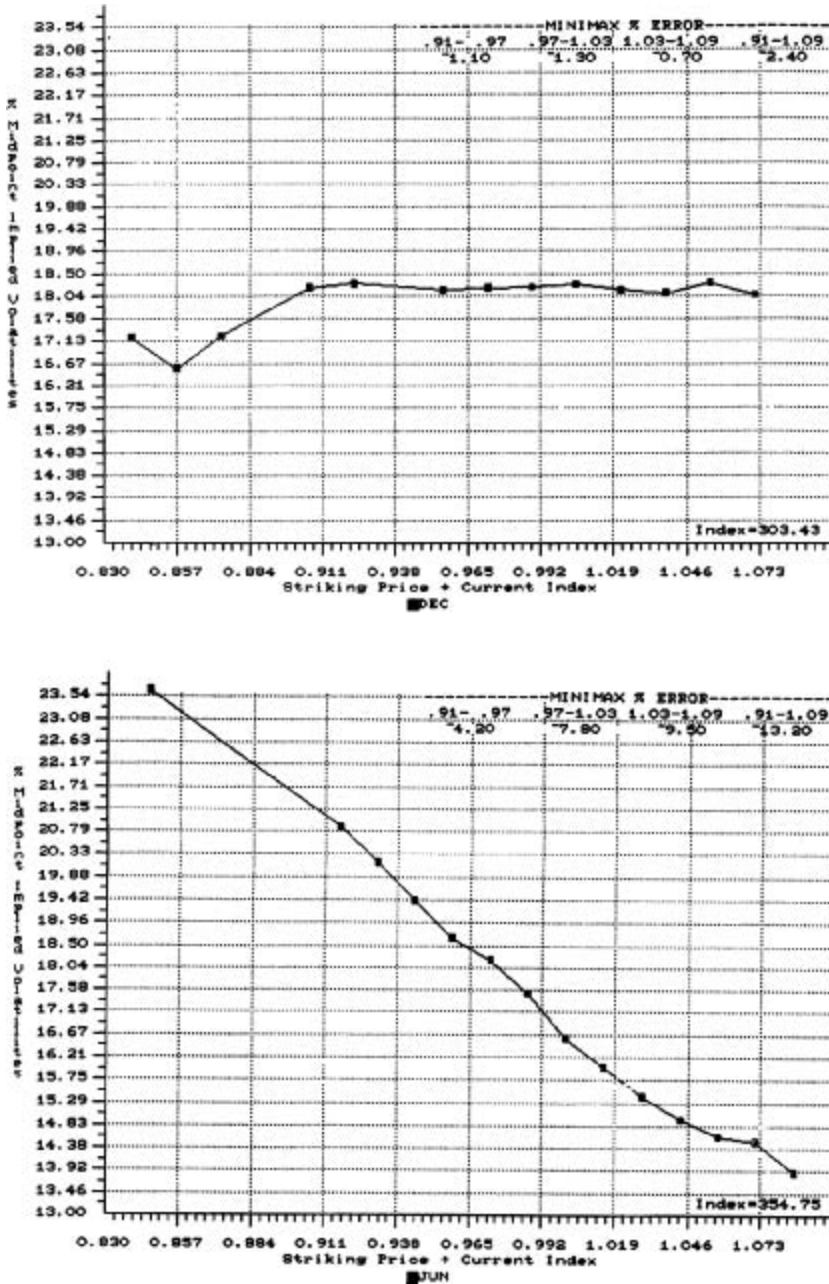
The empirical history of option pricing in the United States, therefore, falls into three distinct phases. The first two phases were described in chapter 6. Before the April 1973 opening of the Chicago Board Options Exchange, and in the first year or so of its operation, there were substantial differences between patterns of market prices and Black-Scholes values. A second phase had begun by 1976 and lasted until summer 1987. In it, the Black-Scholes-Merton model was an excellent fit to market prices.

The third phase in the empirical history of option pricing is from autumn 1987 to the present, when the Black-Scholes-Merton model’s fit has again been poor, especially for index options, in the crucial matter of the relationship between strike price and implied volatility. The Black-Scholes-Merton flat-line relationship vanished, and it has not returned.

There is little doubt that the 1987 crash is the event that separates the second and third phases of the history of option pricing, and there appears to be econometric consensus. No analysis now finds the Black-Scholes-Merton model to fit the observed pattern of prices of options well; all find a volatility skew that is variable in nature but, in general, has in the case of options on stock indices roughly the same pattern as in the lower graph in figure 7.1; and the skew is evident both in the index options traded on the Chicago Board Options Exchange and in the options on index futures traded on the Chicago Mercantile Exchange.<sup>14</sup>

Thus, what is now performed in Chicago is no longer classic option-pricing theory. That theory has, as was noted in chapter 6, become part of the market’s vernacular: whenever participants talk of “implied volatility,” they implicitly





**Figure 7.1**

Volatility skew of Chicago Board Options Exchange S&P 500 index options, 9:00 A.M., July 1, 1987 (upper graph), and 10:00 A.M., January 2, 1990. Source: Rubinstein 1994, pp. 776-777. Courtesy of Mark Rubinstein and Blackwell Publishing Ltd.



draw on it. However, a layer that mixes market processes and practitioner know-how is laid upon that communicative idiom, and upon the algorithms of the Cox-Ross-Rubinstein model.

On the morning of every trading day on the Chicago Board Options Exchange, “Designated Primary Market Makers” set the skew for the options for which they are responsible. As the underlying stock price or index value changes, Autoquote uses this skew and the Cox-Ross-Rubinstein model to generate prices. If trading conditions shift markedly, the primary market maker can reset the skew during the day, but this by no means always happens.

The skew has become arguably the central cognitive aspect of options trading:

... when experienced traders ... move to a different pit ... say they're trading [options on] telephone [stocks] and now they're going to trade AOL [America Online], the first thing they want to know when they walk into a pit is, “What's the skew like?” To them that tells them lots. And it's the most vital information, more than what's the potential earnings of AOL and who their competitors are and any fundamental kind of stuff; it's, well, “How does this pit trade this thing?” ... They still have competing bids and offers. ... So ... if you look at the actual skew relative to prices it's kind of choppy, but if you look at it consistently it really does kind of become ... a function that's relatively smooth. ... It's amazing, people just talk about volatility skew, I mean, that, that is the market. And the sophistication of how that [the skew] changes. ... Different ... people will say “This is what I think the skew should be,” “It's a little bit different today; is it enough different for a trading opportunity?” ... “Here's how I think that skew will react to different scenarios.” ... So people certainly have their opinions. But what the floor in general seems to [believe] is ... “Here's what the consensus kind of [skew] is.” (Hinkes interview 1)

### ***The Skew and the Memory of the Market***

Unfortunately, the econometric and trading-floor consensus *that* the skew exists does not extend to consensus as to *why* it exists.<sup>15</sup> The skew seems more extreme than can be accounted for simply by the extent to which empirical price distributions depart from the log-normal assumption of the Black-Scholes-Merton model. Furthermore, the index-option and index-future option prices that have prevailed since 1987 seem to present profit opportunities: for a prolonged period, excess risk-adjusted profits seem to have been available.

The econometrician David Bates concludes that “the substantial negative skewness and leptokurtosis<sup>16</sup> implicit in actively traded short-maturity option prices appear fundamentally inconsistent with an absence of large weekly movements in S&P 500 futures returns over 1988–93” (Bates 2000, p. 227). “The parameter values necessary to match the smirk [skew] in S&P 500 futures

options appear inconsistent with the time series properties of the stock index futures and of option prices" (Bates 2003, p. 399).

Of course, the level and pattern of option prices might reflect not just the empirical distribution of changes in index levels or in index-future prices but the incorporation into option prices of the possibility of a catastrophic but low-probability event that did not in fact take place. In other words, prices might have incorporated the *fear* that the 1987 crash would be repeated.<sup>17</sup> This possibility has been explored by financial economist Jens Jackwerth (working in his case with the prices of index options, not those of index-future options). Remarkably, Jackwerth (2000) shows not only that excess risk-adjusted returns were available from the sale of index puts, but that the strategy remained profitable even when artificial crashes of 20 percent in a month were introduced into the computation with probabilities as high as one every four years.<sup>18</sup>

To explain observed index-option prices since 1988 thus requires the artificial addition to the actual price record of crashes of almost 1987's severity more frequent than one every four years, when "even a 20% [one month] crash every eight years seems to be a rather pessimistic outlook" (Jackwerth 2000, p. 447). (Between 1802 and 1988 there were only seven calendar months in which the U.S. stock market fell by more than 20 percent [Schwert 1990, p. 80, table 1].) "The most likely explanation," Jackwerth concludes (2000, p. 450), "is mispricing of options in the market."

Of course one should be wary about extrapolating from trading strategies that "work" in econometricians' studies to conditions in actual markets. However, what market participants reported to me in interview was consistent with Bates's and Jackwerth's analyses. Since 1987, healthy returns could be earned in practice by selling index options, for example by "betting against the skew," in other words by selling low-strike-price puts with their high implied volatilities.

If Jackwerth is right, and options are indeed mispriced, this is a striking finding: participants in one of high modernity's most sophisticated markets have been behaving irrationally. Such a conclusion, however, seems to me to involve too narrow a view of rationality. Because of the limited wider effects of the 1987 crash, it is hard now to recapture just how traumatic it was to those most centrally involved. The arbitrageur Eric Rosenfeld, then at the investment bank Salomon Brothers, recalls "sitting at the [trading] desk and wondering about the end of the whole financial system" (quoted in Dunbar 2000, p. 97).

Others acted on the feeling that the financial system was on the brink of collapse. The trader Marty Schwartz had bought S&P index futures contracts on Friday October 16, 1987. As the market plunged on the Monday he

realized he had to sell, even at a huge loss: “I got out of most of my positions and protected my family.” He then began taking action to protect himself and them against the possibility of bank failures: “. . . at 1:30 P.M. [on October 19, 1987], with the Dow down 275 points, I went to my safe deposit box and took my gold out. Half an hour later, I went to another bank and started writing checks to get my cash out.” (quoted in Schwager 1993, p. 268)

Fearing that the 1987 crash might have been exacerbated by the widespread adoption of portfolio insurance, Mark Rubinstein entered two weeks of what he now regards as clinical depression. He could not rid himself of the fear that the weakening of the American markets could tempt the Soviet Union to a challenge to the United States akin to the one that had provoked the Cuban missile crisis, and nuclear war might ensue (Rubinstein interview).

Nowhere was the trauma of 1987 more intense than in Chicago’s derivatives markets. The Mercantile Exchange’s clearing problems were described in chapter 1. In only slightly less dramatic circumstances, John Hiatt and his colleagues at the Options Clearing Corporation stayed awake not just on Monday October 19 but also on the next two nights, and then spent the entire following weekend in their offices dealing with a particular problem case (Hiatt interview 1). Option prices rose to levels that defeated the nascent automated systems, many of which could cope only with double-digit dollar prices. When option prices rose to \$106, “it appeared on your sheets as . . . \$6. . . . So your account [with the clearing firm] was off by \$20 million the next day. . . . Nobody knew where they stood.” (Hull interview)

The sudden, huge rise in the prices of index put options—some rose eightfold (Brady Commission 1988, p. VI-19), with implied volatilities, normally around 20 percent or less, soaring to 170 percent (Bodie and Merton 1995, p. 217, figure 6-1)—meant that enormous margin payments were owed to clearing firms by market makers who had sold options that had shot up in value, and by the firms to the Options Clearing Corporation. First Options, the leading clearing firm, absorbed huge losses from market makers who could not meet their obligations. Its failure would have been a calamity, but it had shortly before been bought by Continental Illinois, and the Federal Reserve permitted the bank to draw on its capital reserves to replenish First Options’ funds.

What was learned in October 1987, therefore, was more than that stock markets could suddenly fall by previously unthinkable amounts: it was also that the consequences of such a fall could threaten the very existence of derivatives markets. Nor were all the effects of the crash short-lived; it was 1995 before stock options trading on the Chicago Board Options Exchange recovered to pre-crash volumes (CBOE/OCC 1998a, p. 12). It is this collective trauma, I conjecture, that sustains the skew.

It is, for example, noteworthy that the skew in index options appears initially to have been largely an American phenomenon.<sup>19</sup> The British options market, for example, does not seem to have responded to 1987 by developing a similar skew (Gemmill 1996). Although stock markets in the United Kingdom and elsewhere also crashed, the effects on the then-tiny British derivatives markets were nowhere near as devastating as in the United States. When a consistent skew did emerge in the U.K. in the first half of the 1990s, it seems to have been only a third as intense as in the U.S. In Japan, too, the skew may have been less marked (Gemmill and Kamiyama 2001).<sup>20</sup>

When asked to explain the skew, my American interviewees differed in the mechanisms they cited, but they all led me back to October 1987. As I noted above, 1987 revealed the desirability of *owning* index puts: the synthetic puts offered by portfolio insurance had in many cases failed fully to protect investors. The nature of the market in options on index futures, for example, changed radically after the 1987 crash. Before the crash, transactions in calls were generally more frequent than transactions in puts; from 1988 on, transactions in puts were nearly always more numerous (Bates 2000, p. 188, figure 3).

Simultaneously, however, 1987 made clear the dangers of *selling* puts.<sup>21</sup> Exiting a position in puts was hard. While the owners of calls that had risen in value might well sell them—allowing market makers who had sold calls to exit their positions by buying them back—the owners of puts would, if they had bought them for insurance purposes, want to keep them.

Furthermore, to hedge index options by using the Black-Scholes “delta hedging” procedure (outlined in chapters 5 and 6) might well be insufficient if an index fell sharply: it might not be possible to adjust the hedge fast enough. In a Black-Scholes world there are no “jumps”—no discontinuous price movements—and no changes in volatility. 1987, however, taught options market makers that indices could go down effectively discontinuously while volatility could leap upward. If that happened, “delta hedging” would fail adequately to protect a market maker who had sold puts.

Both “jump risk” and “volatility risk” can be hedged by purchasing options, but doing so adds to a market maker’s costs. In addition, as Bates (2003, p. 400) points out, hedging “jump risk” and “volatility risk” by buying options “just transfers the risk to another market maker.”

While no analysis of the skew is yet definitive, the explanation most consistent with my interviews is Bates’s suggestion that the index options markets in the United States have come to resemble the markets in catastrophe risk:

The stock index options markets . . . appear to be functioning as an insurance market. A large clientele of assorted institutional investors predominantly buy stock index

options, whereas a relatively small number of specialized option market making firms predominantly write them. These firms can delta-hedge their positions against small market fluctuations, but are perforce exposed in aggregate to jump and volatility risks. . . . Consequently, market makers limit their exposure to these risks by risk management techniques such as value at risk [see glossary] and worst-case scenario analysis. The limited capitalization of the market makers implies a limited supply of options, and the pricing of the extra risks spanned by options at the shadow prices of investors with relatively high aversions to those risks. (Bates 2003, p. 400)<sup>22</sup>

Why are the careful, specialized market-making firms described by Bates, with their desirable but expensive product (index puts with high relative prices), not undercut by new entrants to the options market? Such new entrants might well be tempted by the high returns that are offered by the sale of puts, and might not be too concerned by the risks that their activities pose to others: to their clearing firms and to the clearing system?<sup>23</sup> I put the issue in the form of a less-than-elegant question to Timothy F. Hinkes, a leader in the design of risk-management systems for the options exchanges, and I received a blunt response:

MacKenzie: I mean, presumably a guy out there just selling puts, selling deep out-of-the-money<sup>24</sup> puts sort of hand-over-fist is. . . .

Hinkes: We call them the shit-sellers! (Hinkes interview 1)

The moral obloquy arises because the 1987 crash taught the options market that unrestrained, unhedged or inadequately hedged put sales threatened not just the seller but the market as a whole. A collective response to the danger came in two phases. First, options clearing firms themselves changed their risk-management practices in response to 1987, for example by starting to check the consequences for each trader who cleared via them of a repetition of the crash (Hinkes interview 2).

Next, the overall risk-management procedures built into the margin and capital requirements for U.S. options exchanges were altered. As a consequence,

. . . the shit-sellers cannot trade the way they used to. Either their capital requirement will be higher than it was or the clearing firm will pay more margin to [the Options Clearing Corporation] than they used to. . . . The exchanges . . . wanted better risk-management tools than the old systems had allowed but they didn't want to kill the market either. . . . [The new systems] gave them [the clearing firms] ammunition to rein in traders that they were always nervous about. The problem before was . . . if you sat on a guy too hard he'd go somewhere else [to a different clearing firm]. And you can make a lot of nice money in two or three years when there's not a crash. And a lot of nice fees for the clearing firm. [But] when there's a kind of new minimum floor . . . you can be tough on a guy and he can't go somewhere else and get a hugely better deal. (Hinkes interview 1)

In consequence, a shift took place in “who supplied that disaster insurance [by selling puts, especially index puts with low strike prices]. The individual traders were not able with their capital to continue to do that. It became the O’Connors [O’Connor and Associates and similar market-making firms] and so on that stepped in.” (ibid.)

Individual memory of the 1987 crash and of the possible systemic risks posed by put sales is fading as older traders are replaced by a new generation. However, there is a sense in which that memory has been institutionalized in the U.S. options markets’ risk-management systems. One delicious nuance is that part of that institutionalized memory is now Mandelbrot’s Lévy distributions, which were, as discussed in chapter 4, the “path not taken” by financial economics.

In 1991, the Options Clearing Corporation (OCC), the collective clearing-house for all the U.S. options exchanges, began to use Lévy distributions as the basis for setting margin requirements. Those requirements depend on the “margin interval,” which is determined by the statistical distribution of “one-day price movement[s] for each class of option” (SEC 1990a, p. 148):

Our [OCC’s] research found that [Lévy] distributions provided estimates of future distributions that were much more consistent (stable) over time than those that either ignored higher moments than variance or relied solely on past extreme instances that had occurred during a past data sampling window. While on average the typical stock alpha was about 1.65, there was a wide distribution about that average (1.3 to virtually 2.0). So OCC uses stock specific values for all four parameters of a stable [Lévy] distribution (including non-symmetry) and assesses the critical tail values (1% and 99% cutoffs) individually. This led to different likely movements across stocks and larger down moves than up moves as well.<sup>25</sup>

“Talking to finance professors” about the use of Lévy distributions, the Options Clearing Corporation “always [got] the ‘Oh, not that infinite variance stuff’” (Hinkes interview 1). Nevertheless, the use of Lévy distributions in setting margin requirements institutionalizes the conviction that extreme events are far more likely than on a log-normal model: “We’re setting our estimates of how much an underlying [stock, index, exchange rate, and so on] could move at a 99 percent confidence level. In standard deviation terms, we’re actually out at five to six standard deviations, which is obviously well beyond what [the] normal distribution would assume.” (Hinkes interview 1)

There is an intriguing counterperformative aspect to this use of Lévy distributions. They are, as noted in chapter 4, a form of “wild randomness.” Tim Hinkes, their key Chicago proponent, believes however that by assuming wild randomness one reduces the impact, and possibly even the likelihood, of disastrous manifestations of such randomness: “we want to set [a] relatively high

bar [margin requirements] and relatively consistent bar for all our firms” (Hinkes interview 1).

Even quite substantial fluctuations in market volatility have limited, slow effects on statistical estimators of the parameters of Lévy distributions with infinite variance. In calm periods, estimates of the probabilities of extreme events do not go down much, so margin requirements are not reduced substantially. When extreme events do happen, Lévy estimators are also affected only relatively modestly, because Lévy distributions with infinite variance “assume” that the probabilities of such events happening are substantial.

In consequence, the use of Lévy distributions means that extreme events do not suddenly increase margin requirements, which would be what would happen if the latter were based on more conventional econometric estimation. The danger of a sudden increase in margin requirements is that it may trigger forced sales, worsening the events that have caused the increase: “Suddenly there is some volatility in the market and suddenly your value-at-risk [see glossary] blows up, and ‘Oh, now I’ve got to do something.’ So [I’ve] got to liquidate. . . . It’s almost likely a secondary feedback effect that adds to the volatility.” (Hinkes interview 1)

The 1987 crash has not been repeated. Up to the time of writing, no subsequent event (not even the attacks of September 11, 2001) has presented the danger to the financial system it posed. Yet 1987 has left its traces. It ended the period of the Barnesian performativity of classic option-pricing theory and replaced it with a seemingly permanent volatility skew.

In Chicago, where a Black-Scholes world was performed, a radically different world is now institutionalized in risk-management techniques: a world of discontinuous price movements, of jumps in volatility, and even of Mandelbrot’s monsters. The goal is not performativity but counterperformativity: to assume “wild” randomness in order to lessen the chance of its manifesting itself.

Although not repeated, 1987 has been echoed. In August and September 1998, another crisis engulfed significant parts of the global financial system. That crisis was probably the most substantial subsequent threat to the system. During it, for example, stock-index implied volatilities hit their post-1987 peak—higher than in September 2001.<sup>26</sup> The 1998 crisis is the central topic of chapter 8.

This chapter examines the background to and activities of the hedge fund Long-Term Capital Management (LTCM) and the causes of the turmoil that engulfed it in 1998.<sup>1</sup> LTCM was highly skilled: it emerged from the celebrated arbitrage group at Salomon Brothers—a group headed by John Meriwether, widely acknowledged as the most talented bond trader of his generation. LTCM was well versed in finance theory—it was run by, among others, Robert C. Merton and Myron Scholes. It was hugely successful.

Nevertheless, in August and September 1998, in one of the defining moments of the economic history of the 1990s, adverse price movements drove LTCM to the brink of bankruptcy. In the midst of a growing global crisis, it was re-capitalized by a consortium of major banks coordinated by the Federal Reserve Bank of New York.

LTCM practiced arbitrage on an unprecedentedly large scale. Its trading was “arbitrage” in the sense in which the term is used in financial markets: it sought to make low-risk profits by exploiting discrepancies in prices, for example when an unduly large “spread” had opened up between the prices of similar assets.

The “arbitrage” invoked in finance theory differs from LTCM’s activities in two respects. First, it demands no capital: it can be performed entirely with borrowed cash and/or borrowed securities. (See, for example, the hypothetical options arbitrage trades in appendix E.) Second, it involves no risk. These are, indeed, precisely the posited features of arbitrage that make its capacity to close price discrepancies unlimited.

LTCM’s activities, in contrast, involved risk (even in “normal” times, not just in 1998) and demanded at least modest amounts of capital. Nevertheless, as we shall see, aspects of LTCM’s trading were quite close counterparts to some of the classic arbitrages of finance theory.



***Bonds, Derivatives, and Arbitrage***

The core of the group that formed LTCM came together at Salomon Brothers in the 1980s. Founded in 1910, and for decades excluded from Wall Street's informal "establishment," Salomon developed a reputation for robust competitiveness and for expertise in underwriting and trading in bonds (Sobel 1986). The bonds that governments such as those of the United States, the United Kingdom, France, and Germany issue in their own currencies are regarded as the safest of investments: the chance of default is conventionally regarded as zero.

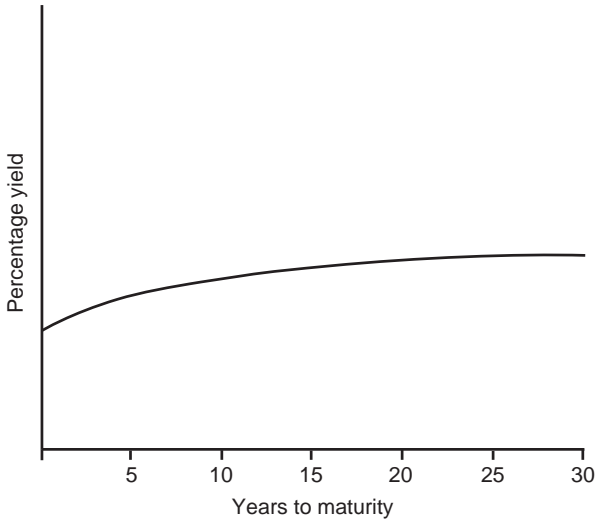
However, the safety of government bonds does not preclude trading opportunities. Indeed, the U.S. bond markets of the 1980s attracted aggressive traders. The expanding government deficits during the presidency of Ronald Reagan meant that increasing numbers of Treasury bonds had to be issued. Trading volumes increased more than fivefold between 1980 and 1988, to levels in excess of \$100 billion a day (Hughes 2004).

Bond prices are related intimately to the level of interest rates. Bonds typically offer fixed "coupons," or interest payments. When interest rates go up, bond prices usually go down (lower prices mean that the "yields" of bonds, the lifetime rates of return they offer at their current market price, go up). If one can predict the future course of interest rates better than others can, one can make money by trading bonds, though in a market like that in U.S. Treasury bonds genuine inefficiencies of this kind appear to be rare.<sup>2</sup>

More subtly, however, anomalies can arise in the pricing of bonds, and these anomalies sometimes become large enough that sophisticated traders can exploit them profitably. For example, the market in newly issued ("on-the-run") U.S. Treasury bonds is more liquid than the market in less recently issued ("off-the-run") bonds: many off-the-run bonds are in the relatively static portfolios of pension funds and insurance companies. Investors concerned with liquidity are therefore prepared to pay a premium for on-the-run bonds.<sup>3</sup>

With the passage of time, however, an on-the-run bond will inevitably become off-the-run, so there may be money to be made by short-selling newly issued bonds and buying their closest off-the-run counterparts. Their yields can be expected to converge, and, crucially, one is insulated from the effects of general rises or falls in interest rates because such changes will affect the prices of both bonds roughly equally.

There is a complex relationship between the yields of bonds and the time remaining to maturity (repayment of the capital sum), a relationship usually summarized by the "yield curve" (figure 8.1). Generally the curve is expected to be reasonably smooth, as in the figure, so if there are "bulges" (for example,



**Figure 8.1**

A hypothetical example of a yield curve (highly schematic). Yield curves usually (but not always) have the upward slope shown here. Source: MacKenzie 2005. Courtesy of Oxford University Press.

if the yield on bonds with five years to maturity is greater than the yields on either three-year or seven-year bonds) an arbitrage opportunity might again exist.

Adding to the arbitrage opportunities offered by the government bond market are those offered by a variety of closely related markets. One is in mortgage-backed securities. In the United States, in order to improve the supply of mortgage funds, federal agencies provide what market participants often take to be implicit government guarantees for bond-like securities backed by pools of mortgages. The prices of these securities, like those of government bonds, have a tight relationship to interest rate movements, but the holder of a mortgage-backed security has also to consider the risk of mortgage pre-payment, which replaces a stream of future interest payments by a sudden return of the capital lent.

Because of the risk of pre-payment, and because the federal bodies involved are agencies, not the government itself, mortgage-backed securities trade at a discount to (or, to put it in other words, offer higher yields than) government bonds. Typically, the yield of mortgage-backed securities is about one percentage point higher than the yield of government bonds, and there can, for example, be profit opportunities if that difference widens or narrows for temporary reasons.

From the mid 1970s on, the arbitrage opportunities offered by bonds and mortgage-backed securities were expanded by the emergence of markets in derivatives of these securities. The Chicago Board of Trade began trading futures on mortgage-backed securities in October 1975 and futures on U.S. Treasury bonds in August 1977. Markets also began to develop in derivatives (such as bond options and swaps) sold “over the counter”—that is, by direct, institution-to-institution negotiations. (Swaps are contracts to exchange income streams, such as fixed-rate and floating-rate interest on the same notional principal sum.)

The proliferation of bond derivatives offered both greater complexity and new possibilities for profitable trading by those who could grasp that complexity. The focal point of Salomon Brothers’ New York headquarters was “The Room,” a huge two-level sales and trading floor (Sobel 1986, pp. 116–117, 160–161). It was a tradition at Salomon Brothers that the managing partner (Bill Salomon from 1963 to 1978, John Gutfreund from 1978 to 1991) managed the firm largely from a desk in The Room.

Complementing Salomon’s trading focus was a group of researchers who concentrated on the bond market, particularly Sidney Homer (see, e.g., Homer 1978), Henry Kaufman (see, e.g., Kaufman 2000), and Martin Leibowitz (see, e.g., Homer and Leibowitz 1972; Leibowitz 1992). Salomon’s tradition was one of “roughneck traders who grew up in the back office, with great instincts” (Meriwether interview), but in the late 1970s and the 1980s there was an increasing emphasis on the recruitment of individuals who combined trading instincts with academic training.

Among the recruits were many of the future principals of LTCM: first John Meriwether, then Larry Hilibrand, Richard Leahy, Victor Haghani, Eric Rosenfeld, Greg Hawkins, and Bill Krasker. At first Meriwether and his colleagues (many of whom Meriwether had hired) focused on simple arbitrage trades, such as on-the-run/off-the-run, but increasingly they performed more complicated trades that required not only the instincts of a “roughneck” trader but also mathematical sophistication.

To identify arbitrage opportunities involving mortgage-backed securities, for example, one has to examine the extent of the “spread” of their yields over government bonds after taking into account the mortgage holders’ prepayment option. It is also necessary to work out how to hedge the pre-payment risk, for instance by purchasing interest-rate options.

The growing complexity of arbitrage led to an increasing connection between Salomon’s proprietary trading and finance theory. Bonds are more complicated than stocks from the viewpoint of mathematical modeling. There is no single dominant model of interest-rate fluctuations equivalent to the

log-normal random walk for stocks. The value of a bond at maturity is a deterministically fixed, not a stochastic, sum of money, and yield curves are both complex and subject to radical changes in shape. Nevertheless, while the financial economics of the markets in bonds and bond derivatives did not achieve the canonical status of the stock and stock-derivative models discussed in earlier chapters, in the 1970s theoretical progress began to be made in the modeling of bond prices.<sup>4</sup>

As bond derivatives developed beyond bond futures to encompass a variety of bond options, the skills of those trained in finance theory became an increasingly important source of competitive advantage for Salomon Brothers. However, it is important not to overstate the sophistication of the application of theory or the criticality of particular models, as popular accounts of the Salomon/LTCM group (Dunbar 2000; Lowenstein 2000) do. In 1984, for example, Meriwether recruited to Salomon Eric Rosenfeld, an assistant professor at the Harvard Business School whose Ph.D. work had been supervised by Robert C. Merton. In addition to his arbitrage trading between the market in bonds and the market in bond futures, Rosenfeld helped a group selling bond options to design and to price their products.

Rosenfeld developed straightforward empirical models of the yield curve, and he priced bond options simply by assuming that the probability distribution of the price of the bond at the expiry of the option was log-normal. “Sometimes,” he recalls, “we’d assume normal just to make it even more simple.” Rosenfeld’s academic work had been much more sophisticated, but there would have been little point in carrying over this sophistication. “We used so much simpler models than I had been used to,” he says. “. . . And . . . I don’t think it mattered. We weren’t out in a region where the particular specification of the model mattered.” (Rosenfeld interview)

At first the arbitrage activities of the Salomon group had focused exclusively on the United States. But as other countries also began to deregulate their financial systems, arbitrage opportunities began to appear in capital markets overseas. Japan, for example, partially liberalized its financial system in the 1980s, and Salomon became heavily involved in arbitrage involving convertible bonds. (A convertible bond is one that includes an option to exchange it for another asset, in most cases for stock of the corporation that has issued it. Often such bonds trade at prices different from those implied by the value of the option as calculated by Black-Scholes or other models of option pricing.) The bank made almost \$1 billion in two years of arbitrage trading of this kind in Japan (Meriwether interview).

As time passed, large and obvious arbitrage opportunities diminished, first in the United States and then elsewhere. By 1986, realizing the need for greater

sophistication in order to keep ahead of his competitors, Meriwether had developed “a pronounced game plan to interact with academia,” indeed to “evolve into a quasi-university environment” (Meriwether interview). He sent Salomon employees to visit universities and to attend the conferences of the American Finance Association. By the late 1980s, Eric Rosenfeld and his colleagues were no longer modeling the yield curve by empirical curve fitting; they were using the more sophisticated models that had begun to appear in the academic literature (Rosenfeld interview).

However, the use of mathematical models by Meriwether’s group played only a limited part in his growing reputation as the best bond trader of the period. At least equally important was his understanding of the institutional structure of the bond market: its “embedding,” as the Granovetterian tradition in economic sociology would put it (Granovetter 1985). A successful arbitrage trader had to attend not only to mathematical models but also to the institutional determinants of supply and demand for bonds: who held which bonds and why, which bonds were readily available and which might suddenly be in short supply, and so on. The mere existence of a price discrepancy was not sufficient to persuade Meriwether to put a trade on: he had to feel satisfied that he knew *why* the discrepancy existed. Among the reasons this kind of institutional understanding was necessary was the possibility of a “short squeeze,” which, though only occasionally the result of deliberate action, is in some ways reminiscent of the grain “corners” described in chapter 1. Typically, one leg of a bond arbitrage trade is constructed by short-selling a particular class of bonds (often government bonds of long maturity). Especially if others have the same or similar trades on, maintaining the ability to borrow the requisite bonds can become difficult and expensive,<sup>5</sup> wiping out the profit from the trade and possibly forcing it to be liquidated at a loss. Such “squeezeability” might not appear as a feature of mathematical models, but was an ever-present risk of which bond-market arbitrageurs had to be aware. “Mathematics was helpful,” says Meriwether, but the kind of understanding of the institutional structure of the market that comes only from experience was—precisely as the Granovetterian tradition would predict—“more important” (Meriwether interview).

As important as understanding the risks arising from the institutional structure of the bond market were financing and obtaining the necessary positions. Arbitrage trading involves trying to profit from pricing discrepancies that often correspond to a difference in yields between similar assets of a fraction of a percentage point. For example, in the 1990s the difference in yields between the on-the-run and the most recent off-the-run 30-year U.S. Treasury bonds was seldom much more than a tenth of a percentage point, and often much

less (Krishnamurthy 2002, p. 465, figure 2). Arbitrage trading therefore inherently involves leverage: the use of borrowed capital to increase rates of return to the point at which they become attractive.

The capacity to borrow money in order to buy securities is thus critical to the practical conduct of arbitrage. (Borrowing is also an essential feature of finance-theory models that invoke arbitrage. As was noted above, it allows the latter to be modeled as demanding no capital.) In bond trading, the most important form of leverage is “repo,” a way of borrowing money to buy securities such as bonds and using those securities as collateral for the loan. (See appendix F.)

In Rosenfeld’s judgment, “a major thing that John [Meriwether] did was making [repo] an integral part of our business” (Rosenfeld interview). It was critical to know what could be “repoed” and on what terms. Typically, lenders do not lend the full price of the securities being repoed; they impose a “haircut” to protect themselves against the risk of borrowers defaulting in a situation in which the market value of the loan’s collateral has fallen. In the U.S. government bond market, “haircuts” usually are modest (around 2 percent), but they can be larger for other securities, and in critical situations they can increase sharply.

Repo, Rosenfeld recalls, was not a prominent or a prestigious business: “In the 1970s and 1980s, it wasn’t done by the top people at the firm; it was . . . almost like a clerk’s job.” Rosenfeld and his Salomon colleagues “always spent a lot of time with those guys and that was very important to us.” Equally important was discovering what bonds could be borrowed for short sale, and on what terms. The members of Meriwether’s group kept in close contact with others at Salomon who knew “if they had any bonds that . . . looked like they were going to be there for a long time that we could borrow. And then we’d sell them and buy the cheap assets against it.” (Rosenfeld interview)

As Salomon’s arbitrage activities began to expand overseas, Meriwether—who, like the traders at Dimensional Fund Advisors, was a good practical economic sociologist—realized that it would not be enough simply to send Americans, however sophisticated mathematically, into overseas markets. “Knowing the culture,” he says, “was more important than just quantitative knowledge.” (Meriwether interview)

Typically, Salomon would seek to recruit people who had been brought up overseas, train them in New York, and then send them back to the markets in the countries in which they had been brought up. The head of Salomon’s trading activities in Japan, the legendarily successful Shigeru Miyojin, is an instance. Someone who was not fluent in Japanese would be at a disadvantage, and in Japan (as elsewhere) the price discrepancies that were of interest

to arbitrage would typically be “driven by the tax and regulatory framework.” An outsider would often find that framework hard to comprehend in sufficient depth (Meriwether interview).

### ***Long-Term Capital Management***

LTCM, which began trading in February 1994, was based in Greenwich, Connecticut. It also had an office in London and a branch in Tokyo. Its primary registration—like that of many other hedge funds—was in the Cayman Islands.

LTCM’s offices were not ostentatious (its Greenwich head office, for example, was a modest, low-rise suburban office block), and the partnership was not large (initially, 11 partners and 30 employees; by September 1997, 15 partners and about 150 employees). These people, however, managed considerable assets: in August 1997, \$126 billion, of which \$6.7 billion was the fund’s own capital. Whereas most hedge funds cater to wealthy individuals, such individuals were the source of less than 4 percent of LTCM’s capital, which came mostly from financial institutions, particularly banks (Perold 1999, pp. A2, A22).

LTCM’s basic strategy was “convergence” and “relative-value” arbitrage: the exploitation of price differences that must be temporary or that have a high probability of being temporary. Typical were its many trades involving “swaps.” By the time of LTCM’s crisis, its swap book consisted of some 10,000 swaps with a total notional value of \$1.25 trillion.<sup>6</sup>

As has already been noted, a swap is a contract to exchange two income streams—for example, fixed-rate and floating-rate interest on the same notional sum. Swaps are a recent invention—they date only from the early 1980s—but they have become important financial derivatives, widely used to manage the risks of interest-rate fluctuations. Around 47 percent of the \$273 trillion in total notional amounts of derivatives contracts outstanding worldwide at the end of June 2004 was made up of interest-rate swaps.<sup>7</sup>

The “swap spread” is the difference between the fixed interest rate at which swaps can be entered into and the yield of a government bond with a similar maturity denominated in the same currency. Swap spreads can indicate arbitrage opportunities because the party to a swap who is paying a floating rate of interest while receiving a fixed rate is in a situation similar to that of someone who has borrowed money at a floating rate and used it to buy a bond that pays fixed interest. If there is enough of a discrepancy between the terms on which swap contracts can be entered into and on which positions in bonds in the same currency and of similar maturities can be financed, arbitrage may be possible. (A typical LTCM swap-spread arbitrage is described in appendix G.)

Several features of swap-spread arbitrage are typical of LTCM's trading. The first is leverage. LTCM's swap-spread trades were highly leveraged—that is, they were constructed largely with borrowed capital. In the trade discussed in appendix G, LTCM's position amounted to \$5 billion. The capital required by LTCM to construct this position was, however, only around \$100–\$125 million: a “haircut” of around \$50 million, and \$50–\$75 million for “risk capital” (provision for adverse price movements). The leverage ratio of the trade—the ratio of the total position to the amount of LTCM's own capital devoted to the trade—was thus in the range from 40:1 to 50:1. While not all the fund's positions were as highly leveraged as that, its overall leverage ratio between June 1994 and December 1997 fluctuated between 14:1 and 31:1 (Perold 1999, pp. A22, C12).

High levels of leverage, however, did not necessarily imply huge risk (as much subsequent commentary suggested). For example, the risks of swap-spread trades are rather limited. Bond prices and the terms on which swaps are offered fluctuate considerably, particularly as interest rates vary. LTCM, however, almost always neutralized that risk by constructing “two-legged” trades, in which the effects on one “leg” of a change in interest rates would be canceled by its equal-but-opposite effect on the other “leg.” (The trade in appendix G is an example.) The chief “market risk” of swap-spread trading is of the spread temporarily moving in an unfavorable direction, but if that happens the arbitrageur can simply continue to hold the position and wait until liquidating it becomes profitable.

Indeed, a swap-spread position such as that described in appendix G can be held until the bond matures and the swap expires. That feature was taken to be the essence of convergence arbitrage: if held to maturity, a convergence arbitrage position *has* to make a profit, whatever the market's fluctuations along the way.

Any “credit risk” (risk of default) associated with swap-spread arbitrage like the trade in appendix G is typically small. The risk of the U.S. government's defaulting on its bonds is regarded as negligible; bond futures contracts are guaranteed by the clearinghouse of a derivatives exchange such as the Chicago Board of Trade; and LTCM's swap contracts were typically with major banks. Even major banks may fail, but because the principal sum in a swap is not exchanged, it is only notional and is at no risk: the credit risk involved is only of the loss of future net differences between fixed-rate and floating-rate interest.

Although the risks were limited, the profits from LTCM's swap-spread trading were impressive. The trade described in appendix G earned a profit of \$35 million, which was a return of 28–35 percent achieved in eight months or less. Nor was this untypical. Between February and December 1994,



LTCM's returns before fees were 28.1 percent (un-annualized); after management and incentive fees were deducted, investors received 19.9 percent (un-annualized). Gross returns in 1995 were 59.0 percent, and returns after fees 42.8 percent; in 1996, the corresponding percentages were 61.5 and 40.8.<sup>8</sup>

Although LTCM was active in the American and Japanese markets, it had particularly heavy involvement in European markets. In the 1990s, financial deregulation in Europe proceeded apace, but arbitrageurs such as LTCM initially found much less competition than in the United States or Japan. "The Japanese banks," according to Costas Kaplanis (who in 1998 was Salomon Brothers' head of global arbitrage), "were the ones who were terribly interested in setting up proprietary [trading] desks. The European banks were still a bit hesitant." (Kaplanis interview)<sup>9</sup>

LTCM scrutinized the "yield curves" for European government bonds and the corresponding swap curves, looking for the "bulges" and other anomalies that might indicate arbitrage opportunities. If LTCM was confident that it understood the reasons for anomalies (often they were matters such as regulatory requirements that caused insurance companies to hold bonds of particular maturities), it would seek to exploit them by trades carefully constructed to neutralize the risks of interest-rate fluctuations or of changes in the overall steepness of the yield curve.

For example, LTCM became heavily involved in the Italian capital markets, which in the late 1990s became a particularly important site of trading, not only by LTCM but also by leading U.S. investment banks. Traditionally, the fiscal efficiency of the Italian state was regarded as poor by international (and many local) investors, who would therefore purchase Italian government bonds only at low prices, and thus at high yields. Until 1995, a 12.5 percent withholding tax on bond coupon payments added to the unattractiveness to international investors of Italian bonds. The tax was refundable, but getting it refunded took time and "back-office capability" (Muehring 1996, pp. 72–73).

The high yields of Italian government bonds contributed to Italy's budgetary difficulties by making the cost of servicing its government debt high. However, with growing European integration, and especially with the prospect of economic and monetary union, arbitrageurs began to believe that Italy's capital-market idiosyncrasies might be temporary. This belief may have been self-fulfilling, in that the resultant flow of capital into Italian government bonds, and the consequent reduction of debt-service costs, helped Italy qualify for monetary union under the Maastricht criteria.<sup>10</sup>

Besides diversifying geographically, LTCM diversified from bonds, bond derivatives, and interest-rate swaps into other asset classes. Some of its relative-value trades involved pairs of stocks, such as Royal Dutch and Shell

Transport. Until 2005, Royal Dutch stocks were traded in Amsterdam and the corresponding American Depositary Receipts were traded in New York, while Shell stocks were traded in London, but the 1907 agreement that created the Royal Dutch/Shell group made the two sets of stocks equivalent entitlements to the income of what was essentially a single entity.<sup>11</sup> The Royal Dutch/Shell group's net income was simply split in a fixed ratio between its two component companies. Nevertheless, the actual ratio of the price of Royal Dutch stock to that of Shell stock was often not the ratio implied by this split. Two sets of stocks that were rights to equivalent income streams were thus trading at inconsistent prices, for reasons that seem to have to do with matters such as the different ways in which dividends paid to different categories of investor were taxed (Froot and Dabora 1999).

When LTCM took a position in Royal Dutch and Shell stocks, the discrepancy in the prices was not big enough, if it remained unchanged, for an arbitrageur to profit simply by holding a short position in the more expensive stock (Royal Dutch) and an equivalent long position in the cheaper one (Shell). The "dividend pickup" income from doing that was more than canceled by the costs of financing the position. However, LTCM believed that forthcoming changes in U.K. tax law would remove much of the reason for the lower relative price of Shell stock (Perold 1999, p. A9). By taking the matched short and long positions, LTCM therefore expected to profit from an expected change in relative value while being protected from overall stock-market fluctuations, from industry-specific factors such as the price of oil, and even from the performance of Royal Dutch/Shell itself.

Another stock-related position, taken on by LTCM in 1997, responded to an anomaly that was developing in the market for stock-index options with long expirations. Increasingly, banks and other financial companies were selling investors products with returns linked to gains in stock indices but also with a guaranteed "floor" to losses. Long-maturity options were attractive to the vendors of such products as a means of hedging their risk, but such options were in short supply. The price of an option is dependent on predictions of the volatility of the underlying asset, and market expectations of that volatility ("implied volatility") can be deduced from option prices using option theory.

In 1997, the demand for long-expiry options had pushed the volatilities implied by their prices to levels that seemed to bear little relation to the volatilities of the underlying indices. Five-year options on the S&P 500 index, for example, were trading at implied volatilities of 20 percent per year and higher, when the volatility of the index itself had for several years fluctuated between 10 percent and 13 percent, and the implied volatilities of shorter-term options were also much less than 20 percent per year. LTCM therefore sold large

quantities of five-year index options, while hedging the risks involved with index futures and sometimes also with short-expiry options (Perold 1999, pp. A7–A8).

Not all of LTCM's trades were successful. For example, Eric Rosenfeld recalls that LTCM "lost a lot of money in France in the front end [of the bond yield curve]" (Rosenfeld interview). Nevertheless, extremely attractive overall returns were earned, and the volatility of those returns was reassuringly low. Most of LTCM's positions were almost completely insulated from overall increases or decreases in the prices of stocks or bonds. The firm had only limited involvement in areas where the chance of default was high, such as "junk bonds" (lower-than-investment-grade corporate bonds) and "emerging markets" (e.g., Russia, Thailand, Argentina).

The risks involved in LTCM's positions were carefully calculated and controlled using the "value-at-risk" approach, a standard practice of the world's leading banks (Meriwether interview). Value-at-risk is a measure of the exposure of a portfolio to adverse price movements. In the case of the dollar swap spread, for example, historical statistics and judgments of likely future values led LTCM to estimate that the spread had an "equilibrium value" of around 30 basis points, with a standard deviation of about 15 basis points per annum (Rosenfeld interview; a "basis point" is a hundredth of a percentage point). Using those estimates, it was then possible to work out the relationship between the magnitude of possible losses and their probabilities, and thus to work out the value-at-risk in the trade.

When a trading firm holds a large number of positions, the estimation of the probabilities of loss in individual positions is less critical to overall value-at-risk than estimates of the correlations between positions. If correlations are low, a large loss in one position is unlikely to be accompanied by large losses in others, so aggregate value-at-risk levels will be modest. In contrast, if correlations are high, then when one position "goes bad," it is likely that other positions will also do so, and overall value-at-risk will be high.

LTCM's positions were geographically dispersed, and in instruments of very different kinds. (See table 8.3 below for an example of the typical range of its major positions.) At the level of economic fundamentals, little if anything connected the spread between U.S. government bonds and mortgage-backed securities to the difference between the prices of the stock of pairs of companies such as Royal Dutch and Shell, the idiosyncrasies of the Italian bond market, the bulges in the yen yield curve, or the chances of specific mergers' failing. LTCM was aware that its own and other arbitrageurs' involvement in these diverse positions would induce some correlation, but nevertheless the observed correlations, based on five years' data, were very small—typically 0.1 or lower.

The standard deviations and correlations that went into LTCM's aggregate-risk model were, however, not simply the empirically observed numbers; they were deliberately conservative estimates of future values. The observed standard deviation of the U.S. dollar swap spread, for example, was around 12 basis points a year, while, as noted above, the risk model assumed it would be 15 (Rosenfeld interview). Past correlation levels, likewise, were "upped" to provide a safety factor: despite observed correlations being 0.1 or less, LTCM was "running analyses at correlations at around 0.3" (Meriwether interview).

The consequence of conservatism in LTCM's modeling was that while the firm's risk model suggested that the annual volatility of its net asset value would be 14.5 percent, in actuality it was only 11 percent (Meriwether interview). Both of these percentages were considerably lower than the risk level—20 percent—that investors had been told to expect (Perold 1999, p. A11).

Of course, such statistical analyses of risk assumed the absence of catastrophic events in the financial markets. The partners in and several of the employees of LTCM had reason to be aware of the possibility of such events. David W. Mullins Jr., who joined LTCM after serving as Vice Chairman of the Federal Reserve and as Assistant Secretary of the Treasury, had been Associate Director of a presidential task force that had produced a report on the 1987 stock-market crash (Brady Commission 1988). Gérard Gennotte had co-authored the analysis of the crash (Gennotte and Leland 1990) mentioned in chapter 7, and Meriwether and his colleagues at Salomon had been heavily involved in trading at that time. LTCM was born into the midst of the bond market turmoil of 1994, when sharp interest-rate increases after a period of relative stability caused large losses to many investors (including the bankruptcy of Orange County, California, which had taken large, unhedged positions in interest-rate derivatives).

So LTCM also "stress tested" its portfolio, investigating the consequences of hypothetical events too extreme to be captured by statistical value-at-risk models—events such as a huge stock-market crash, a bond default by the Italian government, devaluation by China, or (particularly salient in view of LTCM's European involvement) a failure of European economic and monetary union. In addition to investigating the consequences of such events for market prices and for LTCM's risk capital, LTCM calculated—and set aside—the funds necessary to cope with a sudden increase in "haircuts" in a situation of stress. When an event could have particularly catastrophic consequences, LTCM either turned to insurance (it bought what was in effect insurance against bond default by the government of Italy) or balanced its portfolio to minimize consequences (as in the case of failure of European monetary union).

### ***Was LTCM's Trading Arbitrage?***

Clearly, LTCM's trading involved risk. It is therefore, tempting to conclude that what LTCM did—although it was unquestionably “arbitrage” in financial-market usage of the term—was not arbitrage as it is conceived within finance theory. However, LTCM's index option positions were quite close to the arbitrage that finance theory posits as imposing Black-Scholes option pricing. LTCM sold index options and hedged them by constructing a “replicating portfolio,” although the detail of the construction of the latter was more complex than in the textbook case, and the model of stock-price changes that LTCM used was a “proprietary” one, not the log-normal random walk of the Black-Scholes-Merton model (Perold 1999, p. A8).

More generally, beginning with the work of Modigliani and Miller, it was fundamental to finance theory that, in the words of Myron Scholes, “the market will price assets such that the expected rates of return on assets of similar risk are equal.” If the market did not do so, Modigliani, Miller, and their successors reasoned, “investors seeing these profit opportunities would soon arbitrage them away” (Scholes 1972, p. 182). LTCM's “relative value” arbitrage can be seen as precisely this kind of arbitrage.

Of course, just what count as assets of “similar risk” is potentially contentious. The practice of arbitrage can, indeed, be seen as hinging on the identification of similarity that is “good enough” for practical purposes—see the work of Beunza and Stark (for example, Beunza and Stark 2004), which will be discussed in chapter 9—and the issues involved are deep: judgments of similarity are basic to the application of concepts (Barnes, Bloor, and Henry 1996).

Nevertheless, consider LTCM's Royal Dutch/Shell arbitrage, described above. The Royal Dutch/Shell group's net cash flow was split on a fixed 60:40 basis between Royal Dutch and Shell (Froot and Dabora 1999, p. 192). Royal Dutch stocks and Shell stocks were thus claims on two future income streams that were identical (the 60:40 constant of proportionality aside), in that they arose from dividing a single income stream in a set ratio. (In Modigliani and Miller's terms, the two sets of stocks were thus in the same “risk class.”<sup>12</sup>)

It would therefore seem not unreasonable for market participants to regard Royal Dutch stocks and Shell stocks as “assets of similar risk” in respect to cash flows. It is indeed a case in which “in a frictionless world, it is clear that arbitrage would occur [and] drive prices to parity” (Froot and Dabora 1999, p. 215). Such cases are close enough to the “arbitrage” posited by finance theory to be of interest.

### ***The Crisis of 1998***

LTCM's crisis provoked widespread comment—for example, books by Dunbar (2000) and Lowenstein (2000)—and even featured in a novel (Jennings 2002). Typically, popular commentary advanced two accounts:

- (1) The partners in LTCM were guilty of greed and gambling (consciously reckless risk-taking).
- (2) LTCM had blind faith in the accuracy of finance theory's mathematical models.

More informed discussion (for example by the President's Working Group on Financial Markets 1999) avoided blaming individuals' alleged character flaws, and instead advanced a third hypothesis:

- (3) LTCM was over-leveraged—too high a proportion of its positions was financed by borrowing, rather than by LTCM's own capital.

This third hypothesis, however, explains at most LTCM's vulnerability to the events of August and September 1998: it does not explain those events. The most common explanation of them is as follows:

- (4) On August 17, 1998, Russia defaulted on its ruble-denominated bonds and devalued the ruble. This triggered a “flight to quality” in the financial markets—a sudden greatly increased preference for financial assets that were safer (less prone to default) and more liquid (more readily bought and sold).

That there was a flight to quality in August and September 1998, and that the Russian default triggered it, cannot be denied. The hypothesis of this chapter, however, is that superimposed on the flight to quality, and sometimes cutting against it, was a process of a different, more directly sociological kind:

- (5) LTCM's success led to widespread imitation, and the imitation led to a “superportfolio” of partially overlapping arbitrage positions. Sales by some holders of the superportfolio moved prices against others, leading to a cascade of self-reinforcing adverse price movements.

The first explanation—consciously reckless risk-taking—is entirely inadequate as an account for LTCM's 1998 disaster. The partners in LTCM believed themselves to be running the fund conservatively, and in the modest volatility of its returns they had evidence for the correctness of this belief. After the fund's crisis, it was commonly portrayed as wildly risk-taking, but it is hard to

find anyone inside or outside LTCM who can be proved to have expressed that view before the crisis.<sup>13</sup>

Nor does the second hypothesis advanced in the commentary—blind faith in mathematical models—explain the crisis. Models were much less critical to LTCM's trading than commonly thought. Many of the pricing anomalies it sought to exploit (such as the premium of shares in Royal Dutch over those in Shell, or the swap-spread example discussed in appendix G) could be identified without sophisticated modeling. Although models were important to how LTCM's trades were implemented and to assessing the risks involved, all those involved knew that models were approximations to reality and a guide to strategy rather than a determinant of it.

LTCM's traders had often themselves developed the models they used: no one was more aware than they of the models' likely deficiencies. The way in which the standard deviations and correlations in the most important model of all—LTCM's overall risk model—were increased by explicitly judgment-based "safety factors" is indicative of that.

The third posited explanation of LTCM's crisis—over-leverage—is almost tautologically correct. If LTCM had been operating without leverage, or at low levels of leverage, the events of August and September 1998 would have placed it under much less strain. However, leverage was intrinsic to the kind of arbitrage performed by LTCM. As can be seen in the example in appendix G, unleveraged rates of return are typically paltry. Only with leverage does arbitrage of the kind conducted by LTCM become attractive.

LTCM's pre-crisis leverage ratios were not, in fact, egregious when compared, for example, to those of investment banks. In the early months of 1998, LTCM's leverage ratio was around 27:1 (Perold 1999, pp. C11–C12). 27:1 was the *average* ratio of the five biggest investment banks at the end of 1998 (President's Working Group on Financial Markets 1999, p. 29).

Blaming LTCM's crisis on leverage is similar to attributing a plane crash to the fact that the aircraft was no longer safely in contact with the ground: it identifies the source of overall vulnerability but not the specific cause. That cause was the financial crisis of August and September 1998, and in particular the way in which the adverse price movements of those months exceeded LTCM's, or anyone else's, expectations. As noted above, the 1998 crisis involved an increased relative preference for safer, more liquid assets.<sup>14</sup> Since many of LTCM's (and other arbitrageurs') trades involved short-selling such assets while having a "long" position in their less creditworthy or less liquid counterparts, this shift in preferences altered prices in a way that caused losses to LTCM and to other arbitrageurs (albeit losses that in many cases one could be confident would be recouped).

However, the interviews drawn on here suggest that overlaying the increased preference for safer, more liquid assets were the effects of a different, more directly sociological process. Meriwether's group at Salomon and at LTCM earned remarkable profits, and were *known* to have earned those profits. This had encouraged others—in other investment banks, and increasingly in other hedge funds—to follow similar strategies.

Meriwether's group had been imitated even in its days at Salomon Brothers. In the market for mortgage-backed securities, a crucial issue, as noted above, is calculating the impact of homeowners' "pre-payment" option. The calculation was a non-trivial modeling task that typically took the form of adjusting the "spread" of the yield of mortgage-backed bonds over the yield of Treasury bonds of similar maturities to take the pre-payment option into account.

From 1985 to 1987, Richard Roll was head of mortgage securities research for Goldman Sachs, and was well placed to observe behavior in the market for such securities. "The people making more money," he says, "were the ones with the better models, the Meriwethers of the world." Those who were less sophisticated in their modeling learned from the Salomon group by what Roll calls "mimicry": by inferring from Salomon's trading the features its model must have. Roll puts it this way: "If you saw Meriwether going long [that is, buying a mortgage-backed bond] with an option-adjusted spread you thought was five basis points, you knew that *his* model said it's 100 basis points." In consequence, less experienced participants in the market would ask themselves what they would have to do to *their* pre-payment model to generate a larger spread, saying to themselves, in Roll's words, "Let's tinker with [the model] and see if we can get that." (Roll interview)

Imitation seems to have intensified after LTCM's success became public. Other traders were being told "LTCM made \$2 billion last year. Can't you?" (Meriwether interview). For example, LTCM's success meant that it rapidly became largely closed to new investors, and in January 1998 a new fund, Convergence Asset Management, "raised \$700 million in a single month purely from disgruntled investors denied a chance to buy into LTCM" (Dunbar 2000, p. 197).

LTCM tried hard not to reveal its trading positions. For example, it would avoid using the same counterparty for both "legs" of an arbitrage trade. However, as one trader and manager not connected to LTCM put it, "the arbitrage community . . . are quite a bright lot, so if they see a trade happening—and the market gets to find out about these trades, even if you're as secretive as Long-Term Capital Management—they'll analyze them and realize there's an opportunity for themselves" (Wenman interview).



Even if the details of LTCM's trading could not be discovered, its basic strategy—convergence and relative-value arbitrage—had to be disclosed to potential investors, and others seeking to follow that strategy would often be led to take positions similar to LTCM's. It “[didn’t] take a rocket scientist” to discover the kinds of arbitrage opportunities being pursued by LTCM (Rosenfeld interview), especially when discovering one leg of an LTCM trade through being a counterparty to it would greatly narrow the range of likely other legs.

Some of LTCM's trades were well known to market insiders before LTCM became involved. The Royal Dutch/Shell trade, for example, was the “classic European arbitrage trade” (Wenman interview), and the relationship between Royal Dutch and Shell shares had even been discussed in the academic literature before LTCM was founded (Rosenthal and Young 1990). News or speculation about other LTCM trades circulated freely. “I can’t believe how many times I was told to do a trade because the boys at Long-Term deemed it a winner,” says the hedge-fund manager James Cramer (2002, p. 179).

As a result of conscious and unconscious imitation, many of LTCM's positions became, in the words of an arbitrageur who was not affiliated to LTCM, “consensus trades” (Kaplanis interview). Of course, the growing number of arbitrage traders in investment banks and hedge funds did not sit down together in a room to identify good arbitrage opportunities. Rather, “the arbitrage philosophy . . . had been disseminated, well disseminated by August ’98; it was there in quite a few hedge funds, it was there in quite a few firms. So Salomon [and LTCM] lost their uniqueness in doing these things. There were many, many others that could do them.” (Kaplanis interview)

There was *some* communication: “If you talk[ed] to another arb trader in the street, they’d say ‘Oh yes, I have this as well, I have that as well.’” (Kaplanis interview) But even had there not been communication, many traders would still have identified the same opportunities. “And what happened by September ’98 is that there was a bunch of arb trades that . . . became consensus. People knew that the U.K. swap spreads was a good trade, people knew that U.S. swap spreads was a good trade.” (Kaplanis interview) No other market participant would have had the same portfolio as LTCM did—many arbitrageurs were restricted organizationally or by limited expertise to particular portions of the spectrum of arbitrage trades—but, collectively, much of LTCM's portfolio of positions was also being held by others.

The initial effect of imitation was probably to LTCM's benefit. If others are also buying an “underpriced” asset and short-selling an “overpriced” one, the effect may be to cause prices to converge more rapidly. However, as Eric Rosenfeld of LTCM indicated to me in interview, the growing presence of

other arbitrageurs also meant that when existing trades *had* been liquidated profitably, replacing them was more difficult:

*MacKenzie*: Did you find that, as the years went by with LTCM—'94, '95, '96, '97, and so on—did you find . . . that the opportunities were drying up a bit?

*Rosenfeld*: Yes, big.

In the summer of 1998, imitation switched to become a disastrously negative factor because of two decisions, neither of which had anything directly to do with LTCM. In 1997, Salomon Brothers had been taken over by the Travelers Corporation, whose chairman, Sanford I. Weill, was building the world's largest financial conglomerate, Citigroup (Booth 1998). According to Kaplanis, Salomon's U.S. arbitrage desk had not consistently been successful since the departure of Meriwether and his group, and in the first half of 1998 it was loss making: by June, "U.S. was down about 200 [million dollars]. . . . So Sandy [Weill] . . . closed it [Salomon's U.S. arbitrage desk] down" (Kaplanis interview). The closing of the desk was announced on July 7.

Though Kaplanis, promoted to head of global arbitrage for Salomon, advised against it, the decision was taken to liquidate the U.S. arbitrage desk's portfolio as quickly as possible, and responsibility for the liquidation was passed to Salomon's U.S. customer desk. Since the latter was "not accountable for the losses generated as a result of the liquidation, the speed of the latter was faster than would otherwise have been the case." This caused losses not just to Travelers/Citicorp but also to all of those who had similar positions: "Not only did we lose money as the positions went against us as we were selling them, but all the other funds that also had these consensus trades also started losing money." (Kaplanis interview)

If the liquidation of Salomon's arbitrage positions was a background factor in the problems of the summer of 1998, the immediate cause of the 1998 crisis was, as noted above, Russia's August 17 default on its ruble-denominated debt. That Russia was in economic trouble was no surprise: what was shocking was that it (unlike previous debtor governments) should default on debt denominated in domestic currency.

"I was expecting them [the Russian government] to just print money" to meet their ruble obligations, says Kaplanis (interview), and he was not alone in this expectation. True, some investors in ruble-denominated bonds had hedged against the risk of Russia defaulting by short-selling Russian hard-currency bonds (Shleifer 2000, p. 108). For those investors, however, even the good news of August 17—Russia's avoidance of a hard-currency default—was damaging, because it meant their hedge failed to protect them to the extent it should have.

Initially, the Russian default seemed to some to be an event of only modest significance. Robert Strong of the Chase Manhattan Bank told analysts that he did “not view Russia as a major issue” for the banking sector. Investors more generally seemed to share his viewpoint: on August 17, the Dow Jones Industrial Average rose nearly 150 points (Lowenstein 2000, p. 144).

In the days that followed, however, it became increasingly clear that Russia’s default had triggered what Kaplanis calls an “avalanche.” The default was combined with a de facto devaluation of the ruble of 25 percent and a three-month moratorium on the “foreign obligations” of Russian banks (Marshall 2001, p. 4). Since Western investors used foreign-exchange forward contracts with these banks to hedge against the declining value of the ruble, widespread losses were incurred.

LTCM itself had limited exposure to the Russian market, and suffered only modest losses, but Credit Suisse, for example, incurred losses of about \$1.3 billion. Arbitrageurs carrying losses incurred in Russia began liquidating other positions to meet the demands of their counterparties. A hedge fund called High-Risk Opportunities, which had a large position in ruble-denominated bonds, was forced into bankruptcy, owing large sums to Bankers Trust, Credit Suisse, and the investment bank Lehman Brothers. Rumors began to circulate that Lehman itself faced bankruptcy. For weeks, Lehman “went bankrupt every Friday” according to the rumor mill. Though the bank survived, its stock price suffered badly.

In a situation in which the failure of a major investment bank was conceivable, there was indeed a “flight to quality,” an increased preference for safe, liquid assets. In August and September 1998, the prices of such assets rose sharply relative to the prices of their less safe or less liquid counterparts. By September 18, the on-the-run “long bond”—the 30-year maturity U.S. Treasury bond—had risen in price to such an extent that its yield was lower than for three decades (President’s Working Group on Financial Markets 1999, p. 21). As noted above, the consequence of the flight to quality triggered by the Russian default was, therefore, a shift in prices the typical effect of which was to cause losses to convergence and relative-value arbitrageurs.

LTCM had known perfectly well that a flight to quality could happen and that this would be its consequence. Indeed, it was of the very essence of convergence and relative-value arbitrage that spreads could widen—relative prices could move against the arbitrageur—before a trade finally converged. For that reason, LTCM had required investors to leave their capital in the fund for a minimum of three years: it was in part this restriction that made the fund *Long-Term Capital Management*.<sup>15</sup>

If spreads widened, however, it was assumed within LTCM that arbitrage capital would move in to exploit them, and in so doing restrict the widening (Rosenfeld interview). Indeed, once spreads had become wide enough, it was expected that purchases by ordinary investors, attracted by the increased relative returns of unfavored assets, would reduce them.

The configuration of the markets by August 1998, however, was that the widening of spreads was self-feeding rather than self-limiting. As arbitrageurs began to incur losses, they almost all seem to have reacted by seeking to reduce their positions, and in so doing they intensified the price pressure that had caused them to make the reductions.<sup>16</sup> In some cases, senior management simply became “queasy” (Rosenfeld interview) at the losses that were being incurred, and unwilling to incur the risk of further, possibly larger, losses before trades turned profitable. In the United Kingdom, for example, Salomon, LTCM, a large British bank, and others had all taken positions in the expectation of a narrowing of sterling swap spreads. As those spreads widened, the senior management of the British bank decided to exit:

[The bank] of course never had a tradition of risk taking. [It] is a household conservative name. So they were the first . . . to start getting out of positions in [the] U.K. swap spread; that hurt us [Salomon], LTCM as well. And that was a situation probably that was sparked by the fact that they [the bank] never had a tradition . . . in arb trading. . . . There were losses. . . . Some manager didn’t like the idea of [the bank] having these big positions that were showing this big volatility, and they decided to bail out. . . . [The] U.K. swap spread is one of those trades that you know that if you hold the [position] until its maturity you’re probably going to make money. But if there are managers out there that can’t stand the daily volatility . . . then that’s when you’re in trouble. (Kaplanis interview)

In some circumstances, such a decision by management might even be anticipated by the traders: “You know that if . . . your manager sees that you’re down \$10 million . . . the likelihood that he will ask you to get out of this position is very high. It’s not a formal stop-loss but . . . it’s there.” (Kaplanis interview)

In the case of hedge funds, the issue was investor rather than manager queasiness. Most funds did not have LTCM’s long capital lockup: “they knew that investors were starting to drain money if they saw more than 15 percent [loss] or whatever. . . . They knew that if they showed big losses a lot of investors would want to get out. They wouldn’t wait until they lost 80 percent of their money . . . so that was the behavioral constraint that led to people unwinding positions even though they knew that those positions had value in the long run. They just had no choice.” (Kaplanis interview) (The fourth quarter of 1998 saw net withdrawals from hedge funds of about \$6 billion.)<sup>17</sup>

Furthermore, as market prices moved against hedge funds, they had to transfer collateral to their counterparties or to clearinghouses, and that might also require them to raise cash by liquidating positions.

Paradoxically, another factor may have been modern risk-management practices, particularly value-at-risk. This allows senior management to control the risks incurred by trading desks by allocating them a risk limit, while avoiding detailed supervision of their trading. When a desk reaches its value-at-risk limit, it must start to liquidate its positions. Says one trader: “a proportion of the investment bank[s] out there . . . are managed by accountants, not smart people, and the accountants have said ‘Well, you’ve hit your risk limit. Close the position.’” (Wenman interview)

One aspect of the 1998 crisis may have been—Jorion (2002) disputes it<sup>18</sup>—an international change in banking supervision practices that increased the significance of value-at-risk. Banks are required to set aside capital reserves to meet the various risks they face, and in 1996 they began to be allowed to use value-at-risk models to calculate the set-aside required in respect to fluctuations in the market value of their portfolios (Basle Committee on Banking Supervision 1996).

The freedom to use value-at-risk models in calculating capital requirements was attractive to banks because it generally reduced those requirements. However, it could have the consequence that as market prices move against a bank and become more volatile, they may eventually either have to liquidate positions or to raise more capital to preserve them, a slow and often unwelcome process. Even if banks were not close to being forced to make this choice, the increased prominence of value-at-risk may have contributed to pressure to liquidate positions in the face of adverse price movements and of increased volatility (Dunbar 2000; Meriwether interview).

The self-reinforcing adverse price movements of August and September 1998 had major effects on the markets in which LTCM traded. A senior hedge-fund manager not affiliated with LTCM puts it this way: “As people were forced to sell, that drove the prices even further down. Market makers quickly became overwhelmed, where the dealers, who would [normally] be willing to buy or sell those positions were simply unwilling to do it, and they either said ‘Just go away: I’m not answering my phone’ or set their prices at ridiculous levels.” (Shaw interview)<sup>19</sup>

The simple fact that the crisis occurred in August, the financial markets’ main holiday month and thus typically the worst time to try to sell large positions, may have exacerbated the effects of sales on prices. The price movements were certainly huge. In a single day (August 21, 1998), LTCM lost \$550 million as swap spreads in the United States and the United Kingdom widened

dramatically and the planned merger between Ciena Corporation and Tellabs, Inc., in which LTCM had a large position that would profit if the merger was completed, was canceled (Perold 1999, pp. C2–C3).

Consider, for example, the premium of Royal Dutch stock over Shell stock, which, as noted above, LTCM expected to decline. In 1997, the premium had been around 8 percent. During the early months of 1998 it started to rise for unclear reasons, and during the crisis it shot up, at times exceeding 17 percent.<sup>20</sup>

LTCM's losses on any single position, including Royal Dutch/Shell, were tolerable. Crucially, however, correlations between the different components of LTCM's portfolio leapt upward from their typical level of 0.1 or less to around 0.7 (Leahy interview).<sup>21</sup> Suddenly, nearly all the positions held by LTCM began to incur losses, even though they were protected by being hedged against the obvious sources of risk and had little or nothing in common at the level of economic fundamentals. The losses were stunning in their size and their rapidity. In a single month, August 1998, LTCM lost 44 percent of its capital.

Although LTCM's August 1998 loss was huge, and far greater than had seemed plausible on the basis of LTCM's risk model, it was not in itself fatal. LTCM still had "working capital" of around \$4 billion (including a largely unused credit facility of \$900 million), of which only \$2.1 billion was being used for financing positions (Perold 1999, p. C3). LTCM was, it seemed, a long way from being bankrupt, and it owned a portfolio of what were now (because of the widened spreads) *very* attractive arbitrage positions: positions that could reasonably be expected to converge and produce substantial profits.

Again, consider the example of LTCM's matched short position in Royal Dutch and long position in Shell. The sharp rise in the premium of Royal Dutch over Shell stock obviously meant that LTCM's position had incurred losses. However, nothing that had happened disturbed the reasoning underpinning the trade: the premium was still expected eventually to shrink dramatically or vanish. LTCM's Royal Dutch/Shell position was thus worth considerably more at a premium of 17 percent than it had been at 8 percent. If LTCM could hold the position until the now huge premium vanished (as it eventually did in the spring of 2001), it would recoup its temporary losses and indeed profit handsomely.

At this point, however, a social process of a different kind intervened: in effect, a run on the bank. "If I had lived through the Depression," says John Meriwether, "I would have been in a better position to understand events" in September 1998 (Meriwether interview). Investment banks report their results quarterly, but LTCM and other hedge funds report theirs monthly. On September 2, Meriwether faxed LTCM's investors its estimate of the August loss.

Quite reasonably, Meriwether told LTCM's investors that the huge widening in price discrepancies that had occurred in August represented an excellent arbitrage opportunity, and his fax (reproduced in Perold 1999, pp. D1–D3) invited further investment: “. . . the opportunity set in these trades at this time is believed to be among the best that LTCM has ever seen. But, as we have seen, good convergence trades can diverge further. In August, many of them diverged at a speed and to an extent that had not been seen before. LTCM thus believes that it is prudent and opportunistic to increase the level of the Fund's capital to take full advantage of this unusually attractive environment.”

Meriwether's fax, intended to be private to LTCM's investors, became public almost instantly: “Five minutes after we sent out first letter . . . to our handful of shareholders, it was on the Internet.” (Merton interview) In an already febrile atmosphere, news of LTCM's losses fed concern that the fund was on the brink of bankruptcy.

Fears of LTCM's collapse had two effects. First, they had an immediate effect on the prices of assets that LTCM was known or believed to hold. It held, for example, a relatively small amount of “hurricane bonds”—securities that permit insurers to “sell on” the risks of hurricanes. (On the emergence of this fascinating market, see Froot 1999.) On September 2, the price of hurricane bonds fell 20 percent, even although there had been no increase either in the probability of hurricanes or in the likely seriousness of their consequences.<sup>22</sup>

Assets that LTCM was believed to hold in large quantity became impossible to sell at anything other than “fire sale” prices. Beliefs about LTCM's portfolio were sometimes incorrect or exaggerated: after the crisis, LTCM was approached with an offer to buy six times the position it actually held in Danish mortgage-backed securities (Meriwether interview). Nevertheless, presumptions about its positions were accurate enough to worsen its situation considerably, and as September went on, and LTCM had to divulge more information to its counterparties, those presumptions became more accurate.

The second effect on LTCM of fears of its collapse was even more direct. Its relationship to its counterparties (those who took the other side of its trades) typically was governed by “two-way mark-to-market”: as market prices moved in favor of LTCM or its counterparty, solid collateral, such as government bonds, flowed from one to the other.

In normal times, in which market prices were reasonably unequivocal, two-way mark-to-market was an eminently sensible way of controlling risk by ensuring that the consequences of a counterparty defaulting were limited. In September 1998, however, the markets within which LTCM operated had

become illiquid. There was “terror” that LTCM was going to liquidate, says Meriwether (interview).

The loss caused to a counterparty if LTCM became bankrupt could be mitigated by it getting as much collateral as possible from LTCM before that happened, and this could be achieved by “marking against” LTCM: choosing, out of the wide spectrum of plausible prices in an illiquid market, a price unfavorable to LTCM, indeed predicated on the latter’s failure (Merton interview; Meriwether interview). LTCM had the contractual right to dispute unfavorable marks. In its index-options contracts, for example, such a dispute would have been arbitrated by getting price quotations from three dealers not directly involved. These dealers, however, would also be anticipating LTCM’s failure, so disputing marks would not have helped greatly.

The outflows of capital resulting from unfavorable marks were particularly damaging in LTCM’s index-option positions, where they cost the fund around \$1 billion, nearly half of the September losses that pushed it to the brink of bankruptcy (Rosenfeld interview). In the 1998 crisis, stock-market volatility did indeed increase. But to this increase was added the results of anticipation of LTCM’s likely demise.

As the prices of the options that LTCM had sold rose (in other words, as their implied volatilities increased), LTCM had to transfer collateral into accounts held by its counterparty banks. If LTCM failed, those banks would lose the hedge LTCM had provided them with (in other words, they would be “short volatility”) but they would now own the collateral in the accounts. So it was in their interest that the implied volatility of the index options LTCM had sold should be as high as possible.

One banker whose bank had bought index options from LTCM says:

“When it became apparent they [LTCM] were having difficulties, we thought that if they are going to default, we’re going to be short a hell of a lot of volatility. So we’d rather be short at 40 [at an implied volatility of 40 percent per annum] than 30, right? So it was clearly in our interest to mark at as high a volatility as possible. That’s why everybody pushed the volatility against them, which contributed to their demise in the end.” (quoted by Dunbar 2000, p. 213)

Indeed, in some cases market participants with no direct involvement with LTCM seem to have profited from its difficulties. For example, LTCM’s trading often involved short positions in Treasury bond futures on the Chicago Board of Trade. To reduce those positions it would have to buy bond futures via the bank that acted as its “prime broker,” Bear Stearns.

A remarkable analysis by Cai (2003) of Board of Trade data obtained from the Commodity Futures Trading Commission via the Freedom of



Information Act shows that market makers seem (perfectly legitimately) to have anticipated such purchases, buying futures a minute or two before Bear Stearns did, alerted perhaps by the arrival in the pit of brokers for Bear Stearns or by the behavior of traders acting on the firm's behalf.<sup>23</sup>

LTCM kept its counterparties and the Federal Reserve informed of the continuing deterioration of its financial position. On September 20, 1998, U.S. Assistant Secretary of the Treasury Gary Gensler and officials from the Federal Reserve Bank of New York met with LTCM. By then, it was clear that without outside intervention bankruptcy was inevitable.

In the words of William J. McDonough, president of the Federal Reserve Bank of New York: "Had Long-Term Capital been suddenly put into default, its counterparties would have immediately 'closed out' their positions. . . . If many firms had rushed to close out hundreds of billions of dollars in transactions simultaneously . . . there was a likelihood that a number of credit and interest rate markets would experience extreme price moves and possibly cease to function for a period of one or more days and maybe longer." (McDonough 1998, pp. 1051–1052) If "the failure of LTCM triggered the seizing up of markets," said Alan Greenspan, it "could have potentially impaired the economies of many nations, including our own" (Greenspan 1998, p. 1046).

McDonough brokered a meeting of LTCM's largest counterparties, which concluded that a re-capitalization of LTCM would be less damaging to them than a "fire sale" of its assets. Fourteen banks contributed a total of \$3.6 billion, in return becoming owners of 90 percent of the fund. LTCM's investors and partners were not "bailed out." They were left with only \$400 million, a mere tenth of what their holdings had recently been worth.

The re-capitalization did not immediately end the crisis. Many in the markets feared that the consortium that now owned LTCM might still decide on an abrupt liquidation. On October 15, 1998, however, the Federal Reserve cut interest rates without waiting for its regular scheduled meeting, and the emergency cut began to restore confidence. It also gradually became clear that the consortium was intent on an orderly, not a sudden, liquidation of LTCM's portfolio, which was achieved by December 1999.

### ***The Flight to Quality and the Superportfolio***

If the "superportfolio" explanation advanced in this chapter is correct, then superimposed on the flight to quality should be distinctive price movements reflecting the unraveling of the positions held by LTCM's conscious and unconscious imitators. The composition of the superportfolio is not known with any precision, but if the imitation-based explanation is correct, LTCM's

portfolio should be a reasonable proxy, and its main components are known from Perold 1999 and from the testimony of interviewees. The hypothesized specific characteristic of September 1998—"run-on-the-bank" declines in the prices of assets believed to be held by LTCM—is identical in its predicted consequences to the "unraveling superportfolio" explanation.

Convergence and relative value arbitrage as conducted by LTCM and its imitators typically involves short-selling an asset with low default risk and/or high liquidity while holding a similar asset with higher default risk and/or lower liquidity. In many cases, therefore, price movements caused by a flight to quality and by the forced sales of components of an arbitrage superportfolio cannot be distinguished.

In cases of two types, however, the predictions of the two explanations differ. Type one is cases in which there is a range of similar spreads or implied volatilities in only some of which LTCM had positions. The superportfolio explanation would then predict greater increases in the spreads or implied volatilities in which LTCM had positions than in those in which it did not (assuming that, as was in general the case, LTCM held the less liquid instrument or was short volatility, in other words had sold the options in question). If the spreads or implied volatilities genuinely are similar, the flight-to-quality explanation would, in contrast, predict similar movements of them all.

The second type of case in which the predictions of the flight-to-quality and superportfolio explanations differ is the minority of arbitrage positions in which LTCM held the more liquid instrument and was short the less liquid one (the swap-spread example discussed in appendix G is an example of this kind of situation). In such a situation, the flight-to-quality interpretation predicts a rising spread; the superportfolio explanation predicts a more slowly rising, or possibly even a falling, spread.

Several of the major positions held by LTCM in the summer of 1998 fall into either type one or type two. Consider, for example, the two sets of positions that, together, were responsible for around two-thirds of LTCM's losses: equity index options and swap spreads (Lewis 1999). Equity index options are a "type one" case. LTCM had sold large amounts of long-dated index options on all the major stock-market indices listed in table 8.1, except the Japanese Nikkei 225.<sup>24</sup> The implied volatilities of all rose, but the increase in Nikkei 225 implied volatilities was much smaller than in the case of the other indices. Since there was, as far as I am aware, no clear flight-to-quality reason for increased relative confidence in the future stability of the Japanese stock market, this is evidence for the superportfolio hypothesis.

Swap spreads encompass two "type two" cases (France and Germany) and also an overall "type one" comparison. Because the market in swaps is less

**Table 8.1**

Average implied volatilities (annualized) of five-year options on major stock-market indices. Source: JWM Partners.

	June–July 1998	September 1998	Increase (percentage points)
S&P 500 (U.S.)	23%	30.3%	7.3
FTSE 100 (U.K.)	22.9%	32.4%	9.5
CAC (France)	25.8%	32.9%	7.1
SMI (Switzerland)	26.1%	35.5%	9.4
DAX (Germany)	26.5%	35.5%	9
NK225 (Japan)	25.6%	30.3%	4.7

**Table 8.2**

Average swap spreads (basis points) against selected government bonds, June–September 1998. Source: JWM Partners.

	June–July 1998	September 1998	Increase
France <sup>a</sup>	17	23	6
U.S. <sup>b</sup>	41	64	23
U.K. <sup>c</sup>	52	92	40
Japan <sup>d</sup>	34	41	7

a. 6% coupon, maturing October 2025

b. 6.625% coupon, maturing May 2007

c. 8% coupon, maturing 2021

d. 2.2% coupon, maturing December 2007

liquid than that in government bonds, and because a crisis may prompt fears of bank failures (and did so in 1998), a flight to quality should increase swap spreads. Table 8.2 contrasts the behavior of swap spreads in four countries.<sup>25</sup> In France, LTCM was “long” the swap spread in 1998 (that is, had a position, akin to that described in the swap spread example in appendix G, which would increase in value if the spread rose). That makes France a “type two” case, one in which the effect of the superportfolio unraveling (downward pressure on the swap spread) would be opposite in direction to the effect of a flight to quality (upward pressure).

The United States and the United Kingdom (in both of which LTCM was short the swap spread in the summer of 1998) are cases in which both the flight-

to-quality and superportfolio explanations predict a rise in the swap spread. Japan is a case that adds to the overall “type one” comparison of changes in the swap spread: in Japan, LTCM had two offsetting positions that left it neutrally placed with respect to overall widening or narrowing of the spread.

As table 8.2 shows, spreads widened markedly in the United States and the United Kingdom. (The same happened in Sweden, where arbitrageurs were also short the swap spread.) In contrast, in France and Japan, swap spreads widened only more modestly during the crisis; that was also the case in Germany, another type two case in which LTCM had a long position in the swap spread akin to that in France.<sup>26</sup> I know of no plausible flight-to-quality explanation of these international contrasts, while they are broadly consistent with the superportfolio explanation.

Equity volatility, U.S. swap spreads, and European differential swap spreads are three of the thirteen major positions held by LTCM in the summer of 1998 (table 8.3). A further two of its positions also fall into type one or type two, as another six do to some extent. The overall pattern in table 8.3 seems clear. In all the cases for which data are available, the relative price movements of the crisis are consistent with the “superportfolio” explanation, while in five cases they are inconsistent (and in a further four, possibly inconsistent) with the flight-to-quality explanation. A flight to quality *did* take place in August and September 1998, but these data do indeed suggest that overlaying it (and sometimes acting in contradiction to it) was an unraveling superportfolio.

A simpler piece of evidence consistent with the superportfolio hypothesis is the contrast between the market reaction to the August 1998 Russian default and to the attacks of September 11, 2001, which also sparked a flight to quality. LTCM’s successor fund, JWM Partners, was active then too, but its capital base was smaller and its leverage levels lower, so its arbitrage positions were considerably smaller (Silverman and Chaffin 2000). The amount of capital devoted to convergence and relative value arbitrage by other market participants such as investment banks was also much smaller (interviewees estimate possibly only a tenth as large in total).

There was thus no significant superportfolio in 2001. With a flight to quality, but no superportfolio, there was no equivalent crisis. While LTCM had been devastated in 1998, JWM Partners’ broadly similar, but much smaller, portfolio emerged unscathed from September 2001: the partnership’s returns in that month were “basically flat.” Nor is that outcome specific to JWM Partners: the fall of 2001 saw no big hedge-fund failures, few major losses, and no significant change in the level of the main index of overall hedge-fund performance. Investors overall added to their hedge-fund holdings, rather than withdrawing capital as in 1998.<sup>27</sup>

**Table 8.3**

LTCM's thirteen major positions in August 1998 (as listed in Perold 1999, pp. C6–C7), classified by relationship to flight-to-quality and superportfolio explanations using price data from JWM Partners. Type 1: comparison of similar spreads or implied volatilities. Type 2: LTCM long the more liquid or more creditworthy instrument. Neutral: predictions of flight-to-quality and superportfolio identical. Libor: London interbank offered rate. BOT: Buoni Ordinari del Tesoro.

	Type of case	Relation of Aug–Sept. '98 price movements to superportfolio (s) and flight-to-quality (q) explanations
Equity volatility	Type 1	Consistent with s, inconsistent with q (see text)
U.S. swap spreads	Type 1 comparison of U.S. and U.K. with	Type 1 and type 2 aspects both consistent with
European differential swap spreads	Japan; type 2 in France and Germany	s, inconsistent with q (see text)
Commercial mortgages	Type 1	Consistent with s, inconsistent with q <sup>a</sup>
Deutschmark/euro swap options	Types 1 and 2	Consistent with s, inconsistent with q <sup>b</sup>
BOTLibor vs. Libor	Element of type 2	Consistent with s; possibly inconsistent with q <sup>c</sup>
Yen differential swap spread	Possible type 2	Consistent with s; possibly inconsistent with q <sup>d</sup>
Residential mortgages	Neutral	Data not available
Sterling differential swap spread	Possible type 1	Data not available
Merger arbitrage	Possible type 1	Consistent with s; possibly inconsistent with q <sup>e</sup>
Corporate capital structure	Unclear	Data not available
European equity pairs	Partial type 1	Consistent with s; possibly inconsistent with q <sup>f</sup>
Japanese bank preference shares	Possible type 2	Data not available

- a. AAA commercial mortgage-backed bonds\* widened vs. Libor by 23 basis points; AA (greater default risk) corporate bonds widened 3 basis points; AAA (similar default risk) Federal National Mortgage Association debentures (e.g. 5.75% coupon maturing February 15, 2008) narrowed versus Libor swaps by 3 basis points. (Here and in other notes, \* indicates an asset in which LTCM had a long position.)
- b. Deutschmark/euro swap option\* implied volatility fell (should rise in flight to quality); dollar swap option volatility unchanged.
- c. Italian government bonds generally seen as somewhat riskier than lira Libor swaps, so BOTLibor (the yield at auction of BOTs) should rise relative to lira Libor in crisis, but fell.
- d. LTCM long yen swap spread at 6-year maturity vs. short swap spread at 9-year maturity. In flight to quality, some expectation that shorter-maturity swap spreads will widen more; in fact, 9-year spread widened more.
- e. Largest-ever drop in "Merger Fund" (risk arbitrage fund) price; interviewees suggest drop 3 times level accountable for by merger breaks. However, perceived risk of latter does rise during market falls.
- f. Royal Dutch premium over Shell\* rose. Relationship to flight to quality explanation affected by extent to which premium reflected greater Royal Dutch liquidity, which is unclear.

### ***A Global Microstructure***

One way of expressing the forms currently taken by the inextricable interweaving of the “economic” and the “social” is Knorr Cetina and Bruegger’s notion of “global microstructure.” The financial markets are now global in their reach, but interaction within them still takes the form of “patterns of relatedness and coordination that are . . . microsocial in character and that assemble and link global domains” (Knorr Cetina and Bruegger 2002a, p. 907).

In a sense, it was globalization that undid LTCM. “Maybe the error of Long-Term was . . . that of not realizing that the world is becoming more and more global over time,” says Myron Scholes (interview). Of course, no one was more aware than LTCM’s principals of globalization *as a general process* (they had surfed globalization’s wave, so to speak), but they were caught unawares by the consequences of the global microstructure created by imitative arbitrage.

What happened in August and September 1998 was not simply that international markets fell in concert (that would have had little effect on LTCM), but that very particular phenomena, which at the level of economic “fundamentals” had seemed quite unrelated, suddenly started to move in close to lock-step: swap spreads, the precise shape of yield curves, the behavior of equity pairs such as Royal Dutch/Shell, and so on.

The “nature of the world had changed,” says John Meriwether, “and we hadn’t recognized it” (Meriwether interview). LTCM’s wide diversification, both internationally and across asset classes, which he had thought kept aggregate risk at acceptably modest levels, failed to do so, because of the effects of a global microstructure rooted in one of the most basic of social processes: imitation.

Often technology develops in a cascade. An initial success provides a basis for further improvements and new developments, and those improvements and developments are in their turn built on. Soon, the technology has outstripped potential rivals and seems to have a momentum of its own.<sup>1</sup>

The emergence of finance theory also has something of the character of a cascade. Markowitz provided Sharpe with his basic model; Modigliani and Miller inspired Treynor; Sharpe and Treynor gave Black and Scholes their crucial intellectual resource; Merton rebuilt Black and Scholes's model on new foundations; Cox, Ross, Rubinstein, Harrison, and Kreps built on Black, Scholes, and Merton; Wall Street's "quants" translated what the academics had done into forms more suited to the exigencies of practice; and so onward into trading rooms.

At times, steps in the cascade involved one researcher's reading the works of previous researchers, as when Treynor read Modigliani and Miller. At other times, especially in the early stages of the cascade, extensive personal contact was involved, as in the cases of Sharpe's relation to Markowitz, Black's relation to Treynor, Merton's relation to Black and Scholes, and Emanuel Derman's relation to Black.<sup>2</sup>

By the 1960s and the early 1970s, the first generation of financial economists had Ph.D. students who were beginning to make their own mark on the field. There was an increasingly rich literature and a network of interconnected scholars. Influential centers of work had emerged, notably at the University of Chicago and the Massachusetts Institute of Technology. By the late 1970s and the 1980s, the new specialty had spread across the business schools of the main research universities of the United States and was beginning to have a significant influence on market practice.



### ***Making Models***

As with cascades in technology, cascades in the social sciences are not self-explaining. In Britain, the economist A. D. Roy produced a mathematical analysis of portfolio selection (Roy 1952) that was contemporaneous with and similar to Markowitz's, while the statistician M. G. Kendall (1953) put forward the view of price changes as random walks. Unlike Markowitz's work, however, Roy's was not carried on by the equivalent of a Sharpe. Kendall's work *was* built upon, but in the United States, by Samuelson, not in the United Kingdom.

The initial developments in the United Kingdom thus did not build into a cascade similar to that in the United States. Chance no doubt played a role, but Whitley (1986a,b) is surely right to see the academicization of American business schools as having facilitated the development of modern financial economics in the United States. As he points out, the slow development in the U.K. of business schools comparable to those in the United States, and the fact that in the U.K. finance was often taught in departments of accounting and finance that were dominated by accountants, created an institutional context that was much less supportive of theoretical, mathematical approaches to the subject.

Financial economics was never simply an academic endeavor, and putting finance theory to practical use was seen as entirely legitimate. The interweaving of innovation in financial economics and developments in the financial markets meant that, despite the hostility of many in the markets to finance theory, attractive consultancies were available from the 1960s on. Before long, there were tempting job opportunities in the finance sector. In 1984, for example, Fischer Black resigned his professorship at MIT's Sloan School of Management for a post at Goldman Sachs, where he remained until his death in 1995. In 1990, Scholes became a consultant to Salomon Brothers, and later he became joint head of the group at Salomon that traded and sold bond derivatives, though he also maintained an academic attachment to Stanford University.

However, despite the many links between financial economics and market practice, the changed priorities of American business schools had the consequence that central aspects of the field's reward system and epistemic culture remained distinctively academic and research-focused. While teaching contributions within business schools were no doubt of some importance, recognition by academic peers for research achievements was indispensable for financial economists who remained within university settings.<sup>3</sup>

For a finance theorist, the key to achieving peer recognition was the development of good models. “Good” meant economically plausible, innovative, and analytically tractable. A model that neither its developer nor anyone else could solve was unlikely to be seen as good. The most influential models, notably the Capital Asset Pricing Model and the Black-Scholes-Merton model, yielded as their solutions relatively simple equations. However, a good model also could not be “obvious” and thus at risk of being seen by theorists’ peers as trivial (as Sharpe had feared would happen to the first equilibrium model he produced in his work on capital asset pricing).<sup>4</sup>

The finished products of theorists’ efforts could sometimes seem straightforward to those whose mathematical training was in the physical sciences and who saw in the Black-Scholes equation merely a version of the familiar diffusion equation. The apparent simplicity, however, was deceptive. To develop a “good” finance-theory model required extensive, imaginative bricolage or tinkering. (See MacKenzie 2003c.) It also required deployment of the theorist’s crucial skill: the capacity to find “good abstractions” (Merton interview).

To those finance theorists who remained in universities, the practical usefulness of models was in general a secondary consideration. (For example, the work of Modigliani and Miller, described in chapter 2, was highly prized, even though the extent to which it can be said to have informed market practice is unclear.) What mattered most to career success in academia was that other financial economists were enthusiastic about a theorist’s models. From the 1960s on, publication in prestigious journals became increasingly essential too.<sup>5</sup>

The most striking example of publication’s significance in reward systems was the case of Jack Treynor, who can reasonably be considered a co-developer, along with William Sharpe, of the Capital Asset Pricing Model. Sharpe was honored with a Nobel Prize; Treynor was not. Based as he was primarily outside of academia, publication did not have for Treynor the priority it had for academics, and he had not published his work on the model. (The other two co-developers of the Capital Asset Pricing Model, John Lintner and Jan Mossin, had published, but both were dead by the time of the awarding of the Nobel Prize in 1990.)

It should not be thought that the priority that academics had to give to publication was at odds with their work’s having practical impact. The fact that Kassouf, Black, Scholes, and Merton had published their work meant that their option-pricing models were available for adoption by practitioners in a way in which their proprietary rivals were not. As chapter 6 suggested, it is possible that this simple fact is significant in explaining the adoption of the Black-Scholes-Merton model by option traders.

When research productivity, peer recognition, and publication are prized, another essential quality of a “good” model is that it can be the foundation for further research that is itself tractable without being trivial. All the “orthodox” models centrally considered here had that feature. Consider, in contrast, Mandelbrot’s infinite-variance Lévy distributions, another case of a cascade that began (with Fama building on Mandelbrot’s work, and Fama’s students, especially Richard Roll, then taking up the Lévy model in their turn) but then lost almost all momentum. Infinite variances undercut standard components of the economist’s and econometrician’s intellectual toolkit, while what Lévy distributions had to offer in their stead could seem dauntingly intractable.

Over and above questions of the analytical tractability of models lay a central theoretical commitment. Orthodox financial economics was, and is, committed strongly to a view of financial markets as efficient. That commitment has now been challenged forcibly by the rise of “behavioral finance,” which emphasizes investors’ propensity to systematic errors in reasoning. Behavioral finance is, however, a relatively recent development. The work by the psychologists Daniel Kahneman and Amos Tversky that inspired behavioral finance dated from the late 1970s, but not until the 1990s did behavioral finance become prominent. In the 1960s and the 1970s, the commitment to market efficiency faced no serious challenge within financial economics, for all that it aroused practitioners’ ire.

The role of theoretical commitment to market efficiency can be seen in the contrast between the work of Edward O. Thorp and that of Black and Scholes. Thorp was far better trained mathematically than they were, and he had extensive experience of trading options (especially warrants), when they had next to none. He and Kassouf also conceived of a hedged portfolio of stock and options, and they, unlike Black and Scholes, had implemented approximations to such hedged portfolios in their investment practice.

However, while Black and Scholes were trying to solve the problem of option pricing by deploying the Capital Asset Pricing Model, Thorp had little interest in the latter; he was aware of it, but not “at the expert level.”<sup>6</sup> Indeed, for him the proposition (central to the mathematics of Black and Scholes, and in a different way to Merton’s analysis as well) that a properly hedged portfolio would earn only the riskless rate of interest would have stood in direct contradiction to his empirical experience. The entire point of his and Kassouf’s book *Beat the Market* (Thorp and Kassouf 1967) was that its authors had found they could earn far more than that from hedged portfolios.

For Thorp, then, to have put forward Black and Scholes’s or Merton’s central argument would have involved overriding what he knew of empirical reality. For Scholes, trained as he was in Chicago economics, and even for Black

(despite his doubts as to the precise extent to which markets were efficient), it was reasonable to postulate that markets would not allow money-making opportunities such as a riskless portfolio that earned more than the riskless rate of interest.

Thorp, in contrast, was equally convinced that such opportunities could be found in the capital markets. The “conventional wisdom” had been “you can’t beat the casino”—in the terminology of economics, “the casino markets are efficient.” With his card-counting methods, Thorp had showed that this was not true—“So why should I believe these people who are saying the financial markets are efficient?” (Thorp interview)

Ultimately, though, what divided Thorp from Black, Scholes, and Merton was not empirical. Black’s, Scholes’s, and Merton’s practical actions are evidence that they too accepted that mispricings that offered opportunities for profit did sometimes exist. While looking for ways of testing their model empirically, Black and Scholes identified apparently unduly cheap warrants issued by a corporation called National General. “Scholes, Merton, and I and others jumped right in and bought a bunch of these warrants” (Black, 1989, p. 7), just as Thorp might have done.

What divided Thorp from Black, Scholes, and Merton was a difference in goals. Thorp was not trying to pursue a career in academic financial economics. His aim was to find arbitrage opportunities, and his modeling work was mainly a means to that end. In contrast, Merton and (at least at that stage of their careers) Black and Scholes were oriented above all to their academic peers: seeking to make money from National General warrants was a sideline. Their model was designed to offer an elegant, economically insightful account of the processes determining option prices. They did not see modeling, as Thorp did, primarily as a tool for identifying opportunities for profitable trading.

### ***The Ambivalence of Finance Theory***

The purchase by Black, Scholes, and Merton of the National General warrants indicates that the theoretical commitments of the financial economists discussed here, strong as they were, did not always take the form of conviction that the field’s models were empirically entirely accurate. *Some* degree of conviction of verisimilitude is perhaps inevitable when a model becomes incorporated centrally into the work of a successful research specialty and researchers have a “shared sense of the group as having achieved a satisfactory, working routine” (Bloor 2004, pp. 24–25).

In the “euphoric period” of the early 1970s, the efficient-market hypothesis and the Capital Asset Pricing Model had successfully been incorporated

into a “working routine.” Their purchase on reality was indeed seen as strong: they “seemed a sufficient description of the behavior of security returns” (Fama 1991, p. 1590).

However, the confidence of the early 1970s soon receded. The efficient-market hypothesis remained the central pivot of financial economics. Even it, however, was not always treated in practice, even by its strongest adherents, as an exact representation of reality.

Probably a majority of the finance theorists discussed in this book have had some involvement in practical activity that would make no sense if the efficient-market hypothesis were taken to be an entirely accurate model of markets. This activity ranged from simple, small-scale private investment like the purchase of National General warrants, to the more demanding exploitation of anomalies such as the “turn-of-the-year” effect, and, on the largest scale, the massive arbitrage trading by Long-Term Capital Management.

Finance theorists believed that markets were made efficient by the actions of arbitrageurs and other knowledgeable investors, so there was no contradiction in those theorists seeking to take these actions themselves. Nevertheless, that they did so shows that they did not construe market efficiency as an already-achieved fact. Rather, the achievement of efficiency was a process—perhaps an endless process<sup>7</sup>—in which they could themselves sometimes take part and from which they could profit.

Similar ambivalence—the capacity both to be committed to a model and, simultaneously, to doubt the extent of its empirical validity—can be found in regard to more specific models. Black and Scholes, for example, drew crucially on the Capital Asset Pricing Model. However, they also knew (indeed, they had themselves showed in their collaboration with Jensen) that the empirical accuracy of the model, at least in its original form, was questionable, and they were involved in the proposal discussed in chapter 4 for an investment fund that would exploit this.

Black delighted in pointing out “the holes in Black-Scholes” (Black 1988a): economically consequential ways in which the option-pricing model’s assumptions were unrealistic. He was among the first to recognize that the 1987 crash had given rise to a volatility skew at odds with the model (Black 1990, p. 1).

The denouement of the National General story was, likewise, intended by Black in part as a warning to his readers. Unknown to him and his colleagues, but apparently privately known to many in the markets, another corporation, American Financial, was about to issue an offer for the stock of National General, and the offer’s terms “had the effect of sharply reducing the value of the warrants. . . . The market knew something that our formula didn’t know.” (Black 1989, p. 7)

Another facet of ambivalence is, however, present here too. The failure to profit from National General warrants might have been a private disappointment, but there was a sense in which it was a vindication of market efficiency: an apparent profit opportunity turned out not to be real, because market prices were incorporating a piece of information that was not known to Black and his colleagues. “Although our trading didn’t turn out very well, this event helped validate the [Black-Scholes] formula,” wrote Black: “The market price was out of line for a very good reason” (Black 1989, p. 7). Paradoxically, theorists’ practical failure could thus be cited as evidence of theory’s robustness.

When I discussed the crisis of Long-Term Capital Management (LTCM) with interviewees, the same aspect of ambivalence was sometimes manifest. Because of the prominent role of finance theorists in LTCM, its crisis was embarrassing to the field, but the debacle could also be read as a vindication of market efficiency. The large, apparently safe arbitrage profits that LTCM had been making contradicted the efficient-market hypothesis, and thus there was a sense in which LTCM’s crisis resolved the contradiction by showing that those profits had not been as safe as they seemed.

Fischer Black was unquestionably unusual in the extent and in the public nature of his sensitivity to the empirical weaknesses of finance theory’s models, a sensitivity that ran alongside his development of those models and his strong commitment to them (especially to the Capital Asset Pricing Model). However, all of the finance theorists centrally discussed in this book would both strongly defend the virtues of their models *and* agree with Fama’s assertion, quoted in chapter 4, that any model is “surely false” in the sense of not being an exact description of market realities. Finance theory is an epistemic culture that makes knowledge using models, but there is a deep ambivalence in the field’s attitude to the extent to which its models can be taken as realistic.

### ***Financial Economics and the Infrastructures of Markets***

As I have emphasized, although financial economics was to a significant extent a distinctively academic enterprise, the boundary that separated it from practical activity was very porous. Through it flowed money, people, procedures, devices, and ideas. Despite the fact that its claims were controversial, financial economics received funding from the burgeoning finance industry in the United States, such as the stream of support that made possible the crucial stock-price databases produced by the University of Chicago’s Center for Research in Security Prices.

Financial economics inherited from market practices not just money and data, but also concepts and tools: discounting and at least the outline of the

dividend discount model; “payoff diagrams” for options (Preda 2001a, p. 226); the put-call parity relationship; the arbitrage-imposed relationship between futures prices and “spot” (immediate delivery) prices; and so on. Practitioners had an important intellectual influence on financial economics; the most obvious example here is Jack Treynor, but another example is Fischer Black, who became a financial economist while working (initially with Treynor) at the consulting firm Arthur D. Little.

This book’s focus, however, has been on the other direction of flow: from finance theory to the markets. How important has that been? Does it amount, as chapter 1 suggested, to the “performativity” of economics, and if so performativity of what sort?

Finance theory has become incorporated into the infrastructures of financial markets—first in the United States, and increasingly worldwide as well—in at least three ways: technical, linguistic, and legitimatory. All three are most evident in the case of financial derivatives, the emergence and development of which have been perhaps the most dramatic change in global finance since the start of the 1970s. The huge derivatives-market trading volume mentioned in chapter 1—a total of \$273 trillion in notional amounts of outstanding contracts in June 2004—would effectively be inconceivable without a guide to the pricing and hedging of such derivatives, and that is surely a case of theory’s technical incorporation into market infrastructure.

The software that is used to price derivatives and to measure the exposure to risk that they involve has developed far beyond the analysis of simple “vanilla” options (as they would now be called) using the Black-Scholes-Merton model. However, many of today’s models are still based on the same underlying model-building strategy of identifying a “replicating portfolio” of more basic assets (see chapter 1) and using its costs to price a derivative.

Derivatives-pricing models implemented in software give large players in the derivatives market, notably investment banks, the ability mathematically to analyze and to decompose the risks involved in their portfolios, and this is vital to their capacity to operate on a large scale in this market. That capacity, in its turn, makes the major investment banks attractive counterparties for customers seeking derivatives contracts, and this is an important component of the central role played in the financial markets by the leading global investment banks such as Goldman Sachs and Morgan Stanley.

A second way in which finance theory has been incorporated into the infrastructures of financial markets is linguistic. The theory offers a way of talking about markets, especially about markets whose complexity might otherwise be baffling. Much of the attractiveness to practitioners of the Capital Asset

Pricing Model's "beta" was that it seemed to offer a simple way of summarizing the relative riskiness of different stocks.

Investment analysts might sometimes not have understood "beta" as Sharpe had intended. They often took beta to be a direct measure of a stock's individual riskiness rather than a measure of a stock's covariance with the overall market, and thus of the stock's contribution to the overall risk of a diversified portfolio.<sup>8</sup> Nevertheless, "beta" became a lasting feature of how sophisticated practitioners talked, and still talk, about financial markets.

Again, though, derivatives offer the best example of the incorporation of finance theory into the linguistic infrastructures of markets. Options market participants talk all the time about "implied volatility": indeed, option prices are sometimes quoted not in dollars but in implied volatilities. "Implied volatility" is an entirely theoretical notion, calculated by running an option-pricing model such as Black-Scholes "backwards" to determine the volatility of the underlying asset implied by option prices.

As was emphasized in chapter 6, the notion of "implied volatility" takes a multiplicity of market prices (the prices of puts and calls with different expiry dates and different strike prices, and the prices of more complicated options as well) and reduces them to a single number or small set of numbers. Options market participants may no longer perform Black-Scholes pricing, but their constant invocation of "implied volatility"—a notion that did not exist before the work of Black, Scholes, and Merton—shows that work's permanent imprint on their culture.

A third form of the incorporation of finance theory is legitimacy. To say of a financial market that it is "efficient"—that its prices incorporate, nearly instantaneously, all available price-relevant information—is to say something commendatory about it, and that has been what orthodox financial economics has said about the central capital markets of the advanced industrial world.

"Efficient," in the sense in which the term is used in financial economics, does not equate to "desirable": one could, for example, imagine a market in human kidneys that was efficient in the speed with which prices incorporate available information, but that does not imply that it would be good to construct such a market. Nevertheless, that our wider culture's most authoritative source of knowledge of markets has declared financial markets to be "efficient"—a word with almost entirely positive connotations in ordinary speech—has surely been of some importance.

The legitimacy role of economics in regard to the development of derivatives markets has been particularly clear-cut. As chapter 1 noted, as recently as 1970 markets in financial derivatives were either non-existent or (by today's standards) small-scale, and many current financial derivatives were illegal.



Derivatives were haunted by the impression, held not only by lay people but by many market regulators, that they were simply wagers on the movement of prices. This was not merely a general obstacle to the development of derivatives markets. The way in which the legal distinction between gambling and legitimate futures trading had been drawn in the United States made cash-settled derivatives contracts illegal, and, as noted in chapter 6, until the 1980s this blocked the emergence of stock-index derivatives such as index futures and index options.

Economists (Baumol, Malkiel, and Quandt in the case of the Chicago Board Options Exchange; Friedman in the case of the Chicago Mercantile Exchange) helped make the Chicago financial derivatives markets possible by providing initial legitimacy. Black, Scholes, and Merton provided the capstone, decisively undermining the regulatory view that derivatives were morally dubious instruments of gambling. The modern derivatives markets might have developed even in the absence of this legitimacy role of economics—futures markets and at least sporadic markets in options had emerged in previous centuries without much in the way of academic legitimization—but there can be little doubt that economics eased their path.

### ***Barnesian Performativity***

There are clearly cases—probably many cases—in which finance theory was invoked but in which the invocation had little or no effect on market processes. In the 1970s, for example, many U.S. investment managers learned to talk plausibly about “betas” and paid for beta books, but, as noted above, a good number of them may have understood “beta” in a different way from how finance theorists understood it. Many such managers also seem to have continued in their investment practice much as they always had done, trying to pick “winners,” to “buy low,” and to “sell high.”

Even for managers who were doing no more than paying lip service to finance theory, the theory’s incorporation into market infrastructure mattered. As chapter 3 suggested, stock-picking managers knew that their investment performance was increasingly being monitored by software-implemented measurement systems. They might not fully have understood those systems’ foundations in the Capital Asset Pricing Model, but their behavior changed nonetheless. Many became “closet” index trackers, their self-perceived stock-picking skills limited to modest over-weighting or under-weighting of particular stocks or sectors.

Overall, there is little doubt that finance theory’s incorporation into market infrastructure was consequential: in the terminology sketched in chapter 1, it

constituted “effective performativity.” The stock market itself was affected—first in the United States and then overseas—by the growth of index funds, which were, as chapter 3 noted, inspired in part by ideas of market efficiency.<sup>9</sup> Again, though, it has been in the derivatives markets that the effects of finance theory have been the most profound. As emphasized above, those markets simply could not function on anything like the scale that they currently do without a technical and communicative infrastructure incorporating models that can be traced back to option theory.

Do the technical, linguistic, and legitimacy roles of finance theory amount to Barnesian performativity in the sense outlined in chapter 1? Did finance theory bring into being that of which it spoke? Did its practical use increase the extent to which economic processes or their outcomes resembled their depiction by theory?

Let me briefly address the question of Barnesian performativity in respect to the Modigliani-Miller propositions, the Capital Asset Pricing Model, and the efficient-market hypothesis before turning to my main case: option theory. (Markowitz’s analysis of portfolio selection was explicitly prescriptive—it laid out how selection ought to be done, rather than offering an analysis of how it was done—so all one can ask in its case is how widely it was used. Although I know of no relevant systematic survey, my impression is that Markowitz’s method eventually came to be employed fairly often in the allocation of investments across asset classes: stocks, bonds, property, and so on. However, its use for its original purpose—selection within asset classes such as stocks—remained rarer.)<sup>10</sup>

In principle, the Modigliani-Miller “irrelevance” propositions could have played a role in undermining the traditional view that capital structure and dividend policy mattered: that a well-managed corporation took on only limited debt and paid a good, regular dividend. When the Modigliani-Miller proposition on the irrelevance of capital structure was adjusted to take into account the tax advantages of debt financing, it suggested that the American corporations of the 1950s and the 1960s were operating at levels of debt that were too low. Similarly, the tax disadvantages of dividends by comparison with capital gains suggested that corporate emphasis on the former was irrational.

Have we subsequently moved into more of a Modigliani-Miller world: one in which their “irrelevance” propositions are more true? As noted in chapter 4, that question is very hard to answer empirically. What can, however, be said with some confidence is that attitudes to corporate debt have changed since Modigliani and Miller published their work in the late 1950s and the early 1960s. Then, top executives often seem to have viewed low levels of debt and a high credit rating as signs that they were managing their corporations

competently and with appropriate prudence. Now, executives seem to see advantages to the use of debt to finance corporate activities, and they appear to give a lower priority to achieving high credit ratings. At the start of the 1980s, 32 U.S. non-financial corporations enjoyed Standard & Poor's highest rating, AAA. By 2005, only six did (Riccio and Galeotafiore 2005).

It also seems clear that dividends have become less salient. In 1983, S&P 500 corporations paid dividends equivalent on average to 47 percent of their reported earnings; by 2000, the percentage had declined to 25 (Berenson 2004, p. 78). It is possible that the declining proportion of reported earnings paid as dividends can be accounted for in part by the increasing inflation of the former by "earnings management" (Hatherly, Leung, and MacKenzie, forthcoming). However, some well-established, profitable high-technology companies have never paid a dividend, and one way of interpreting the growing popularity with markets since the early 1980s of stock re-purchases by corporations is that those re-purchases can act as a tax-efficient substitute for dividend payments.<sup>11</sup>

Unfortunately, the role of Modigliani and Miller's work in achieving these partial moves toward a Modigliani-Miller world remains unclear. The most dramatic changes did not take place until the 1980s, some 20 years after their work was published, and precise channels of influence are hard to identify. For example, Michael Milken, the "junk bond king," played perhaps the most influential practical role in encouraging the growth of debt finance by giving less-established companies access to such finance and by facilitating bond-financed hostile takeovers of "under-performing" firms by the celebrated corporate raiders of the 1980s such as Carl Icahn and Ronald Perelman.

Milken did have a background in financial economics. He studied the subject as an undergraduate at the University of California at Berkeley and while pursuing an MBA at the Wharton School of the University of Pennsylvania, before joining the investment bank Drexel Firestone. However, Milken's interest in "junk bonds" (bonds rated as below investment grade) seems to have been sparked not by Modigliani and Miller, but by empirical work on corporate bonds by W. Braddock Hickman and T. R. Atkinson for the National Bureau of Economic Research, which suggested that healthy returns were offered at limited risk by diversified portfolios of bonds with low ratings.<sup>12</sup>

Matters are somewhat clearer in the case of the Capital Asset Pricing Model. It too could be argued not to be testable econometrically: that was the essence of Richard Roll's critique, discussed in chapter 4. However, if one does allow a broad stock index to stand as a proxy for the market in risky assets (which was what Roll objected to), and also permits realized returns to stand as a proxy for expected returns, then the model can be tested relatively straightforwardly. As was discussed in chapter 4, the studies that did this found

at best an approximate agreement between the model and patterns of stock returns.

What is crucial from the viewpoint of Barnesian performativity is that the agreement between the Capital Asset Pricing Model and empirical data does not seem to have improved after the model was formulated. If anything, tests on later data suggested poorer agreement than the original tests, which were conducted using price series stretching back into the period before the Capital Asset Pricing Model was formulated. It is thus difficult to find in the econometric literature any persuasive evidence for the Barnesian performativity of the model.

The Capital Asset Pricing Model certainly had its effects—it reshaped how sophisticated practitioners thought about investment and provided a foundation for systematic performance measurement—but those effects do not seem in general to have shifted prices toward conformity with the model. The Wells Fargo leveraged low-beta fund discussed in chapter 4 might, as noted in that chapter, have begun a performative process in which the model was used as the basis for practical action that could have reduced discrepancies between the model and “reality,” but the fund was stillborn.

In one specific set of contexts, however, the Capital Asset Pricing Model has the power of government behind it. In the United States, the United Kingdom, and other countries, privately owned utilities (such as the suppliers of electricity, natural gas, and water) are often regulated by government-appointed commissions that set the maximum prices that utilities can charge or the maximum rates of profit that they can earn. In so doing, those commissions are typically required to ensure that, in the words of the crucial U.S. legal judgment, the 1949 *Hope* decision, “the return to the equity owner should be commensurate with returns on investments and other enterprises having corresponding risks.”<sup>13</sup>

In the early 1970s, Stewart Myers, a member of the finance group at MIT’s Sloan School of Management, suggested that the Capital Asset Pricing Model should be used to help determine what that “commensurate” rate of return for a regulated utility should be (Myers 1972). Its use for this purpose now seems to be common.<sup>14</sup> If the use of the model in this way actually generated model-compliant rates of return (I know of no empirical study of the issue), it would be an instance of Barnesian performativity.

The case for the Barnesian performativity of the efficient-market hypothesis seems stronger than for the performativity of the Capital Asset Pricing Model. The hypothesis provided a systematic framework within which researchers identified “anomalies”: market phenomena at variance with the hypothesis. Most of those researchers had an efficient-market viewpoint, and

many were Ph.D. students at the University of Chicago's Graduate School of Business, where Fama had formulated the hypothesis and where support for it was strong. Once anomalies were identified, they were often made the object of trading strategies that, in general, seem to have had the effect of reducing their size or even of eliminating them.

Practical action informed by efficient-market theory thus had the effect, at least sometimes, of making markets more consistent with their portrayal by the theory. However, there is one major exception to this pattern. Efficient-market theory's most important practical product was the index fund, and the growth of indexing created an anomaly from the viewpoint of the theory: the sharp increases in price that typically followed a stock's inclusion in a major index, which is an essentially "informationless" event that should not affect prices. (Very recently, this effect seems to have diminished, probably because of the extent to which it is being exploited.)

Option pricing is the aspect of finance theory whose performativity has been examined most closely in the previous chapters. The fit between the Black-Scholes-Merton model and empirical patterns of option prices was originally only approximate, but it improved rapidly after the model was published and adopted by market practitioners. There is no way of being certain that the improved fit was due to the practical adoption of the model: the year of its publication also saw the opening in Chicago of the first modern options exchange, and the development of organized options trading would have had an effect on patterns of prices quite independent of the model.

Nevertheless, the availability of convenient material implementations of the Black-Scholes-Merton model (notably Black's "sheets"), and their use to identify arbitrage opportunities—particularly opportunities for "spreading"—most likely had the effect of reducing discrepancies between empirical prices and the model, especially in the econometrically crucial matter of the flat-line relationship between implied volatility and strike price. As was noted in chapter 6, there was thus a homology between the way the model was tested econometrically and options market practices based on the model. Use of the model does indeed seem to have helped to create patterns of prices consistent with the model.

Gradually, too, the financial markets changed in a way that made the Black-Scholes-Merton model's assumptions, which at first were grossly at variance with market conditions, more realistic. In part, this was a result of technological improvements to price dissemination and transaction processing. It was also a result of the general liberalizing influence of free market economics. At least a small part of the process, however, was the way in which, by making options trading seem legitimate, option theory contributing to the emergence

of the high-volume, efficient, low-transaction-cost liquid market the theory posited.

Furthermore, earlier upsurges of options trading had typically been reversed, not just because of problems of legitimacy (though these were often real enough), but also because option prices were in general probably “too high,” in the sense that they made options a poor purchase: options could too seldom be exercised profitably (Kairys and Valerio 1997). With plausible estimates of volatility, the Black-Scholes formula generated option prices that typically were below those produced by traditional rule-of-thumb heuristics or by models that were “fitted” to pre-existing patterns of prices by empirical techniques.

If the discrepancy between Black-Scholes prices and market prices was seen (as Scholes persuaded Mathew Gladstein: see chapter 6) as a flaw in the market, not in the model, then adoption of the model would have had the effect of reducing prices: it would give the confidence to keep selling options, as Gladstein did, until prices dropped. To the extent that others did this (just how widespread it was isn’t clear), the availability of the Black-Scholes formula and of its associated hedging technique gave options market participants the confidence to write options at prices lower in relative terms than in previous markets.

Keenly priced options in their turn helped the new U.S. options exchanges of the 1970s to grow, to prosper, and to become more efficient, rather than to falter as their predecessors had. Since the Black-Scholes-Merton analysis posits a liquid, zero-transaction-cost options market, this would again be an effect of the model that was performative in the Barnesian sense.

The role played by theory seems also to have been important in the growth of the “over-the-counter” (direct, institution-to-institution) derivatives market. Many of the instruments traded in this market are highly specialized, and sometimes no liquid market, and thus no observable “market price,” exists for them. However, both the vendors of them (most usually investment banks) and at least the more sophisticated purchasers of them can often use analysis in the tradition of Black, Scholes, and Merton to calculate theoretical prices, and thus have a benchmark “fair” price that facilitates negotiation.

As noted above, the theory of derivatives also enables the risks involved in derivatives portfolios to be decomposed mathematically, and in the case of large investment banks, many of these risks turn out to be mutually offsetting. In consequence, the residual risk to be hedged is often quite small in relation to the overall derivatives portfolio of a major financial intermediary such as a big investment bank (Merton 1992, pp. 450–457). Such banks can thus “operate on such a scale that they can provide liquidity as if they had no transaction costs” (Taleb 1998, p. 36).

As a result, the Black-Scholes-Merton model's assumption of zero transaction costs is now close to true for the hedging of derivatives by major investment banks, in part because the use of that model and its descendants by those banks allows them to manage their portfolios in a way that minimizes transaction costs. To the extent that "the equilibrium prices of financial products and services are more closely linked to the costs of the *efficient* actual producers than to *inefficient* potential ones," customers can in consequence purchase derivatives such as options "at nearly the same price *as if* those individuals could trade continuously without cost" (Merton and Bodie 2005, pp. 6–7, emphases in original).

As was noted in chapter 1, the effects on markets of Black-Scholes-Merton option theory could in principle be described in terms simpler than those of performativity: as consequences of the discovery of the correct way to price options. However, the first two phases in the empirical history of option pricing (initially a fit between model and price patterns that was only approximate, and then a much better fit) were followed by the third phase described in chapter 7: the apparently permanent emergence after the 1987 crash of a phenomenon at variance with the model, the volatility skew or smile. To put it crudely, if Black-Scholes is the "right" way to price options, then the market has been wrong since 1987; on the other hand, if a pronounced volatility skew in option prices is "correct," then the market was wrong before 1987.

Given the three-phase history, it is hard to sustain the view that the second phase of the empirical history of option pricing, the close fit between the model and empirical patterns of market prices from the mid 1970s to the summer of 1987, was simply the discovery by market participants of the correct way to price options. It is more plausible to see the third phase, the emergence of the skew, as rational learning from the near-catastrophe of October 1987 (the huge price movements of October 19 can be seen as an emphatic rejection of the null hypothesis of log-normality), but even here there are difficulties. That the distributions of price changes over short time periods had fat tails (high frequencies of extreme events) was known before 1987: Mandelbrot and Fama had made the point vigorously in the 1960s. Furthermore, as discussed in chapter 7, there is a sense in which the options market has "over-learned": the magnitude of the skew is greater than can be accounted for simply by the extent to which the empirical distributions of price changes have fatter tails than the log-normal model underpinning Black-Scholes-Merton.

In chapter 6 I quoted a comment by the finance theorist Stephen Ross that was published in 1987, just before the second phase of the empirical history of option pricing ended: "When judged by its ability to explain the empirical data, option-pricing theory is the most successful theory not only in finance,

but in all of economics.” That success was not discovery of what was already there. As noted above, the option prices of the period before and around the time of the formulation of the Black-Scholes-Merton model fitted the model only approximately. Nor was it discovery of the (timelessly) correct way to price options: that interpretation is sustainable only if one is prepared to say that the market has systematically been wrong since 1987.

The empirical success of the Black-Scholes-Merton model was a historically contingent process in which the model itself played a constitutive role. To say that is in no way to diminish the brilliant achievement of Black, Scholes, and Merton; it would be a curious prejudice to see a theory that changed the world (as their theory did) as inferior to one that merely reported on it. Rather, it is to assert that the model was a theoretical innovation, not simply an empirical observation; that the model’s relation to the market was not always passive, but sometimes active; that its role was not always descriptive, but sometimes performative, even in the strongest, Barnesian sense. An engine, not a camera.

### ***Counterperformativity***

This book has, however, also emphasized that counterperformativity is possible: that practical action based on economic models can undermine the empirical validity of those models. One instance noted above is the way in which the development of index funds—an expression of the efficient-market view that systematic success in stock picking is unlikely—seems to have created an anomaly from the viewpoint of efficient-market theory: the increases in price that typically followed inclusion of a stock in a major index. However, by far the most significant possible instance of counterperformativity is the role of portfolio insurance (which was based, albeit loosely, on a Black-Scholes world, in other words a world in which the assumptions of the Black-Scholes-Merton model were valid) in exacerbating the 1987 crash and thus undermining the Black-Scholes world.

The extent of portfolio insurance’s role in the 1987 crash is not clear. Nevertheless, if it did play a role, it would indeed be a case of counterperformativity. First, on the log-normal model underpinning Black-Scholes-Merton the crash was, as already noted, an event whose probability was vanishingly small. Second, both the crash and, paradoxically, perhaps to an even greater extent the partial recovery that took place immediately afterward are hard to account for in standard efficient-market terms as rational responses to new information. Third, the crash was the historical trauma that, at least in U.S. options markets, gave rise to the volatility skew and thus ended the Barnesian performativity of the classic form of option theory.



It is conceivable that models of the stochastic dynamics of asset price movements may have a general counterperformative aspect if large amounts of practical action (especially in the form of risk-control measures) are based on them. In most of its forms, portfolio insurance depended on continuity in price movements; to the extent that it played a role in the 1987 crash, it generated a discontinuity. While the role of value-at-risk models in the 1998 crisis described in chapter 8 is also unclear, it is possible that sudden, widespread efforts to liquidate similar trading positions in the face of rising value-at-risk estimates contributed to price movements well beyond those envisaged in the models on the basis of which the estimates had been calculated.

In both 1987 and 1998, practical action involving models that incorporated the assumption that randomness is “mild” may thus have helped to generate “wild” randomness. The adoption by the Options Clearing Corporation of Mandelbrot’s wildly random infinite-variance Lévy distributions can also be seen as counterperformative, although in this case deliberately so. By assuming wild randomness, the Options Clearing Corporation hopes to keep randomness mild.

### ***The Financialization of the Corporation***

In the background of the developments discussed in this book stands an epochal change in the nature of the modern corporation. Between the 1950s and the 1990s, the attention paid by corporations to financial markets greatly increased, above all in the United States and the United Kingdom, but increasingly in other countries too.<sup>15</sup> Corporate leaders for whom the financial markets were one concern among many—Roger Lowenstein points out that Alfred P. Sloan Jr., who headed General Motors for four decades, never mentions in his autobiography the price of its stock “as a factor in his decision making” (Lowenstein 2004, p. 29)—were replaced by a generation for whom their corporations’ stock prices were an obsession.

In the first contribution to finance theory discussed in chapter 2, Modigliani and Miller asked what it was that the managers of a corporation should be seen as maximizing. Their answer was the corporation’s market value: “. . . any investment project and its concomitant financing plan must pass only the following test: Will the project, as financed, raise the market value of the firm’s shares? If so, it is worth undertaking; if not, its return is less than the marginal cost of capital to the firm.” (Modigliani and Miller 1958, p. 264)

There is no reason to think that in emphasizing the maximization of market value Modigliani and Miller saw themselves as acting politically. They (Modigliani in particular) were exercised, rather, by an academic issue: the

incoherence of the traditional criterion of profit maximization in a world of uncertainty. If “profit outcome” is “a random variable,” they wrote, “its maximization no longer has an operational meaning” (Modigliani and Miller 1958, p. 263). However, as Jack Treynor (for whom this aspect of Modigliani and Miller’s work was central) realized, the assertion of the centrality of market-value maximization could be read as a statement of how things ought to be.

Finance theory looked at the corporation from the “outside”: from the viewpoint of the investor and of the financial markets. There is, therefore, an affinity between finance theory’s view of the corporation and the way in which the governance of corporate life, especially of American corporate life, evolved. The economist Alan Blinder (2000, p. 18) puts it well: “. . . the stark assumption of [market] value maximization comes much closer to the mark today than it did a decade or two ago.”

Just how important financial economics was in “the cultural frames of actors” (Fligstein and Markowitz 1993, p. 185) involved in the “financialization” of American corporations—in the sense of the growing priority of the maximization of market value—largely remains to be investigated. It certainly was not the only factor. A different area of economics, agency theory, was the main academic source of legitimacy for a crucial aspect of the change, the greatly increased proportion of corporate executives’ rewards that came in the form of stock and stock options.<sup>16</sup>

The “market for corporate control” sparked by the hostile acquisitions of the 1980s was another cause of the financialization of American corporations, and financial economics played only a minor part in the emergence of this market. (See the discussion of Michael Milken above.) The wider political and economic changes both symbolized and encouraged by the presidency of Ronald Reagan—and the accommodation with those changes reached in the 1990s by the Democrats—were clearly important too. Free-market economics was of obvious importance to those changes, but financial economics specifically did not play a prominent role.

The case for seeing the growing priority given by American corporations to market-value maximization as a direct performative effect of financial economics is therefore weak. However, once the students in American business schools realized the strength of the influence of the financial markets on the fate of the corporations that were going to employ them—and increasingly on their own personal wealth too—they would be likely to be far keener to learn about those markets than they had been in the 1950s, when the Harvard Business School’s course on investments was the despised “darkness at noon” described in chapter 3.

It is also plausible—though I know of no direct evidence on the point—that when the students who graduated with their MBAs from American business schools from the 1960s on came to occupy senior positions in corporations, they would have remembered at least some of what they had been taught in courses on the new financial economics. They would thus know that the goal of market-value maximization could be defended as being right, as well as being career-enhancing and wealth-enlarging. Many people were exposed to financial economics in the course of studying for the MBA degree. By the end of the 1990s, U.S. business schools were awarding about 100,000 MBAs a year (Skapinker 2002). The growing prestige of financial economics and the increasing emphasis in American corporate life on market-value maximization may thus have been to at least some degree mutually self-reinforcing.

Furthermore, corporations in the United States and elsewhere seem increasingly to be becoming “financialized” in a sense additional to that of market-value maximization. The use of derivatives to manage financial risks such as exchange-rate and interest-rate fluctuations is established, but corporations now also have available to them derivatives of a growing range of physical substances and processes: for example, oil and gasoline derivatives, natural gas derivatives, even weather derivatives.<sup>17</sup>

Derivatives of this kind allow risks that traditionally had to be managed by physical or organizational means to be hedged financially. Oil companies, for example, traditionally sought to control risk by vertical integration—ownership of an entire supply chain from oil fields, through transportation and refining, and into retail—but the availability of oil and gasoline derivatives offers another route to the same goal (Merton 1998, p. 59).

The use of derivatives for risk management is less straightforward than it might seem, as a variety of corporate catastrophes have shown. Sometimes those catastrophes are the result simply of hedging having tipped over into speculation, of weak management controls allowing “rogue trading,” or of Wall Street’s exploitation of corporate naiveté.

However, one of the most famous of the corporate catastrophes was the crippling \$1.3 billion loss suffered in the early 1990s by Metallgesellschaft AG as a result of the over-ambitious but sensibly motivated use of oil and gasoline futures to hedge fixed-price contracts with the corporation’s customers.<sup>18</sup> The difficulties of Enron, a pioneer of energy derivatives and of the “asset-light” virtualized corporation, are also notorious. (See MacKenzie 2003a.) Nevertheless, the use of derivatives is one factor making possible corporate structures in which the ownership of physical assets is of decreasing importance, risks are hedged financially, and the ownership of “virtual” assets such as intellectual property rights is more important.

### ***Performativity and Economic Sociology***

Michel Callon's critic Daniel Miller argues that economic sociology and anthropology should "radically separate out the market as a ritual and ideological system constructed by economists and the actual practice of economics" (D. Miller 2002, p. 224). In the sections above, we have seen a range of performative effects of financial economics. They vary in importance and in the extent to which they approximate to strong, Barnesian performativity. Nevertheless, it is clear that in at least some contexts—most obviously in the derivatives markets—the radical separation called for by Miller is impossible. The economic theory of derivatives and the "actual practice" of derivatives markets are interwoven too intimately.<sup>19</sup>

However, when in pursuit of a theme such as the performativity of economics, one should not give in to the temptation to exaggerate its significance. In particular, attention to performativity does not render redundant economic sociology's focus on the embedding of economic action in cultures, in political systems, and in networks of interpersonal connections.<sup>20</sup> Especially in chapter 6, we saw performativity articulating with these matters, rather than displacing them.

Therefore, we must be careful how we interpret the following assertion:

... yes, *homo economicus* does exist, but is not an a-historical reality; he does not describe the hidden nature of the human being. He is the result of a process of configuration. ... Of course it mobilizes material and metrological investments, property rights and money, but we should not forget the essential contribution of economics in the performing of the economy. (Callon, 1998, pp. 22–23)<sup>21</sup>

The market participants discussed in earlier chapters were certainly economically calculative, and—as Callon's formulation would suggest—their calculations were often conducted using procedures and devices made possible by finance theory. However, they did not become *Homines economici* in the sense of atomistic, amoral egotists.

For example, chapter 6 suggested that it was the mutual susceptibility of human beings—our susceptibility to the "symbolic sanctioning considered as an aspect of the communicative interaction that is normal and natural to us as social beings" (Barnes 1992, p. 263)—that helped make possible the solution of the collective action problem represented by the very creation of the Chicago derivatives exchanges. Some members of those exchanges acted for the common good of the exchange, rather than behaving as rational egotists. The respect they gained helped them persuade others to contribute too, rather than to free-ride.

The more stable prices in smaller options “crowds” observed by Wayne Baker (1981; 1984a,b) were also a form of collective action. Even the volatility skew can in a sense be seen as the upshot of collective action. If the econometric analyses discussed in chapter 7 are correct, it would be perfectly rational for an individual trader to sell index puts, especially low-strike-price index puts, at prices somewhat below those that have prevailed since 1987: for example, to set prices with a skew reflecting at most only the extent of empirical departures from log-normality.

Yet if unrestrained sales of puts were engaged in too widely (especially without hedging, or with forms of hedging that simply pass risk on to other participants in the options market), a collective danger would be created: that another 1987 could undermine the very foundations of the options market. At least the more extreme manifestations of that kind of trading, even if individually rational, are thus seen as immoral. To indulge in them is to be a “shit seller,” and they have been kept in check by collective measures that are partly formal (such as margin requirements based on risk levels calculated using Mandelbrot’s Lévy distributions) and partly informal. Throughout, it has remained important that Chicago is a place of rough-and-ready, but economically significant, market morality, a place with “an invisible sheet with an invisible line down the middle of it.”

Chicago’s open-outcry pits are unusual in their intensity of social interaction, and even in Chicago such pits are now in decline. Elsewhere, they have nearly vanished. The telephone-mediated and screen-based trading that is replacing them does not promote such intense interaction. Even in those markets, however, personal interconnections and mutual susceptibility are evident. Those were, for example, the markets within which LTCM mainly traded. Interviewee David Wenman’s use of the phrase “arbitrage community” is not happenstance: arbitrageurs in the same markets often seem to know each other and to be affected by each other.

“Community” does not imply harmony. For example, one interviewee at LTCM suggested that it had generated resentment among Wall Street investment banks (for instance by pressing hard to reduce “haircuts”) and that traders in those banks “were, I think, jealous of the money we made.” Jealousy, however, is a quintessentially social emotion. A trader who is concerned less by the absolute size of his or her bonus than by its size relative to others’ bonuses (which appears to be not uncommon) is behaving “socially,” not as an atomistic individual.

The networks of connections to personally known others on which the economic sociologist Mark Granovetter (1973, 1985) has focused are also in evidence in respect to the legitimacy role of economics in facilitating the

emergence of derivatives markets. The persuasive power of economics did not on its own create the Chicago Board Options Exchange and the Merc's International Monetary Market. That persuasive power flowed through a network of interpersonal connections: in the case of the Options Exchange, from Baumol, Malkiel, and Quandt, via Edmund O'Connor and Nathan Associates, to Milton Cohen, and, crucially, to William Casey; in the case of the Merc, from Milton Friedman to Arthur Burns, George Shultz, Bill Simon, and others in Washington.

Nor did the significance of embedding of the kind studied by Granovetter cease once the Chicago derivatives markets were established. Baker's research demonstrates its economic significance for the trading of options. Without the network of interpersonal connections mobilized by Melamed on the night of October 19, 1987, the American derivatives exchanges might have ceased to exist.

Furthermore, wider cultural-political issues also mediate the legitimacy role of economics. In the United States in the late 1960s and the early 1970s, economics had persuasive force, particularly with the Nixon Administration, committed as it was to freeing markets from their Rooseveltian shackles. In contrast, in Malaysia in the late 1990s and the early 2000s a similar battle to permit derivatives trading was fought, but those engaged in it appealed for legitimacy to Islamic jurisprudence, not to neoclassical economics (Kamali 1997; Maurer 2001).

Part of the significance of the performativity of economics arises from the fact that individual human beings tend to have quite limited powers of memory, information-processing, and calculation.<sup>22</sup> Those limitations explain the centrality—especially in complex markets—of simplifying concepts (e.g., “implied volatility”) and of material means of calculation (e.g., Black's sheets and the Chicago Board Options Exchange's Autoquote system).

That concepts and material means are therefore constitutive of economic action—that economic action involves distributed cognition in the sense of Hutchins (1995a,b)—implies that the economic theory crystallized in concepts and devices is performed. Sometimes, I have suggested, this performativity is *Barnesian*: it helps to create the phenomena the theory posits.

There is thus a sense in which the capacity for economics to be performative arises from the cognitive limitations of human beings—limitations that require standard models of the rational actor to be weakened. However, in order better to understand economic behavior, even in financial markets, it is also necessary to enrich standard models of the rational actor to encompass ways in which we affect each other that are not reducible to calculation in the ordinary sense of the word. We are often profoundly influenced by

anticipation of how our behavior will appear to others, even if their approval or their disapproval has no direct material consequences. (See, for example, Scheff 1988.)

A simultaneous weakening and enriching of standard views of the rational actor is thus one way of expressing the articulation between performativity and economic sociology's emphasis on the cultural, political, and interpersonal embedding of economic action. Human beings need to be viewed both as limited in their cognitive capacity—which is one reason why distributed cognition and performativity matter—and also as mutually susceptible in the sense explored by Scheff or by Barnes (1992), which is one reason why the concerns of economic sociology remain relevant.

### ***Behavioral Finance, Performativity, and the Sociology of Markets***

The weakening of standard views of rational economic actors is currently prominent in “behavioral finance,” which portrays investors as prone to systematic biases.<sup>23</sup> For instance, the “prospect theory” put forward by Daniel Kahneman and Amos Tversky (1979) points to the way in which attitudes to a risky decision are affected by how the decision is framed in relation to subjectively important reference points. When psychologists confront experimental subjects with risky decisions, those subjects are typically risk averse with respect to perceived gains, but risk seeking with respect to what are seen as losses. They are prepared to “gamble” to recoup a perceived loss, but are less willing to endanger gains.

Differences in the way in which the same decision (the choice between courses of action involving identical outcomes with identical probabilities) is framed in its presentation to experimental subjects can therefore lead them to choose differently. In the case of professional traders in financial markets, the relevant subjective reference point may be an anticipated level of bonus, and Kahneman and Tversky's prospect theory would suggest that “a trader expecting a good performance (and thus bonus) outcome will be reluctant to put those anticipated gains at risk whereas a trader anticipating a poor outcome will be more willing to take risks to avoid that negative outcome” (Fenton-O'Creevy, Nicholson, Soane, and Willman 2005, p. 181).

Behavioral finance has had a mixed reception within financial economics. It has sparked considerable interest, but many people prominent in the field are deeply skeptical. (See, for example, Fama 1998; Ross 2001; Rubinstein 2001.) Sometimes, behavioral finance seems to me to be too narrowly psychological and individualistic in its focus. Take, for example, the above prospect-theory prediction of risk aversion in the domain of perceived gains

and risk seeking in the domain of perceived losses. Individual traders may well be predisposed to act in those ways, but the predisposition is countered by two factors that are cultural and organizational rather than psychological.

One factor is the lore of traders, in which perhaps the most common maxim—"run your profits and cut your losses" (Fenton-O'Creevy, Nicholson, Soane, and Willman 2005, p. 192)—directly contradicts the predisposition posited by prospect theory. The maxim enjoins traders to continue to hold risky positions when in the domain of gains and to liquidate those in the domain of losses. The need for the maxim suggests that prospect theory's diagnosis of the typical predisposition is correct, but if the maxim is influential it will counter the predisposition. A second factor again may provide both evidence of the predisposition and a mechanism countering it: the role of traders' managers, part of whose job is often to discourage undue risk aversion while also detecting and stopping the dangerous "doubling up" of loss-bearing trades (Fenton-O'Creevy, Nicholson, Soane, and Willman 2005).

A pitfall to avoid in discussing such matters is the assumption that economic sociology and the social studies of finance examine "irrational" behavior while economics deals with "rational" behavior. As noted above, economic sociology in the tradition of Mark Granovetter shows the role of the structure of networks of connections to personally known others in the explanation of economic processes and of their outcomes. (For examples, see Baker 1984a, Burt 1992, and Podolny 2001.) Such networks are often cultivated deliberately for perfectly "rational" reasons, as in the case of the cultivation by Meriwether's group at Salomon of those responsible for repo.

The trading activities of both Meriwether's group and Dimensional Fund Advisors were informed by financial economics. However, as emphasized in chapters 4 and 8, both sets of traders also had to be good practical economic sociologists, and there is no contradiction between the two aspects of their trading behavior. For example, in order successfully to exploit in practice what Banz's research had shown about the returns to small stocks, Dimensional's traders had to think constantly about the identities of the individuals or firms who were offering to sell them such stocks, what information those individuals or firms were likely to possess, and how trustworthy they had been in the past.

Indeed, when the social studies of finance focuses on market infrastructure, what is being examined are the preconditions for "rational" economic action: the technical systems, procedures, ways of communicating, and so on, that make such action possible. As suggested above, the field can be seen as sharing behavioral finance's view that the cognitive capacities of unaided individual human beings are limited. However, the social studies of finance also



emphasizes the ways in which sophisticated economic calculations are nevertheless made possible by material devices (the computerized equivalents of Black's sheets, for example), by organizational routines, by concepts (such as "implied volatility") that simplify complex realities, and so on. An economic actor equipped with all of these is quite different from an unaided human individual.

Indeed, markets themselves can be seen as prostheses in the sense that they enable human beings to achieve outcomes that go beyond their individual cognitive grasp. The classic examples are the demonstrations that when placed in a double-auction market akin in its structure to open-outcry trading, both untutored human beings and even computerized "zero-intelligence traders" submitting "random bids and offers" achieve economic outcomes close to those predicted by theories that posit fully rational economic actors (Smith 1991; Gode and Sunder 1993, p. 119). Markets can indeed be seen as machines (Mirowski 2002) or as devices for collective calculation (Callon and Muniesa 2003), and with the increasing implementation of market mechanisms in software those are not simply metaphors.<sup>24</sup>

In emphasizing human cognitive limitations, as behavioral finance rightly does, we therefore need also to remember the interactions between those limitations and the mechanisms, both "social" and "technological," that constitute markets. The interactions by no means always eliminate the effects of the limitations—the crises of 1987 and 1998 can be seen as cases in which they exacerbated them—but I suspect they are always important.

### ***The Limits of Arbitrage***

Of the market processes of relevance to the relations among "orthodox" financial economics, behavioral finance, and social studies of finance, the most prominent is arbitrage. As Paul Harrison argues, arbitrage "was the notion that allowed economics to revolutionize finance and which simultaneously brought finance into the fold of economics" (1997, p. 173).

Arbitrage is also a crucial mechanism by which finance theory affects markets. For example, the most important channel by which the Barnesian performativity of finance theory is achieved is the use of the theory to identify, exploit, and thus diminish discrepancies between "model" and "reality." Examples include the "spreading" arbitrage in options markets that seems to have contributed to moving price patterns toward the Black-Scholes-Merton model, and the arbitrage-like exploitation of "anomalies" that helped to eliminate them.

An influential response to behavioral finance by proponents of market efficiency is to point out that even if some, or nearly all, investors are subject to identical behavioral biases, the patterns of prices posited by efficient-market theory would still pertain if the resultant departures from those patterns can be exploited by arbitrage. Stephen Ross, whose option-theory work was described briefly in chapter 5 and appendix E, puts it this way:

I, for one, never thought that people—myself included—were all that rational in their behavior. To the contrary, I am always amazed at what people do. But, that was never the point of financial theory.

The absence of arbitrage requires that there be enough well financed and smart investors to close arbitrage opportunities when they appear. . . . Neoclassical finance is a theory of sharks [arbitrageurs] and not a theory of rational *homo economicus*, and that is the principal distinction between finance and traditional economics. In most economic models aggregate demand depends on average demand and for that reason, traditional economic theories require the average individual to be rational. In liquid securities markets, though, profit opportunities bring about infinite discrepancies between demand and supply. Well financed arbitrageurs spot these opportunities, pile on, and by their actions they close aberrant price differentials. . . . Rational finance has stripped the assumptions [about the behavior of investors] down to only those required to support efficient markets and the absence of arbitrage, and has worked very hard to rid the field of its sensitivity to the psychological vagaries of investors. (Ross 2001, p. 4)

If, in contrast, the potential for arbitrage is constrained, then discrepancies between finance-theory models and “reality” may persist, and the effects on patterns of prices of the phenomena pointed to by behavioral finance may not be eliminated. For example, much of the interviewing for this book took place during the boom in dotcom and telecommunications stocks at the end of the 1990s and the beginning of the 2000s.

Although I did not focus on the boom in technology stocks, incidental comments by interviewees made clear that they were deeply skeptical of the valuations the market was placing on many of the companies involved. (For a quantitative analysis of the extreme degree of optimism implied by those valuations, see Ofek and Richardson 2002.) The strain such valuations placed on the beliefs of an efficient-market theorist who also had responsibility for practical investment decisions was, Stephen Ross memorably told me, “like a Christian Scientist with appendicitis” (Ross interview).

However, two partners in LTCM independently pointed out to me the impossibility of making arbitrage profits from exaggerated valuations. Suppose they had been vouchsafed a little peek into the future: that they knew, with absolute certainty, that at a particular point in time the stock price of company X would be zero. Could they now, they asked me, make money with certainty

from this knowledge? Their question was rhetorical; they knew the answer to be “No.” Of course, they could sell the stock short. If they could hold their position until the stock price became zero, they could indeed profit handsomely. But if in the interim the price continued to rise, they could still exhaust their capital and be forced to liquidate at a loss.<sup>25</sup>

Perhaps the most interesting single line of work in behavioral finance—pioneered especially by Andrei Shleifer—has been the modeling of situations of the kind referred to by my LTCM interviewees.<sup>26</sup> These are situations in which the powers of arbitrage are limited, for example because arbitrageurs have access to only finite amounts of capital, and temporarily adverse price movements can generate margin calls and other demands on capital.

If arbitrageurs are constrained by finite supplies of capital, even an arbitrage trade that is certain to converge profitably may nevertheless have to be abandoned at a loss, for instance because those who provide arbitrageurs with capital withdraw it prematurely in the face of adverse price movements. The analysis of this risk in Shleifer and Vishny 1997 was prescient: as chapter 8 suggests, a flight of arbitrage capital of this kind was integral to the crisis surrounding Long-Term Capital Management. Work on the limits of arbitrage has now moved on from modeling to empirical analysis. In 2002 a double issue of the *Journal of Financial Economics* was devoted to the topic.<sup>27</sup>

Arbitrage typically requires buying an “underpriced” asset (or establishing a “long” position in it by other means), while simultaneously short-selling a similar “overpriced” asset. One important issue in terms of the limits of arbitrage is thus the asymmetry in many financial markets, especially stock markets, whereby “short” positions can sometimes be much harder to establish and to maintain than “long” positions. The issue blends into a more general one: “positive” opinions on, or information about, stock valuations are typically much easier to “register” than negative opinions. Virtually any individual or institution with the requisite funds can register a positive opinion by buying stock. Registering a negative opinion or negative information by short-selling stock is much harder and much rarer.<sup>28</sup>

Many institutions are in effect prohibited from short-selling, and in the United States short sales are in general legal only if made on an “uptick” (that is, after an upward movement of the price of the stock in question), although at the time of writing the uptick rule was under review. It can be hard to borrow stocks and the cost of doing so can be high. There is the risk of a “short squeeze,” in which it becomes impossible or too expensive to keep borrowing stock (for example, because the lenders of the stock demand it back). In such a squeeze, short-sellers may have no alternative but to “cover” their short positions by buying stock, sometimes at high cost.

Added to the constraints on short sales is what may be a tendency for negative information to remain “bottled up” within corporations for longer than positive information. Even perfectly orthodox financial economists suspect that the result may be that the efficiency of the incorporation into prices of negative information and opinions (for example, the gradually increasing knowledge that the corporate earnings reported in the late 1990s and the early 2000s often were inflated<sup>29</sup>) is sometimes less than the efficiency of the incorporation of positive information.

If negative information is incorporated into prices less efficiently than positive information, one might expect an intermittent upward “bias” to stock prices punctuated by occasional steep declines as “pessimistic information . . . largely hidden from other investors, particularly after a market rise, because of constraints on short sales” becomes apparent as the “market [begins] to fall” and “pessimistic investors fail . . . to materialize as buyers” (Rubinstein 2001, p. 26, summarizing a preprint version of Hong and Stein 2003). The mechanism could, for example, explain the dangerous tendency for stock-market declines to be much more rapid than rises, and Mark Rubinstein—who is on the orthodox side of the divide with behavioral finance—believes it may be part of the explanation of the 1987 crash (Rubinstein interview).<sup>30</sup>

Another pivotal issue in terms of arbitrage is the material and cognitive construction of what one might call “sufficient similarity.” The superb ethnographic study of a Wall Street investment bank arbitrage trading room by Daniel Beunza and David Stark shows the extent to which such a room is organized—in terms of physical layout, technical systems, and human interaction—in order to find ways in which pairs of securities can be made comparable: “similar enough so that their prices change in related ways, but different enough so that other traders have not perceived the correspondence” (Beunza and Stark 2004, p. 375). The Black-Scholes-Merton model, for example, can be seen as a paradigmatic contribution to what Beunza and Stark (p. 369) call the “art of association” that is the heart of arbitrage. Market practitioners had already intuited that it was possible to use purchases and sales of stock to construct a position “sufficiently similar” to an option or warrant for arbitrage to be possible. Black, Scholes, and Merton gave precise quantitative form to the intuition.

Whether “sufficiently similar” securities are available to make arbitrage feasible bears centrally on market efficiency. Paul Samuelson’s recent writing suggests that markets may be efficient in a “micro” sense, since discrepancies in relative value can be arbitrated away (because “similar” assets are available), but not in a “macro” sense, because “bubbles” that affect the prices of *all* similar assets cannot be arbitrated (Samuelson 2001). For example, “there does

not exist a close substitute that could be used to hedge a short position in the technology sector” (Brunnermeier and Nagel 2004, p. 2014). Someone who was convinced that the dotcom boom was a bubble and that it would eventually burst would, as noted above, have been taking a major risk in short-selling dotcom stocks, or in taking a short position in them in other ways. Being “right” about the over-valuation of the stocks involved would not have removed the risk.<sup>31</sup>

Had the dotcom bubble been an arbitrage opportunity, one would have expected hedge funds to exploit it. Instead, Brunnermeier and Nagel (2004) find that hedge funds generally “rode” the bubble, increasing their holdings of technology stocks as the bubble inflated, and seeking (with some success) to exit before it crashed.

Julian Robertson, the leading hedge fund manager who was most skeptical of technology stocks, simply liquidated his holdings of them in 1999, when the bubble was approaching its maximum, rather than take a short position on the sector. His Jaguar Fund nevertheless suffered large investor withdrawals, presumably as investors shifted capital to those still heavily involved in technology stocks. In March 2000—ironically, the point at which in retrospect the bubble can be seen as starting to burst—Robertson had to decide to shut the fund down (Brunnermeier and Nagel 2004, p. 2032).

### ***Imitation***

Another potentially crucial issue in respect to the limits of arbitrage is the apparent propensity of others to seek to imitate successful arbitrageurs. Although economists too have become increasingly interested in imitation, the underlying general point is prominent in economic sociology, and has been emphasized, for example, by White (1981, 2001) and Fligstein (1996, 2001).<sup>32</sup>

Firms and other economic actors do not choose their courses of action in isolation: they monitor each other, and make inferences about the uncertain situation they face by noting the success or failure of others’ strategies. When this leads to diversity—to firms selecting different strategies and coming to occupy different “niches”—a stable market structure can result. But if firms imitate, each choosing the same strategy, potentially disastrous “crowding” can occur.<sup>33</sup>

“Crowding” is what appears to have taken place in global arbitrage in the 1990s. As discussed in chapter 8, it seems to have been driven at least in part by imitation of Long-Term Capital Management. The consequence was the sudden appearance in the crisis of 1998 of substantial correlations between

arbitrage positions that had little or nothing in common at the level of economic fundamentals and which previously had manifested very low correlations.

The possibility of imitation-induced correlation tightens the limits to arbitrage caused by the risk, discussed above, of the “flight” of capital-constrained arbitrageurs in the face of temporarily adverse price movements. If correlations between different arbitrage positions could be relied upon to remain low, the risk of forced flight could be reduced considerably by holding a large portfolio of diverse arbitrage positions, as Shleifer and Vishny acknowledge (1997, p. 47). However, as LTCM found, diversification is of little avail if correlation can suddenly appear around the world and across asset classes when things “go bad.”

That was the lesson that LTCM’s successor fund, JWM Partners, drew from its predecessor’s crisis. In JWM Partners’ risk model, all the fund’s positions, no matter how diversified geographically or how unrelated in asset type, are now assumed to have correlations of 1.0 “to the worst event” (Meriwether interview). In other words, it is assumed that in an extreme crisis all the fund’s positions may move in lock-step and adversely, even those positions that would in the abstract be expected to rise in relative value in a crisis (in other words, the cases in which it holds the more liquid or more creditworthy of two instruments and has a short position in its less liquid or less creditworthy counterpart).

JWM Partners’ revised risk model thus incorporates the possibility that an imitative, overlapping arbitrage “superportfolio” will again come into being, and that its unwinding will not merely exacerbate a crisis but will even override the “flight to quality” that typically accompanies crisis. The new risk model was informed by the experience of LTCM’s 1998 crisis, not by economic sociology or by the social studies of finance, but it is an acknowledgment of the social, imitative aspect of financial risk.

### ***Conversations about Markets***

As was noted in chapter 1, this book embodies a hope: that the social studies of finance—and, more generally, a broadening of the disciplinary bases of research on financial markets—may encourage richer conversations about those markets, and may thus begin to widen the basis for a politics of market design that is nuanced, flexible, modest, cautious, and open-minded (including on the topic of whether markets are appropriate solutions to the problems at issue). The conversations and the politics that I want to encourage would

treat markets not in the abstract but concretely, and would delve into apparent detail as well as discussing markets' overall virtues and disadvantages.

Consider one example of an apparent detail discussed briefly above: the "uptick rule," constraining short sales of stock. It is a deliberate design feature of U.S. stock markets, introduced in 1938 (Seligman 1982, p. 165). On the surface, the rule is a minor matter: active investors aside, most American citizens have most likely not heard of it, and of those who have, fewer still probably care about it. Yet the uptick rule is no *mere* technicality. Although its significance has been reduced by the availability of derivatives (index futures, options, and now single-stock futures) that allow short positions to be constructed in other ways, the uptick rule remains one of a number of features that continue to build a substantial asymmetry into the design of the U.S. stock market.

There is, furthermore, a sense in which significant strands of American history and culture are condensed in the uptick rule. These include images of powerful stock-market operators mounting manipulative "bear raids" in order to profit by forcing down stock prices. Such images were particularly potent in Depression-era attacks (such as Foster 1932) on those "selling the United States short": those attacks were the immediate context of the rule's original imposition. In the background to the uptick rule is perhaps also a vague but pervasive sense that being "bullish" is "American" and of wide benefit, while being "bearish" is un-American and of only private benefit.<sup>34</sup>

I raise the topic of the uptick rule not to express a view on its desirability, but because it is an example of a general point: the financial and other markets of high modernity are to a significant extent entities subject to deliberate design, and the details of their design are consequential. For example, the financial derivatives exchanges discussed in chapter 6 were conscious creations, and U.S. stock markets have been thoroughly remodeled over the years. We have seen in earlier chapters both how the design of those markets interacted with broader issues (such as the legal prohibition on gambling) and how that design mattered to economic processes.

Among new, deliberately designed markets outside the sphere of finance are the market in permits to emit sulfur dioxide set up in the United States in the 1990s, and the analogous but even more important carbon dioxide emissions markets that are being shaped at the time of writing. Sometimes market design—understood broadly enough—is, quite literally, a life-and-death matter: consider, for example, the global pharmaceuticals market. Economics is already making important practical as well as theoretical contributions to market design.<sup>35</sup> The other social sciences, however, have hardly begun.

As argued above, the analogy between markets and technologies may be a productive one for theorizing markets. It also suggests something more. Markets, like technologies, are surely means—to be tinkered with, modified, redesigned, improved, and on occasion delimited—not ends that can only be embraced or be rejected. They are not forces of nature, but human creations. More fruitful conversations about them, involving a far wider range of participants than currently, are much to be desired. To be sure, as chapter 1 acknowledged, in a world of vested interests and huge inequalities the forces constraining those conversations are strong, but that is no reason to avoid seeking to embark on them.

To examine the performativity of finance theory—in other words, to investigate how the development of our culture’s most authoritative form of knowledge of financial markets has affected those markets—is but one of many ways our conversations about markets can be enriched. I hope, nevertheless, that this book shows it to be a useful one.

The notion of “performativity” is no panacea, and I trust I have emphasized adequately its limitations. Performativity has, however, three virtues. First, when confronted with a theory or model it is natural to ask: is it accurate? Keeping performativity in mind reminds us also to ask: if the model is adopted and used widely, what will its effects be? What will the use of the model *do*? Second, because performativity often resides not just in “big ideas” but also in apparently small technicalities, the notion reminds us also to pay attention to the latter, and to consider the role they play in the infrastructures of markets: in the material devices, procedures, routines, rules, and design features that make markets what they are. And, finally, the notion of performativity prompts the most important question of all: What sort of a world do we want to see performed?





## ***Appendix A***

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### ***An Example of Modigliani and Miller's “Arbitrage Proof” of the Irrelevance of Capital Structure to Total Market Value***

Modigliani and Miller (1958) present their reasoning algebraically, but its logic can be grasped by considering a numerical example (taken, with some modification to make it correspond more closely to the detail of Modigliani and Miller's original argument, from pp. 172–173 of Bernstein 1992).<sup>1</sup> Modigliani and Miller imagine firms divided up into “risk classes.” Within a risk class, “the shares of different firms are ‘homogeneous,’ that is, perfect substitutes for one another” (Modigliani and Miller 1958, p. 266). They are entitlements to income streams with identical characteristics: streams that, a constant of proportionality aside, will be the same “under all circumstances” (Modigliani and Miller 1958, p. 269).

Imagine two firms, A and B, in the same risk class, each with an “expected profit before deduction of interest [on bonds]” (Modigliani and Miller 1958, p. 268) of \$100 million per year. Firm A is unleveraged: it has issued no bonds and taken on no other form of debt. Imagine that its market value—the total value of its stock—is \$1 billion. Firm B is leveraged: some of its capital has been raised by issuing \$500 million of bonds, which pay 5 percent per annum in interest. (That is also the rate at which individuals can borrow money: in Modigliani and Miller's model the two rates of interest are equal.)

Suppose B's total market value is currently higher than A's—for example, that B's stock is worth \$550 million, which, with its \$500 million of bonds, is a total market value of B of \$1.05 billion. If that is the case, Modigliani and Miller argued, a holder of B's stock can earn a greater expected return, without incurring extra risk, by liquidating his position in B and taking on a leveraged position in A's stock, employing personal borrowing so that the position is leveraged at exactly the same ratio as the stock of B is leveraged by corporate borrowing.

Imagine, for example, that an investor holds 1 percent of B's stock, a holding which is currently worth \$5.5 million. The expected return on that holding is therefore 1 percent of whatever is left to B's stockholders after B's

bondholders receive their interest payments. B's total expected income is \$100 million, and the bondholders receive 5 percent of \$500 million, or \$25 million, leaving \$75 million for the shareholders. So a holding of 1 percent of B's stock has an expected return of \$0.75 million, or \$750,000.

To B's \$550 million of stock corresponds \$500 million of borrowing (that is, of bonds). So what the investor does is to sell her holding of B's stock, yielding \$5.5 million, to borrow another \$5 million, and to buy \$10.5 million of A's stock. This stock is an entitlement to an expected income stream of \$1.05 million. The investor has to pay interest of 5 percent on the borrowed \$5 million, in other words \$0.25 million, so her net expected income is \$0.8 million, or \$800,000. That is \$50,000 more than she would expect to earn from her original holding of B's stock, and no additional risk has been taken.

The operation yields increased expected income whenever B's total market value is higher than A's, but not when the two total market values are equal. Suppose that is the case: that B's stock is worth \$500 million, which when added to B's \$500 million of bonds gives a total market value equal to A's \$1 billion. An investor who holds 1 percent of B's stock still has an expected return from that stock of \$750,000. Let her repeat the same operation, selling her holding of B for \$5 million, taking on equivalent leverage by borrowing \$5 million, and buying \$10 million of stock in A. That stock is an entitlement to an expected income of \$1 million, which after the deduction of \$250,000 interest on the sum borrowed, is \$750,000, precisely the same as the expected return from her holding of B's stock. (Of course, the investor can increase her expected return by borrowing more money and buying more of A's stock, but if she does that then the position she has constructed is more highly leveraged—and so more risky—than the original holding of B's stock.)

Thus, what stops the leveraged firm (B) from having a higher total market value than the unleveraged firm (A) is that “investors have the opportunity of putting the equivalent leverage into their portfolio directly by borrowing on personal account.” Similarly, if B's total market value becomes less than A's, an investor can sell stocks in A and earn a higher expected income, at the same level of risk, by buying an appropriate combination of B's stocks and bonds. “It is this possibility of undoing leverage which prevents the value of levered firms from being consistently less than those of unlevered firms.” (Modigliani and Miller 1958, p. 270) There is equilibrium—no possibility of increased expected return without taking on extra risk—only if the total market values of the leveraged firm and the unleveraged firm are identical.

## Appendix B

### Lévy Distributions

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The distributions of continuous random variables are normally written in terms of their “probability density functions.” If  $X$  is a continuous random variable with probability density function  $f(x)$ , then—to express matters loosely—the probability that  $X$  falls in the small interval between  $x$  and  $x + dx$  is  $f(x)dx$ . For example, the probability density function for a normal distribution with zero mean and unit standard deviation is

$$\frac{1}{\sqrt{2\pi}} \exp(-x^2/2).$$

Special cases aside, no equivalent explicit form, using only elementary functions, is known for the probability density function of a Lévy distribution. So a Lévy distribution is most easily expressed using what is called the “characteristic function.” If  $X$  is a continuous random variable with probability density function  $f(x)$ , its characteristic function  $g(u)$  is

$$g(u) = \int_{-\infty}^{\infty} f(x) \exp(iux) dx,$$

where  $u$  is real and  $i = \sqrt{-1}$ . The natural logarithm of the characteristic function for a Lévy distribution is (following Gnedenko and Kolmogorov 1954, p. 164 or Fama 1963, pp. 421–422)<sup>1</sup>

$$i\delta u - \gamma|u|^\alpha [1 + i\beta(u/|u|)\tan(\alpha\pi/2)]$$

unless  $\alpha = 1$ , in which case  $\tan(\alpha\pi/2)$  is replaced by  $2(\ln|u|)/\pi$ , where  $\ln$  indicates natural logarithm.

The four parameters of the characteristic function that specify particular members of the Lévy-stable family of distributions are  $\alpha$  (the characteristic exponent, discussed in chapter 4),  $\beta$  (skewness:  $\beta = 0$  indicates a symmetrical distribution),  $\gamma$  (a “scale parameter”), and  $\delta$  (a “location parameter,” which is equal to the expected value if the latter exists).

For example, the values of these parameters for a normal distribution with mean  $\mu$  and standard deviation  $\sigma$  are  $\alpha = 2$ ,  $\beta = 0$ ,  $\gamma = \sigma^2/2$ , and  $\delta = \mu$  (Fama 1963, p. 422).



## ***Appendix C***

### ***Sprenkle's and Kassouf's Equations for Warrant Prices***

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#### ***Sprenkle***

Let  $x^*$  be the price of a stock on the expiration date of a warrant. A warrant is a form of call option: it gives the right to purchase the underlying stock at price  $c$ . At expiration, the payoff of a warrant will be zero if  $x^*$  is below  $c$ , since exercising the warrant would be more expensive than simply buying the stock on the market. If  $x^*$  is higher than  $c$ , the warrant permits purchase of a stock worth  $x^*$  for  $c$ , so its payoff is  $x^* - c$ . In other words, the warrant's payoff is 0 if  $x^* < c$  and is  $x^* - c$  if  $x^* \geq c$ . Of course, the stock price  $x^*$  is not known in advance, so to calculate the expected value of the warrant's payoff Sprenkle had to "weight" these outcomes by  $f(x^*)$ , the probability distribution of  $x^*$ . By the standard integral formula for the expected value of a continuous random variable, the expected value of the warrant's payoff is

$$\int_c^{\infty} (x^* - c) f(x^*) dx^*.$$

To evaluate this integral, Sprenkle assumed that  $f(x^*)$  was log-normal and that the value of  $x^*$  expected by an investor was the current stock price  $x$  multiplied by a constant  $k$ . After a little manipulation (see Sprenkle 1961, pp. 197–198), the above integral expression for the expected value of the warrant's payoff became

$$kxN\left(\frac{\ln(kx/c) + s^2/2}{s}\right) - cN\left(\frac{\ln(kx/c) - s^2/2}{s}\right), \quad (1)$$

where  $s^2$  is the investor's estimate of the variance of the distribution of  $\ln x^*$ , and  $N$  is the (cumulative) distribution function for a Gaussian or normal distribution, the values of which can be found in standard statistical tables.

Sprenkle argued that a risk-seeking investor would pay a positive price for leverage, and a risk-averse investor a negative one: that is, a leveraged asset

such as a warrant or option would have to offer an expected rate of return sufficiently higher than an unleveraged one before a risk-averse investor would buy it.  $V$ , the value of a warrant to an investor, was then given, Sprenkle showed, by

$$V = kxN\left(\frac{\ln(kx/c) + s^2/2}{s}\right) - (1 - P_e)cN\left(\frac{\ln(kx/c) - s^2/2}{s}\right), \quad (2)$$

where  $P_e$  is Sprenkle's "price" of leverage. (The right-hand side of this equation reduces to (1) in the case of a risk neutral investor for whom  $P_e = 0$  and for whom the value of a warrant is just the expected value of its payoff.) The values of  $k$ ,  $s$ , and  $P_e$  were posited by Sprenkle (1961) as specific to each investor, representing the investor's subjective expectations and attitude to risk.

### ***Kassouf***

Kassouf's first model of the relationship between warrant ( $w$ ) and stock ( $x$ ) prices was the curve

$$w = \sqrt{c^2 + x^2} - c, \quad (3)$$

where  $c$  is again the strike or exercise price (Kassouf 1962, p. 26). The more complex formula he developed for his Ph.D. thesis was

$$w/c = \left[(x/c)^z + 1\right]^{1/z} - 1, \quad (4)$$

where  $z$  was an empirically determined function of the stock price, strike price, stock-price "trend," time to expiration, stock dividend, and the extent of the dilution of existing shares that would occur if all warrants were exercised (Kassouf 1965, pp. 41–42, 52, 55). (Stock-price "trend" was measured by "the ratio of the present price to the average of the year's high and low" (*ibid.*, p. 50).)

## ***Appendix D***

### ***The Black-Scholes Equation for a European Option on a Non-Dividend-Bearing Stock***

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As I noted in chapter 5, the initial steps in Fischer Black's original approach to analysis of the dependence of the price  $w$  of a warrant on the price  $x$  of the underlying stock were modeled closely on his earlier work with Jack Treynor.<sup>1</sup> If  $\Delta w$  is the change in the value of the warrant in time interval  $(t, t + \Delta t)$ ,

$$\Delta w = w(x + \Delta x, t + \Delta t) - w(x, t),$$

where  $\Delta x$  is the change in stock price over the interval. As he and Treynor had done, Black expanded this expression in a Taylor series and took expected values:

$$E(\Delta w) = \frac{\partial w}{\partial x} E(\Delta x) + \frac{\partial w}{\partial t} \Delta t + \frac{1}{2} \frac{\partial^2 w}{\partial x^2} E(\Delta x^2) + \frac{\partial^2 w}{\partial x \partial t} \Delta t E(\Delta x) + \frac{1}{2} \frac{\partial^2 w}{\partial t^2} \Delta t^2,$$

where  $E$  designates expected value and higher-order terms are dropped.

Black then assumed that the Capital Asset Pricing Model applied both to the stock and warrant, so that  $E(\Delta x)$  and  $E(\Delta w)$  would depend on, respectively, the beta of the stock and the beta of the warrant. He also assumed that the stock price followed a log-normal random walk and that he could eliminate those terms that are "second order" in  $\Delta t$ . In addition, Black seems to have simplified the problem by assuming that, as he and Scholes put it (1973, p. 640): "The stock pays no dividends or other distributions."

Black then proceeded as he and Treynor had (the details are in Black and Scholes 1970b, pp. 11–12). A little manipulation, and letting  $\Delta t$  tend to zero, yielded the differential equation

$$\frac{\partial w}{\partial t} = rw - rx \frac{\partial w}{\partial x} - \frac{1}{2} \sigma^2 x^2 \frac{\partial^2 w}{\partial x^2}, \quad (1)$$

where  $r$  is the riskless rate of interest and  $\sigma$  is the volatility of the stock price.



Black's work with Scholes led to the same equation by the different route described in chapter 5; Merton's analysis, also described there, formed a third route to it. Note that the details of the warrant (or, more generally, the option) involved have not so far been specified. They enter in the form of what mathematicians call a boundary condition. If, for example, the option is a European call, it can be exercised only at its expiration, and gives the right to buy the underlying stock at price  $c$ . As noted in appendix C, at expiration (in other words at time  $t^*$ ), a call option's value is thus zero if  $x^*$ , the stock price at time  $t^*$ , is less than  $c$ , and  $x^* - c$  if  $x^*$  is greater than or equal to  $c$ .

This known set of values for  $w$  at time  $t^*$  forms a boundary condition, and equation (1) can then be solved to yield the following expression for the value of a European call option:

$$w = xN\left(\frac{\ln(x/c) + (r + \frac{1}{2}\sigma^2)(t^* - t)}{\sigma\sqrt{t^* - t}}\right) - c\{\exp[r(t - t^*)]\}N\left(\frac{\ln(x/c) + (r - \frac{1}{2}\sigma^2)(t^* - t)}{\sigma\sqrt{t^* - t}}\right), \quad (2)$$

where  $N$  is the (cumulative) distribution function of a normal or Gaussian distribution. As noted in chapter 5, however, Black and Scholes were unable directly to solve equation (1), and reached equation (2) by "tinkering" with Sprenkle's solution.

## ***Appendix E***

### ***Pricing Options in a Binomial World***

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A simple numerical example (based on Sharpe 1978, pp. 366–373) captures important features of option pricing in a model that is extremely straightforward but nevertheless possesses essential aspects of more complex models. The setting in the model is “binomial”: in any given time period, the price of a stock can only do two things, rise by a set amount or fall by a set amount. (To make the model more realistic, we would have to examine the path of a stock price over many such periods, as in the Cox-Ross-Rubinstein model discussed below, but the main points emerge when considering a single period.) In the model it is assumed that investors can both lend and borrow at the riskless rate of interest—of, let us say, 5 percent a year—that the stock pays no dividends and can be sold short without penalty, and that buying or selling stock or options incurs no transaction costs.

Consider a stock, currently priced at \$100, that in a year will either double or halve in value, in other words will be priced at either \$200 or \$50. How should one price a European call option, exercisable in a year, with a strike price of \$150 (that is, a contract that gives its holder the right but does not oblige the holder to purchase the stock in a year’s time at a price of \$150)? Suppose you decide to write and sell these call options. If the stock price falls to \$50, an option that gives the right to buy the stock for \$150 will expire worthless, but if the stock’s price goes up to \$200 the call options will be exercised and you will lose  $\$200 - \$150 = \$50$  on each option. You can, however, hedge this risk entirely by buying one unit of the stock for each three options you sell:

- (1) If over the year the stock falls to \$50, a portfolio of one unit of stock and the sale of three options will then be worth \$50, since the options won’t be exercised.
- (2) If the stock rises to \$200, you lose  $3 \times \$50 = \$150$  when the three options are exercised, so a portfolio of a unit of stock and the sale of three options is worth  $\$200 - \$150 = \$50$ .

The portfolio of a unit of stock and sale of three calls is therefore riskless: its value at the end of the year is \$50, whatever happens. Over the year, therefore, it must earn the riskless rate of interest of 5 percent, for there will otherwise be an arbitrage opportunity. This enables us to work out what the price,  $w$ , of each call option must be. The cost of the portfolio of a unit of the stock and the sale of three options at the start of the year must be  $\$50 \times (1/1.05) = \$47.62$ . So  $\$100 - 3w = \$47.62$  and  $w = \$17.46$ .

If the price of a call option is not equal to  $w$ , arbitrage profits are possible:

(i) Suppose the option price is greater than  $w$ —for example, suppose it is \$18.00. To buy one unit of the stock costs \$100, and the sale of three options yields  $3 \times \$18 = \$54$ . Borrow the difference of \$46. At the end of the year, this portfolio will be worth \$50 whatever happens. Repayment of the loan requires only  $\$46 \times 1.05 = \$48.30$ . With no outlay and no risk, you have made a profit of \$1.70.

(ii) Suppose the option price is less than  $w$ —for example, suppose it is \$17.00. Borrow one unit of the stock, sell it for \$100, buy three calls, and lend the remaining \$49 (for example, by buying a government bond that matures in a year, earning 5 percent interest). At the end of the year, that \$49 will have become  $\$49 \times 1.05 = \$51.45$ . If the price of the stock has fallen to \$50, you can buy it and return it, and make a profit of \$1.45, even though the options you bought have expired worthless. If the stock has risen to \$200, exercising each call option and immediately selling the stock thus acquired earns you  $3 \times \$50 = \$150$ . So you have  $\$51.45 + \$150 = \$201.45$ . The unit of stock you have borrowed can be purchased and returned at a cost of \$200, so again you have made a profit of \$1.45.

In the model, we are of course making “standard” option-theory assumptions about transaction costs, short sales, and so on. Note, however, that we do not need to assume anything about the probabilities of a rise in stock price to \$200 or fall to \$50. The arbitrage-determined price of the call option is not affected by these probabilities and thus is not influenced by the expected return on the stock. Nor do we need to assume anything about investors’ attitudes toward risk, because arbitrage operations (i) and (ii) above, which exploit deviations of the option price from  $w$ , are free of risk: they yield a profit that is certain (in the world assumed in the model).

### ***Risk-Neutral Valuation***

Thus far, the argument used to determine the option price is simply the binomial analogue of Black-Scholes-Merton option pricing. Risk-neutral valuation

as put forward by Cox and Ross involves assuming (a) that all investors are risk neutral, and so the value to them of risky investments is the present value of the expected payoff of these investments; and (b) that “the expected returns on both stock and option must equal the riskless rate” of interest (Cox and Ross 1976, p. 153). Cox and Ross’s claim was that the price of an option as worked out under these assumptions must be its arbitrage-determined price in our original world (in which we assumed nothing about expected returns or attitudes to risk).

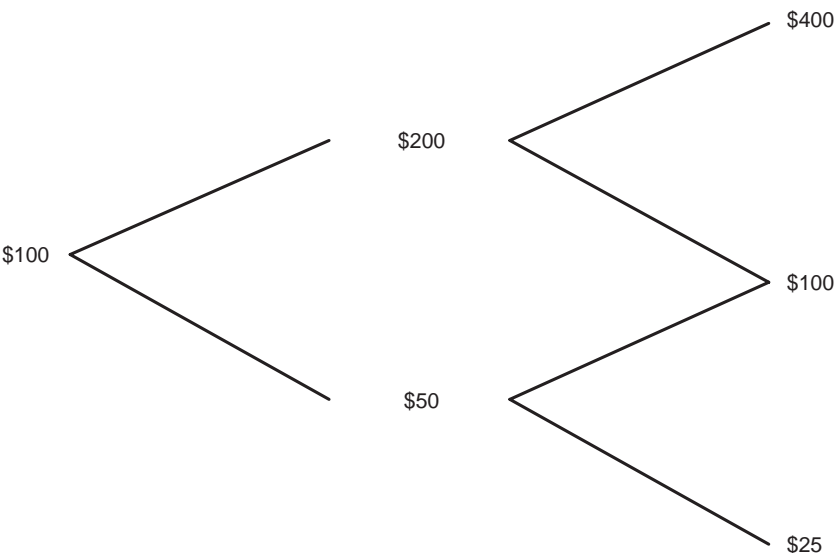
To see the equivalence of the two ways of determining option price in the example discussed here, we first work out the probabilities of a rise to \$200 and fall to \$50 that would make the expected return on the stock equal to the riskless rate of interest of 5 percent (these probabilities are what are called the “martingale probabilities”). Let the probability of a rise to \$200 be  $p$ ; the probability of a fall to \$50 is thus  $1 - p$ . The initial stock price is \$100, so if the stock’s expected return is 5 percent, the expected value of its price after a year must be \$105. Hence,  $200p + 50(1 - p) = 105$  and  $p = 55/150$ .

If the stock’s price rises to \$200, the call option is worth \$50; if the price falls to \$50, the option is worthless. So the expected value of the option’s payoff is  $50 \times p = 50 \times (55/150) = \$18.33$ . The option’s present value to a risk-neutral investor is thus \$18.33 discounted at the riskless interest rate of 5 percent:  $\$18.33/1.05 = \$17.46$ , which is equal to the price as determined by arbitrage.

### ***The Cox-Ross-Rubinstein Model***

The Cox-Ross-Rubinstein model extends the single-period binomial model over many periods. One could, for example, imagine extending the single-period model discussed here to a second period in which the stock again either halves or doubles from whatever price it has reached at the end of the first period (see figure E.1), and adding further time periods creates a ramifying “tree” of possible outcomes. The principle of risk-neutral valuation still applies: for example, “the value of a call should be the expectation, in a risk-neutral world, of the discounted value of the payoff it will receive” (Cox, Ross, and Rubinstein 1979, p. 239).

The “tree” can be elaborated to take into account dividends, and “American” as well as “European” options can be treated (the former can be exercised at any time up to their expiration, raising the need to examine when early exercise may be rational). Such problems can become intractable analytically, but the Cox-Ross-Rubinstein model allows them to be solved numerically and recursively by working “backwards” through the tree.



**Figure E.1**  
An example of a two-period binomial process.

The Black-Scholes-Merton model is a limit case of the Cox-Ross-Rubinstein model. If the time periods become infinitely many and infinitesimally small, a “multiplicative” binomial process like that in figure E.1 becomes the Black-Scholes-Merton log-normal random walk in continuous time, but other stochastic processes can also be modeled using the Cox-Ross-Rubinstein approach.

## ***Appendix F***

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### ***Repo, Haircuts, and Reverse Repo***

Suppose party A wishes to buy a security such as a bond using borrowed money. It approaches party B with a request to “repo” the bond. If the repo is agreed to, party B lends party A an amount of money close to the value of the bond. Party A buys the bond on the market and transfers it to the custody of party B for safekeeping. The two parties (A and B) also agree that on a specified future date party A will repay the amount of money lent by party B, plus interest, and party B will return the bond to A. The bond is thus serving as collateral for the loan from B to A. The interest rate that A pays B is called the “repo rate.”

The reason that this arrangement is called a “repurchase” (repo) agreement is that it can often be executed in another form, also known as a “buy-sell.” In this equivalent form of the transaction, party A “sells” the bond to B, and B simultaneously agrees to “sell it back” to A (that is, A agrees to *repurchase* it) at a future date. The difference between the future price and the initial price represents the “repo” interest charged by B for in effect lending the money to A.

Party B is engaged in earning interest from a form of lending in which risks are limited. The chief risk to B is if A defaults in a situation in which the price of the bond has fallen, but B can mitigate this risk by imposing a “haircut,” in other words lending less than the market price of the bond. In the markets within which Salomon and LTCM operated, haircuts of 2 percent were standard for long duration U. S. Treasury bonds: in other words, B would lend A only 98 percent of the bond’s market price. Haircuts, however, vary with market conditions, with the type of and maturity date of the security, and with B’s assessment of the likelihood of A defaulting.

By reversing the roles of the two parties, a repo can also be used to establish a “short” position in a bond (one that benefits from falls in price). Market participants refer to this as a “reverse repo” or simply a “reverse.” Suppose A wants to establish a short position in a bond, and B owns the bond in

question. Party A finds a third party to which to sell the bond, and uses the money thus received as collateral to “borrow” the bond from B in order to deliver it to the third party. A and B also agree that A will return an equivalent bond to B (or sell the bond back in the buy-sell form of the transaction) on a specified future date. A fall in the market price of the bond will benefit A, so A has established the requisite short position, while B can earn interest on the money it holds for the duration of the transaction.

The above account is schematic and glosses over complexities such as the treatment of coupons paid during the transaction and the exact legal status of a bond held by B as repo collateral if A becomes bankrupt. That legal status was clarified only in the 1980s. Previously, the conduct of repo had been remarkably informal: there was no clearly established legal definition of “repo.” Sometimes “two parties will do a repo but sign *no* agreement; they may be just sloppy, but more likely, party A’s lawyer doesn’t like party B’s agreement and vice versa” (Stigum 1989, p. 5, emphasis in original).

Part of the reason that transactions involving large sums of money could be entered into without a clear legal basis was that traditional participants in the repo market were well-established institutions which were unlikely to default, while newcomers’ financial strength would be scrutinized carefully. Another part of the reason is explained as follows by Marcia Stigum, whose guide to the repo market is the main source for this appendix: “The lack of concern that money market people displayed about dotting [*sic*] the legal i’s and t’s with respect to repo reflected . . . their notion that they were afforded much protection by the standards of the business in which they operated. Money market folk have always felt, at least until recently, that they are in a business in which people say ‘My word is my bond’ and mean it; a business in which, if someone says something is ‘done,’ it is done.” (Stigum 1989, p. 218)

It should be noted that “repo” has uses quite other than those discussed above. It is also common, for example, to repo securities one already owns in order to raise money, and repo is in addition a main tool of the open-market operations of the Federal Reserve and other central banks.

## ***Appendix G***

### ***A Typical Swap-Spread Arbitrage Trade***

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On February 8, 1997, twenty-year U.S. dollar interest-rate swaps could be entered into at a fixed interest rate of 6.94 percent. (This example and the data in this appendix are taken from Perold 1999, pp. A4–A6, supplemented by an interviewee’s estimate of risk-capital requirements for the trade in question.) In other words, one party would undertake to pay a fixed interest rate of 6.94 percent per annum for 20 years, while receiving a floating rate of interest, dollar Libor, from the other party. (Libor is the London interbank offered rate, the average rate of interest at which a panel of major banks report other banks as being prepared to lend them funds in a particular currency, in this case dollars, for a particular period, in this case six months.) On February 8, the yield of U.S. government bonds with maturities similar to that of the swaps was 6.77 percent.

The “swap spread”—the difference between the fixed interest rate at which swaps can be entered into (in this case, 6.94 percent) and the yield of government bonds of equivalent maturity (6.77 percent)—was thus 17 basis points (a “basis point” is a hundredth of a percentage point). The interest rate at which repo can be used to borrow money to buy bonds is called “repo rate.” (See appendix F.) Because the bonds are held by the creditor as collateral, and because a “haircut” protects against the consequences of default, repo rate is typically lower than Libor (by 20 basis points at the time we are discussing).

A tiny positive cash flow could thus be obtained by buying bonds yielding 6.77 percent, funding the purchase at repo rate, and paying fixed interest in a swap while receiving Libor in return. The incoming cash flow would be in effect 6.77 percent + Libor, while the outgoing flow would be repo rate + 6.94 percent. Since Libor was 20 basis points above repo rate, the net annual cash flow was thus three basis points.

Clearly, a profit of 0.03 percent per annum is not enticing! The use of repo meant that LTCM could construct the position by borrowing all but a 1 percent haircut on the repo transaction, so the return on the capital devoted



to the haircut was 3 percent, but even that was clearly not in itself attractive, particularly when prudent management of the trade required LTCM to set aside risk capital equivalent to about 1 or 1.5 times the haircut to protect against market fluctuations. (No precise estimate can be given for risk capital because it was calculated as an increment to LTCM's overall risk-capital requirement, which was in its turn determined by the partnership's risk analyses.)

Crucially, however, a swap spread of 17 basis points was unusually low: between May 1994 and February 1997 the 20-year U.S. dollar swap spread had fluctuated between 17 and 33 basis points (Perold 1999, p. A20). If the spread widened again, LTCM's bond position would be worth more than its swap position, and it could profit—perhaps substantially—by selling the bonds and entering into a swap contract “canceling out” the original.

LTCM would not enter into a trade such as a swap-spread arbitrage without an understanding of why an apparently attractive opportunity had opened up. In early 1997, LTCM believed that there were identifiable, temporary reasons why the swap spread had become unusually narrow. (See Perold 1999, p. A5.) Accordingly, it gradually built its position, using repo to buy bonds (and also buying bond futures), while entering into swap contracts in which it paid fixed interest and received dollar Libor. By July 1997, the predicted increase in the swap spread had indeed taken place, and the fund was able over the summer to liquidate its position, with a total net gain of about \$35 million.

## ***Appendix H***

### ***List of Interviewees***

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*Not all the interviewees were willing to be identified, so this list excludes those who preferred anonymity, testimony from whom appears in the text without attribution. Transcripts of the interviews (or, in the case of a small number of interviewees who did not wish to be tape-recorded, notes on the interview) are in my possession. Undertakings of confidentiality given to the interviewees prevent me making these transcripts and notes available to other researchers. Quotations from named interviewees were shown to them in advance of publication, and in a limited number of cases the quotations that appear here incorporate minor amendments that they requested. —DM*

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Interviewee	Date	Location of interview
Thomas A. Bond	November 9, 2000	Chicago
Richard Brealey	December 2, 1999	London
Michael Carusillo and Clayton Struve	November 7 and 8, 2000	Chicago
Geoffrey Chamberlain	June 22, 2001	London
John Cox	September 25, 2001	Cambridge, Massachusetts
David T. DeArmey	November 8, 1999	Chicago
Emanuel Derman	November 14, 2000	New York
Joseph Doherty	December 4, 2000	London
Eugene Fama	November 5, 1999	Chicago
Doyne Farmer	September 28, 2001	Santa Fe
William L. Fouse	June 14, 2000	San Francisco
Milton Friedman	October 9, 2001	San Francisco
Mathew L. Gladstein	November 15, 1999	New York

Interviewee	Date	Location of interview
Victor Haghani, Gérard Gennotte, Fabio Bassi, and Gustavo Lao	February 11, 2000	London
J. Michael Harrison	October 8, 2001	Stanford
John C. Hiatt	November 7, 2000	Chicago
Timothy F. Hinkes	November 8, 1999 and November 6, 2000.	Chicago
M. Blair Hull	November 10, 2000	Chicago
Sir Michael Jenkins	March 16, 2000	London
Costas Kaplanis	February 11, 2000	London
Sheen Kassouf	October 3, 2001	Newport Beach, California
Tim Kempster	June 23, 1999	Edinburgh
Thomas P. Knorring	November 10, 2000	Chicago
Mark Kritzman	November 11, 1999	Cambridge, Massachusetts
Jan Kwiatkowski	December 2, 1999	London
Andreas Kyprianou	July 21, 1999	Edinburgh
Per E. Larsson	November 25, 2002	Stockholm
Richard F. Leahy	October 31, 2000	Greenwich, Connecticut
Dean LeBaron	November 3, 1999	by videophone
Hayne E. Leland	June 12, 2000	Berkeley
Barry Lind	November 9, 2000	Chicago
James J. McNulty	November 6, 2000	Chicago
John McQuown	October 9, 2001	San Francisco
Benoit B. Mandelbrot	May 25, 2002	Djursholm, Sweden
Harry M. Markowitz	September 17, 2002	San Diego
Leo Melamed	November 8, 2000	Chicago
John W. Meriwether	November 14, 2000	Greenwich, Connecticut
Robert C. Merton	November 2, 1999	Cambridge, Massachusetts
Merton Miller	November 5, 1999	Chicago
Franco Modigliani	September 25, 2001	Cambridge, Massachusetts

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Interviewee	Date	Location of interview
John O'Brien	October 3, 2001	Newport Beach, California
Norman Packard	September 27, 2001	Santa Fe
William R. Power	November 10, 2000	Chicago
Burton R. Rissman	November 9, 1999	Chicago
Richard Roll	November 22, 2004	Westwood, California
Eric Rosenfeld	October 30, 2000	Rye, New York
Stephen A. Ross	September 24, 2001	Cambridge, Massachusetts
Mark Rubinstein	June 12, 2000	Berkeley
Paul A. Samuelson	November 3, 1999	Cambridge, Massachusetts
Myron S. Scholes	June 15, 2000	San Francisco
William F. Sharpe	October 8, 2001	Stanford
David E. Shaw	November 13, 2000	New York
Rex A. Sinquefeld	November 23, 2004	Santa Monica
Case M. Sprenkle	October 16, 2002	by telephone
David Steen	June 21, 2001	Sevenoaks, Kent
Paul G. Stevens Jr.	November 8, 1999	Chicago
Joseph W. Sullivan	October 24, 2000	Knoxville
Nassim N. Taleb	November 14, 1999	New York
Edward O. Thorp	October 1, 2001	Newport Beach, California
Jack Treynor	October 3, 2001	Palos Verdes Estates, California
Oldrich Vasicek	October 5, 2001	San Francisco
David Weinberger	November 9, 2000	Chicago
David Wenman	June 22 and 28, 2001	London, and by telephone

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## Glossary

*This glossary contains the main financial-market and finance-theory terms to be found in this book, and a number of the more general technical terms used. I have not attempted precise definitions, and several of the terms have multiple meanings; I have given only the meaning employed in this book. For broader definitions, see Moles and Terry 1999, which is by some distance the best of the various dictionaries of finance and which is the source I have consulted and drawn on most often.—DM*

**American option** An *option* exercisable at any point until its expiration. See *European option*.

**arbitrage** In finance theory, arbitrage is trading that generates riskless profit with no capital outlay. Market practitioners use the term more broadly to refer to trading that seeks to make low-risk profits from price discrepancies, for example between the prices of similar assets.

**basis point** A hundredth of a percentage point.

**bear** A market in which prices fall, or an investor who expects them to fall. See *bull*.

**beta** A coefficient indicating the extent to which the returns on a stock or other security are sensitive to how the market as a whole fluctuates. See *Capital Asset Pricing Model*.

**bill** A *bond* for which the time between issuance of the bond and repayment of the principal is short.

**Black-Scholes** or **Black-Scholes-Merton** The canonical *option*-pricing model, based on the assumption that the underlying stock price follows a *log-normal random walk*.

**bond** A tradable form of debt. Bonds normally commit the issuer (most commonly a government or a corporation) to repay a fixed sum (the principal) on a given date and to make periodic interest payments (coupons) of fixed amounts until that date. The owner of a bond can sell it to another investor.

**broker** A market participant who executes customer orders. See *market maker*.

**Brownian motion** The random movement of tiny particles, for example of dust or pollen, that results from collisions with the molecules of the gas or liquid in which they are suspended.

**bull** A market in which prices rise, or an investor who expects them to rise. See *bear*.

**call** An *option* to buy.

**Capital Asset Pricing Model** The theory that in equilibrium (a) all investors in risky capital assets such as stock hold every such asset in proportion to its market value—in other words, hold the market portfolio—and (b) the expected return on a capital asset depends in a straight-line fashion on its beta, which indicates the extent to which returns on the asset are sensitive to fluctuations in the market as a whole (that is, to fluctuations in returns on the market portfolio).

**clearing** The process of registering traded contracts (such as *futures*), of setting, receiving, and adjusting *margin* deposits, and of settling contracts.

**clearinghouse** In a derivatives exchange, the organization that acts as the intermediary between purchasers and sellers, becomes the *counterparty* to all contracts, and conducts the process of *clearing*.

**common stock** The standard category of stock. (See, for example, *preferred stock*.)

**convertible bond** A *bond* that contains an option to convert the bond into a security of another type, such as the stock of the corporation issuing the bond.

**counterparty** The other party to a contract. From the point of view of the purchaser of a call *option*, for example, the counterparty is the individual or institution that must sell the underlying asset if the purchaser exercises the option.

**coupon** An interest payment by a *bond*'s issuer to its holder.

**Cox-Ross-Rubinstein** An *option*-pricing model similar to *Black-Scholes*, but based on a discrete-time rather than continuous *random walk*.

**delta** The extent to which the value of an *option* is sensitive to changes in the price of the underlying asset (that is, the rate at which option value changes as the asset price changes).

**derivative** A contract or a security (such as a *forward*, *future*, *option*, or *swap*) whose value depends on the price of another “underlying” asset, or on the level of an index or interest rate.

**discount** To calculate the amount by which future payments must be reduced to give their present value.

**dividend** A periodic distribution of money by a corporation to the holders of its *stock*.

**efficient-market hypothesis** The conjecture that prices in capital markets incorporate—effectively instantaneously—all available price-relevant information.

**European option** An *option* exercisable only at its expiration. See *American option*.

**exercise price** See *option*.

**expected value** If a quantity varies randomly, its expected value is its value averaged across possible outcomes, when those outcomes are “weighted” by their probabilities. For example, if a fair coin is tossed, and a player wins \$1 if it lands heads and loses \$1 if it lands tails, then the expected value of the player’s net gain is zero. The player has a 50% chance of winning \$1 and a 50% chance of losing \$1, so the expected value of the player’s net gain is  $0.5 \times \$1 + 0.5 \times (-\$1) = 0$ .

**expiration** The point in time when a derivatives contract such as an *option* ceases to be valid.

**forward** A contract in which one party undertakes to buy, and the other party to sell, a set quantity of an asset of a particular type at a set price at a given future time. If the contract is standardized and traded on an organized exchange, it is referred to as a *future*.

**front-run** To buy assets in advance of known purchases, or to sell them or take a *short position* in them in advance of known sales. The term is generally used for cases in which the knowledge of coming purchases/sales is privileged, as when a *broker* uses knowledge of sales or purchases he or she is about to make for a customer to make advantageous personal trades.

**future** A standardized contract traded on an organized exchange in which one party undertakes to buy, and the other to sell, a set quantity of an asset of a particular type at a set price at a given point in time in the future. The term is also used for contracts that are equivalent economically to such future purchases/sales but are settled by cash payments.

**haircut** The difference between the amount in effect lent in a *repo* or similar agreement and the market price of the securities pledged as “collateral” for the “loan.”

**hedge** To eliminate or minimize a risk by entering into transactions that offset it.

**hedge fund** A special category of investment vehicle, often registered offshore and/or falling within the “private funds” exemption from the U.S. Investment Company Act of 1940, which typically permits only very large investments and/or a strictly limited number of investors, is exempt from many regulatory requirements, and is free to adopt strategies (such as *short selling* and using borrowed funds to enhance returns) that many other categories of investor are prohibited from using.

**illiquid** In an illiquid market, assets can be bought and/or sold only with difficulty or with large effects on prices. See *liquid*.

**implied volatility** The *volatility* of a stock or index consistent with the price of *options* on the stock or index, as indicated by an option-pricing model.

**leverage** The extent to which an investment offers prospects for gains and losses that are large relative to the size of investment; the extent to which a firm or transaction is financed by taking on debt.



**Libor** The London interbank offered rate: the average rate of interest at which a panel of major banks report other banks as being prepared to lend them funds in a particular currency for a particular period. Other interest rates are often set with Libor as the benchmark.

**liquid** In a liquid market, assets can readily be bought and sold at or close to the current market price. See *illiquid*.

**log-normal** A variable is log-normally distributed if the values of its natural logarithm spread statistically according to the standard “bell-shaped” curve (the so-called “normal distribution”) of statistical theory.

**long position** A portfolio of an asset and/or *derivative* of that asset that will rise in value if the price of the asset rises. See *short position*.

**margin** The deposit, normally adjusted daily, that a *broker* or a *clearinghouse* requires from those who have bought or sold contracts (such as *futures*) traded on an exchange.

**margin call** An instruction from a *broker* or a *clearinghouse* to deposit additional *margin*.

**market maker** A market participant who is expected always to quote prices at which he or she will buy and sell the asset being traded. Some exchanges (such as the Chicago Board Options Exchange) distinguish strictly between a market maker trading on his or her firm’s account and a broker.

**mark to market** To revalue a trading position as market prices fluctuate, for example in order to calculate *margin* requirements.

**martingale** A random process in which future *expected values* of the variable in question, given the information known at the present, are the variable’s current value. An example of a martingale is the amounts a player holds in a game in which a fair coin is tossed repeatedly, the player wins \$1 if it lands heads, and loses \$1 if it lands tails. At any point in the game, the expected value of what the player will hold at any point in the future is what the player currently holds.

**maturity** The date at which a *bond*’s principal must be repaid, or the date at which a *derivative* contract expires.

**open outcry** A system of face-to-face trading within a fixed arena such as a *pit*, in which both buyers and sellers shout or hand-signal the prices (and quantities) at which they will buy or sell.

**option** A contract the purchaser of which gains the right, but is not obliged, to buy (call) or to sell (put) an asset at a given price (the strike price or exercise price) on, or up to, a given future date (the expiration). The seller (or writer) of the option is obliged to fulfill his or her part of the option contract if so demanded.

**pit** The physical location (normally shaped as an amphitheater with stepped sides) of *open-outcry* trading.

**preferred stock** *Stock* whose holder must be paid a set *dividend* before any other distribution of dividends can be made, and whose claim on the corporation's assets has priority over those of other stockholders if the corporation is liquidated.

**put** An *option* to sell.

**random walk** A sequence of movements, for example of a price or of a physical particle, that is governed by chance—in particular, one in which the probabilities of the price or particle moving “up” or “down” are independent of the price's or particle's previous movements.

**repo** A repurchase agreement: a contract in which (in effect) party A borrows money from party B to buy securities which B holds as collateral for the loan.

**return** The return on a *stock* or another security over a given period is the sum of its change of price over the period and any *dividends*, *coupons* or other payments received during the period. The return is normally expressed as a proportion or percentage of the security's price at the start of the period.

**riskless rate** The rate of interest paid by a borrower who creditors are certain will not default, often taken as indicated by the *yield* of a *bond* issued in its own currency by a major government.

**security** A tradable financial instrument or asset, such as a *stock*, a *bond*, a *future*, or an *option*.

**share** See *stock*.

**short position** A portfolio of an asset and/or *derivative* of that asset that will rise in value if the price of the asset falls. A short position can, for example, be constructed by *short selling* an asset. See *long position*.

**short selling** A process in which a trader sells a security he or she does not yet own, or owns only temporarily. Short selling is often accomplished by finding an owner of the security who is prepared, for what is in effect a fee, to “lend” it to the trader: in other words to transfer ownership of it to the trader, who in turn undertakes to replace it. The trader who short sells may, for example, expect that the price of the security will have fallen by the time he or she has to replace it, so the trader can keep the difference in price (minus the fee).

**skew** A pattern of *option* prices, sometimes also called a “smile,” in which *implied volatility* is not independent of strike price (as it should be on the *Black-Scholes* model).

**smile** See *skew*.

**specialist** On the New York Stock Exchange and on other U.S. stock exchanges, an exchange member who maintains the “book” of buy and sell orders for the stocks for whom he or she is responsible, matches and executes such orders, and is expected to trade with his or her firm's own capital if there is an imbalance.

**spot trading** Trading of contracts for immediate or near-immediate delivery.

**spread** In this book, the difference in price of, or rate of return on, two similar assets. (The term has a particularly wide range of additional meanings in financial markets.)

**standard deviation** A statistical measure of the extent to which a set of numbers is spread out around their average.

**stock (U.S.) or share (U.K. and U.S.)** A security that confers part-ownership of a corporation.

**strike price** See *option*.

**swap** A contract to exchange two income streams, for example fixed-rate and floating-rate interest on the same notional principal sum.

**swap spread** The difference between the fixed interest rate at which interest-rate *swaps* can be entered into and the *yield* of a government bond of equivalent maturity denominated in the same currency.

**value-at-risk** A method of estimating the exposure of a portfolio of assets to potential losses. A typical VAR calculation might estimate the level of loss over a given time period that is expected to be exceeded only 1% of the time.

**variance** A statistical measure of the extent to which a set of numbers is spread out around their average. The variance is the square of the *standard deviation*.

**volatility** The extent of the fluctuations of the price of an asset, conventionally measured by the annualized *standard deviation* of continuously compounded returns on the asset.

**warrant** A call *option* issued by a corporation on its own stock. Its exercise typically leads to the creation of new stock rather than the transfer of ownership of existing stock.

**yield** The yield of a *bond* is the rate of return it offers over its remaining lifetime at its current market price, normally measured by finding the rate of interest at which a bond's *coupons* and principal have to be *discounted* so that their total present value is the bond's current price.

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# Notes

## Chapter 1

1. Leo Melamed, email message to D. MacKenzie, July 22, 2004; Melamed and Tamarkin 1996, pp. 358–359; Woodward 2001, p. 37.
2. Melamed and Tamarkin 1996; Melamed interview.
3. Robert K. Merton (1910–2003), a dominant figure in twentieth-century American sociology, was the father of the finance theorist Robert C. Merton.
4. Lind interview; Barry Lind, private communication to D. MacKenzie, August 13, 2004.
5. Woodward 2001, pp. 38–47; Stewart and Hertzberg 1987.
6. For details, see chapters 6 and 7.
7. The figure of \$273 trillion is the sum of the total notional amounts of exchange-traded and over-the-counter derivatives contracts at the end of June 2004, as calculated by the Bank for International Settlements (2004, p. 10, table A2). The Bank's estimate of the gross market value corresponding to the \$220 trillion over-the-counter portion of this total is \$6.4 trillion: the gross market value is the sum "of the absolute values of all open contracts with either positive or negative replacement values evaluated at market prices prevailing at the reporting date" (Bank for International Settlements 2003, p. 4).
8. Gross credit exposure is the sum of the values of outstanding over-the-counter contracts whose value is positive, "after taking account of legally enforceable bilateral netting agreements" (Bank for International Settlements 2003, p. 5). No corresponding credit exposure estimate is available for exchange-traded derivatives contracts (which had a total notional amount at the end of June 2004 of nearly \$53 trillion).
9. Summers wrote: "The differences [between the disciplinary mainstream and financial economics] I am discussing may be clarified by considering a field of economics which could but does not exist: ketchup economics. There are two groups of researchers concerned with ketchup economics. Some general economists study the market for

ketchup as part of the broader economic system. The other group is comprised of [*sic*] ketchup economists located in Department of Ketchup where they receive much higher salaries than do general economists. . . . Ketchup economists have an impressive research program, focusing on the scope for excess opportunities in the ketchup market. They have shown that two quart bottles of ketchup invariably sell for twice as much as one quart bottles of ketchup except for deviations traceable to transactions costs, and that one cannot get a bargain on ketchup by buying and combining ingredients once one takes account of transactions costs. . . . Indeed, most ketchup economists regard the efficiency of the ketchup market as the best established fact in empirical economics.” (1985, pp. 633–634) As an account of what financial economists actually had done, Summers’s analogy was quite unfair, but it may have been more accurate in respect to many mainstream economists’ *view* of financial economics.

10. See Summers’s comment on the salaries of “ketchup economists,” quoted in the preceding note.

11. See, for example, Hesse 1966; Shackley and Wynne 1995, 1996; Morgan and Morrison 1999.

12. See, for example, Camic 1987; Morgan and Rutherford 1998; Yonay 1994, 1998.

13. See chapter 2 below for the meaning of “equilibrium.”

14. Arrow and Debreu 1954. See also Mirowski 2002; Weintraub 2002.

15. See, for example, Hall 1989.

16. For historical treatments of the emergence of rational-expectations economics, see Sent 1998, 1999.

17. See McCloskey 1985, pp. 87–89.

18. As Hammond (1998, p. 198) says, “typesetting the essay [Friedman 1953a] philosophically . . . proved not to be an easy task.” See, e.g., Hirsch and de Marchi 1984, 1990; Hammond 1996.

19. Friedman and Friedman 1998, p. 159. See also Stigler 1988, pp. 143–147. Hayek’s opening address to the Mont Pèlerin meeting can be found on pp. 148–159 of Hayek 1967.

20. For some insight into Friedman’s earlier methodological views, see Friedman 1941, 1946.

21. Friedman saw himself as defending the legacy of the great British economist Alfred Marshall, for example against the accounts of “imperfect” or “monopolistic” competition put forward by E. H. Chamberlin and Joan Robinson (Friedman 1953a, pp. 38–39). The sentence that I have drawn on in this book’s title invokes Marshall: “Marshall took the world as it is; he sought to construct an ‘engine’ to analyze it, not a photographic reproduction of it.” (*ibid.*, p. 35)

22. Samuelson’s *Economics* appeared in many editions and translations. The estimate of its total sales is from p. 31 of Dougherty 2002.

23. See, for example, Feyerabend 1975.

24. To point to the centrality of Friedman's essay to the epistemic culture of modern economics is not to argue that Friedman's own economic work should be seen as typical, which in many ways it was not, as David Teira Serrano noted in a personal communication to D. MacKenzie (August 30, 2004). Above all, Friedman eschewed the elaborate mathematical apparatus of economists associated with the Cowles Commission (see chapter 2) such as Kenneth Arrow and Gerard Debreu.

25. It should be stressed that the "camera" invoked here is a metaphor, not a comment on the actual process of photography, which is often not passive recording of an unchanged external world. See, for example, Dyer's (1997) elegant discussion of the process of "making white people white" for the purposes of film, or consider the question of the impact of photographic images of thin models on the propensity to diet (and thus to alter one's own body) or even to suffer eating disorders.

26. See Faulhaber and Baumol 1988; Sharpe 1990; Blinder 2000; Merton 1994; Merton and Bodie 2005.

27. Perhaps most relevant is Daniel Miller's theory of "virtualism." See Carrier and Miller 1998 (especially Miller's own essay); see also Miller's critique of Callon (D. Miller 2002). For a sharp, self-consciously tendentious comparison of Miller's and Callon's viewpoints, see Holm 2003. For Callon's response to Miller's critique, see Callon 2005. Among the other works that bear on the effect of economics on its subject matter are Breslau 2003 and Ferraro, Pfeffer, and Sutton 2005. Work on economics by the philosophers John Dupré and (especially) Nancy Cartwright is relevant to the ideas of performativity discussed below; see Dupré 2001 and Cartwright 1999. Clearly, too, Ian Hacking's philosophical work is compatible with those ideas; see Hacking 1983; 1992a,b; 1995a,b.

28. See also D. Miller 2002.

29. Another useful source, closer in time to the events described, is Emery 1896.

30. The degree of disentanglement of a futures contract from those who initially entered into it depends on the clearing mechanism in place (Millo, Muniesa, Panourgias, and Scott, forthcoming). Moser (2000) describes the evolution of clearing on the Chicago Board of Trade from "direct clearing" of bilateral contracts, via "ring clearing," to "complete clearing" in which the clearinghouse itself became the counterparty in all contracts. This last is now the standard situation in derivatives exchanges.

31. Cronon 1991, p. 131; Falloon 1998, pp. 117–120; Norris 1903.

32. See Grant 2003. Also see Zaloom's (2003, 2004) fine ethnographic studies of pit trading and risk taking at the Chicago Board of Trade. Though sensationalized and not a formal ethnography, Lynn 2004 contains insights into behavior in the pits of the Chicago Mercantile Exchange.

33. See also Austin 1962.

34. Because the term "performative" can be used in a wide range of senses—for example, to invoke not Austin but Erving Goffman's emphasis on the dramaturgical

aspects of social life (Goffman 1959)—the set of meanings proposed here should not be taken as intended to be exhaustive. For a sample of usages of “performative,” see Butler 1990, 1997; Pickering 1994, 2003; Law 2002.

35. See MacKenzie 2004. “Austinian performativity” also had the disadvantage of seeming to restrict discussion to situations in which simple performative utterances are possible. (My argument that the Black-Scholes-Merton model was performative was taken as intended to be directly analogous to the absolute monarch proclaiming Robin Hood an outlaw.) Another possible term would be “Mertonian performativity,” because Robert K. Merton emphasized the feedback between social knowledge and the referents of that knowledge much earlier than Barnes did. However, as noted in the text Merton, unlike Barnes, tended sometimes to treat those loops as pathological, and I certainly do not see them as such in the cases discussed here. Furthermore, his son, the economist Robert C. Merton, plays a central role in my story, and “Mertonian performativity” might thus be misunderstood as a reference to the economist rather than the sociologist.

36. Barnes’s work on self-validating feedback loops goes far beyond these elementary observations—for him such loops are the foundations of a theory of social order and of power (Barnes 1988)—but the simple examples convey the essential point.

37. Counterperformativity could be construed as a form of what Callon calls “overflow” (see, e.g., Callon 1998, p. 18), but it seems to me to be an important enough possibility to require special identification.

38. Callon’s emphasis on the role in *agencements* of “prostheses, tools, equipment, technical devices, algorithms, etc.” (Callon 2005, p. 4) is a strength of his analysis.

39. Some of this reluctance is based on a misinterpretation of the “symmetry” requirement of David Bloor’s (1973, 1976) “strong program” of the sociology of scientific knowledge: see MacKenzie (2002a). That, however, is a debate that need not be entered into here. It has long been acknowledged by sociologists of scientific knowledge that agnosticism or symmetry is not appropriate under some circumstances—for example, when one is dealing with questions of the nature of knowledge and comparing views within artificial intelligence and views from the sociology of knowledge. (See Collins 1990.)

40. Before the recent decimalization, U.S. securities prices were quoted in binary fractions of dollars. Because the options contracts in question referred to 100 shares, a price of, for example, “ $3\frac{1}{4}$ ” translates into a cost of \$325 per contract. A “July/40 call” gave the right to buy 100 Polaroid shares at \$40 per share at any time up to its July expiration date. In the years examined by Rubinstein, the latter was “10:59 P.M. Central Time (11:59 P.M. Eastern Time) on the Saturday immediately following the third Friday of the month.” However, “to exercise an option, a customer must instruct his broker no later than 4:30 P.M. Central Time on the business day immediately preceding the expiration date” (Cox and Rubinstein 1985, p. 26). The price of an option is of course affected by variables other than the stock price, such as time to expiration. The latter effect is, however, too slow to explain substantial price variation in brief time periods of the kind discussed by Rubinstein.

41. A “limit order” to buy an option or other security specifies a maximum purchase price, and a limit order to sell specifies a minimum sale price.
42. See, especially, Latour 1999.
43. For a discussion of replication in econometrics, see Collins 1991.
44. See Fama 1970, pp. 413–414.
45. On the need to substitute realized for expected returns, see Black, Jensen, and Scholes 1972, pp. 83–84; Fama and MacBeth 1973, pp. 611–612.
46. See Agnew 1986.
47. For a useful account of thinking about markets that highlights some of the underlying tensions, see Slater and Tonkiss 2001.
48. For the origins of the term “social studies of finance,” see Muniesa 2003, p. 35. Examples of work in the field include Beunza and Stark 2004; Brügger 2000; Hassoun 2000; Holtzer and Millo, forthcoming; Izquierdo 1998, 2001; Knorr Cetina and Bruegger 2000, 2002a,b; Knorr Cetina and Preda 2001; Lépinay 2000; Lépinay and Rousseau 2000; Millo, forthcoming a,b,c; Millo, Muniesa, Panourgias, and Scott, forthcoming; Muniesa 2000a,b, 2002, 2003; Preda 2001a,b, 2004a,b. For a collection including some articles from social studies of finance and some from more traditional economic sociology, see Knorr Cetina and Preda 2005.
49. For a good introduction to economic sociology, see Swedberg 2003.
50. An exception in regard to knowledge is Smith 1999.
51. See, among many examples, Winner 1977; Bijker, Hughes, and Pinch 1987; MacKenzie and Wajcman 1999; Oudshoorn and Pinch 2003.
52. Fischer Black was already dead before I began this study, and James Tobin died before I was able to interview him.
53. A new edition of *Capital Ideas* will be published by John Wiley in 2006.
54. See the autobiography of one such “quant,” Derman 2004.

## **Chapter 2**

1. Hunt did criticize Dewing for insufficient attention to some aspects of economics, especially theories of the business cycle (Hunt 1943, p. 309).
2. See also Kavesh, Weston, and Sauvain 1970, p. 5. These authors note that the *Journal of Finance*’s “early years were filled with descriptive articles, with a heavy ‘institutional’ flavor—largely reflecting the type of research being carried out in those years.” Occasional symposia and debates published in the *Journal of Finance* are a useful way of tracking the field’s preoccupations. See, for example, the 1950 discussion of “Materials and Methods of Teaching Business Finance” (volume 5), the 1955 discussion of “Theory



of Business Finance” (volume 10), and the 1967–68 discussion of the “State of the Finance Field” (volumes 22 and 23).

3. See, e.g., MacKenzie 2001, pp. 63–71, 90.

4. On the influence of Carnegie’s divide on the transaction-cost economics of Oliver Williamson, who was a Ph.D. student there between 1960 and 1963, see Klaes 2001. On the divide more generally and its possible connections to rational-expectations theory, see Sent 1998, pp. 8–9.

5. Friedman and Friedman 1998, pp. 158–159.

6. As noted below, for Modigliani the spark was attendance at a 1950 conference organized by the National Bureau of Economic Research, at which he heard David Durand present the paper that became Durand 1952. As discussed in the text, Durand explored, but ultimately dismissed, the idea that capital structure might be irrelevant. Modigliani felt the proposition might hold despite Durand’s belief that it did not. (Modigliani’s own contribution to the meeting—Modigliani and Zeman 1952—was further from his eventual position than Durand’s paper.) It took Modigliani several years of occasional reflection on the problem to come up with “a really convincing proof” that the proposition held (Modigliani interview). When he did, he described the proposition that capital structure was irrelevant to a class he was teaching at Carnegie that Miller was also attending. “Merton Miller said ‘I have the evidence for you.’” (Modigliani interview) Miller had set his graduate students the task of finding empirically the best capital structure from the viewpoint of minimizing a corporation’s cost of capital, and they had been unable to do so: “They couldn’t find any optimum.” (Miller interview)

7. Modigliani and Miller (1958, p. 268) defined a firm’s “average cost of capital” as the “ratio of its expected return to the [total] market value of all its securities” (bonds and stocks).

8. A broadly similar theoretical account of portfolio selection was developed independently by the British economist A.D. Roy (see Roy 1952), but Roy did not develop a fleshed-out “operations research” analysis analogous to Markowitz’s. For Roy’s work and other antecedents of Markowitz’s analysis, see Markowitz 1999 and Pradier 2000.

9. See Koopmans 1951.

10. Thus Robert G. Wiese of Scudder, Stevens & Clark wrote in 1930: “Theoretically, *the proper price of any security, whether a stock or a bond, is the sum of all future income payments discounted at the current rate of interest in order to arrive at the present value*” (Wiese 1930, p. 5). Williams quoted Wiese’s article to this effect (Williams 1938, p. 55).

11. If the interest rate is 5 percent per annum, for instance, a dollar received now can be turned into \$1.05 in a year’s time. The present value of \$21 to be received in a year’s time is thus  $\$21/1.05$  or \$20. (To check this, note that adding a year’s interest at 5 percent to \$20 produces \$21.)

12. See Wiese 1930.

13. Another source recommended to Markowitz by Ketchum was Arthur Wiesenberger's annual survey of investment companies (see, e.g., Wiesenberger 1942). The attraction of such companies, if well run, was precisely the "intelligent diversification of investment" permitted by their wide-ranging holdings of different stocks (Wiesenberger 1942, p. 5).

14. Depending on how the portfolio selection problem was formulated, either a constraint (the maximum level of risk) was a quadratic function or the function to be minimized was quadratic.

15. Mirowski (2002) highlights the significance of Rand for postwar U.S. economics.

16. It is more elegant to present Markowitz's method, as he did, in terms of covariances of return, but I use the term "correlation" because it will be more familiar to a wider audience. The correlation and the covariance of two variables are related by a simple formula. If the two variables have standard deviations  $\sigma_1$  and  $\sigma_2$ , their correlation is  $\rho_{12}$ , and their covariance is  $\sigma_{12}$ , then  $\sigma_{12} = \rho_{12}\sigma_1\sigma_2$ . See, e.g., Markowitz 1952, p. 80.

17. As Markowitz worked on the problem further he realized that the shape of the "inefficient" boundary—"the side which *maximizes* variance for given return"—was not correctly drawn in this, his first published graphical representation of the attainable set (letter to D. MacKenzie, February 10, 2004). He corrected its shape in the equivalent figure in Markowitz 1956 (p. 111). The shape of the attainable set is discussed at length in Markowitz and Todd 2000 (pp. 225–239).

18. Kavesh, Weston, and Sauvain 1970, p. 5.

19. Durand 1960, pp. 235–236. "Rational Man" and "Perfect Computing Machine" are Markowitz's phrases and capitalizations; see Markowitz 1959, p. 206.

20. See also Tobin 1995, p. 129.

21. See Keynes 1936/1964, chapter 13.

22. See Markowitz, Hausner, and Karr 1963; Markowitz 2002a.

23. See Weston 1966.

24. Sharpe 1963, p. 285. On the IBM 7090, see Bashe, Johnson, Palmer, and Pugh 1986, pp. 446–448.

25. "I was doing things mostly graphically," says Sharpe (interview).

26. Sharpe also assumed that investors could borrow or lend at the riskless rate of interest, and in the 1964 version of his model short sales were not permitted (Sharpe 1964, p. 433).

27. I am extremely grateful to John P. Shelton, who refereed Sharpe's paper for the *Journal of Finance*, for a most helpful account of practitioner responses to Sharpe's work (letter to D. MacKenzie, June 2, 2004).

28. On Alchian's membership of the Mont Pèlerin Society, see Mirowski 2002, p. 318. For Alchian's methodological views, see Alchian 1950. This paper was cited by Friedman (1953a, p. 19) as similar in "spirit" and "approach" to one of the main strands of Friedman's argument.

29. William F. Sharpe, email to D. MacKenzie, January 29, 2004.

30. Ibid.

31. For a more detailed account of this reasoning, see Sharpe 1970, p. 82.

32. See Treynor 1962, p. 14; Lintner 1965a, p. 25; Mossin 1966, p. 775.

33. Regnault 1863, pp. 94–95, my translation, capitalization in original deleted. I am grateful to Franck Jovanovic for a copy of Regnault 1863.

34. Ibid., pp. 50–51, my translation.

35. Bachelier 1900, p. 21. In this and the subsequent quotation my translation follows that of A. James Boness (Bachelier 1964).

36. Denoting the probability that the price of the bond at time  $t$  will be between  $x$  and  $x + dx$  by  $p_x dx$ , Bachelier showed that his integral equation was satisfied by  $p_{x,t} = (H/\sqrt{t})\exp[-(\pi H^2 x^2/t)]$ , where  $H$  is a constant. See Bachelier 1900, p. 38.

37. The Moscow State University econometrician Evgeny Evgenievich Slutsky also investigated the possibility of random fluctuations being the source of "cyclic processes" such as business cycles. Working knew of Slutsky's work (Working 1934, p. 11), but probably second-hand: it did not appear in English until 1937 (Slutsky 1937).

38. See Pearson 1905. On Pearson's interests in the application of statistical methods to biology and to eugenics, see MacKenzie 1981 and Porter 2004. Readers of Galison 1997 will know that there are deep issues as to what "random" means in contexts such as "random number" tables, but those can be set aside for current purposes.

39. Given this use of the Stanford data, it might be assumed that Kendall knew of Working's analyses, but Kendall (1953) does not cite him.

40. Cotton was the exception. The first-order (month and immediately previous month) serial correlations were substantial, ranging from 0.2 to 0.4 (Kendall 1953, p. 23).

41. Samuelson n.d., p. 2. This unpublished typescript titled "The Economic Brownian Motion" is in Samuelson's personal files, and at Samuelson's request was kindly retrieved for me by his assistant Janice Murray. It probably dates from around 1960 or shortly thereafter, because Samuelson's "acquaintanceship" with Bachelier's work is described as dating back "only half a dozen years" (n.d., p. 2). As noted in the text, Kruizenga (and hence Samuelson) certainly knew of Bachelier's work by 1956, and possibly somewhat earlier.

42. In modern option theory, the price of options is seen as determined by considerations of arbitrage, not by beliefs about whether stock prices will rise or fall.

43. See, e.g., Galison 1994; Jerison, Singer, and Stroock 1997; Mirowski 2002, pp. 58–68.

44. Samuelson's attention may first have been drawn to the need to revise Bachelier's arithmetic Brownian motion by the fact that on that assumption the price of an option is proportional to the square root of time to expiration, and so the price of an option of long enough duration can rise above the price of the stock on which it is an option. A geometric Brownian motion "get[s] rid of the paradox" (Samuelson n.d., pp. 3–4 and 13–14).

45. Whereas the normal distribution is symmetrical, the log-normal is skewed, with a long positive "tail," suggesting it might be useful in the analysis of phenomena such as the distribution of income, and it was relatively well-known to economists. Among the pioneers of the use of the log-normal distribution was the French engineer and economist Robert Gibrat (Armatte 1998). Samuelson had "long known" of Gibrat's work (letter to D. MacKenzie, January 28, 2004). In 1957, J. Aitchison and J. A. C. Brown of the University of Cambridge published what was in effect a textbook of the log-normal distribution and its economic applications (Aitchison and Brown 1957).

46. Samuelson n.d. is most likely the text of one or more of these lectures, and Samuelson recalls using its opening lines in a 1960 talk to the American Philosophical Society.

47. Samuelson n.d., p. 11; Samuelson 1965a, p. 13.

48. Samuelson 1973, p. 5. Churchill Downs is the Louisville racetrack on which the Kentucky Derby is run.

49. The only previous work of this kind cited in Osborne 1959 was Cowles and Jones 1937.

50. Roberts 1959; Osborne 1959.

51. Samuelson 1965b; Mandelbrot 1966. On the replacement of random walk models by martingale models, see LeRoy 1989, especially pp. 1588–1589.

52. Fama 1970, p. 409. See also Scholes 1970.

53. Here Fama was drawing on Niederhoffer and Osborne 1966.

### **Chapter 3**

1. Durand 1968, p. 848. Research on Regnault and Bachelier is at too early a stage for us to be certain, but they do not seem to have created "schools" of research that outlived them.

2. Miller joined the faculty in 1961. Fama was appointed in 1963.

3. According to Black (1989, p. 7), Fama and Miller "suggested to the *Journal of Political Economy* . . . that perhaps [Black and Scholes 1973] was worth more serious consideration."

4. Leland interview; email message to D. MacKenzie from Hayne Leland, April 22, 2004.
5. I am grateful to Perry Mehrling for useful comments on economists' reluctance to embrace work on finance.
6. Sharpe 1995, p. 218. Sharpe admits that his passion for sailing was a factor in his acceptance of the offer from Seattle.
7. Cootner 1977, p. 553.
8. In his comments on the first draft of this book, Perry Mehrling emphasized the extent to which U.S. markets became focused on war debts.
9. On Dow's career, see Wendt 1982.
10. See also Godechot 2001, pp. 218–230; Lynn 2004.
11. Jensen 1968, p. 415. (See also Jensen 1969.) Before 1957, the S&P index comprised only 90 stocks (it was enlarged to 500 on March 1, 1957), so “for the earlier period the index is a poorer estimate of the returns on the market portfolio” (Jensen 1968, p. 399).
12. In later years, part of mutual fund underperformance seems to have resulted from arbitrageurs exploiting the fact that the prices of mutual fund shares were fixed once a day at a set time while the prices of the underlying portfolio fluctuated continuously. A study by Eric Zitzewitz of Stanford University estimated that in 2001 “stale-price arbitrage” cost the average shareholder in an internationally diversified mutual fund 1.1 percent of the value of his/her holding (Plender 2003). That this “stale-price arbitrage” was practiced in the 1960s is, however, doubtful. The much higher transaction costs of stock trading in the 1960s would probably have made it unattractive.
13. There are transcriptions of Kaplan's and Rinfret's remarks on pp. 407–408 and 417–424 of Hontchar 1968. My quotations of Rinfret are from p. 420.
14. I am grateful to Perry Mehrling for pointing me to this editorial, which is unsigned but is clearly by Treynor.
15. Brooks 1973, pp. 4, 18, 22–23, 292, and 305.
16. John O'Brien moved to James A. Oliphant & Co. to set up performance measurement.
17. “Before dinner at home, he [Rosenberg] solemnly intones a Sanskrit chant. He attends Esalen. He has meditated. He believes in and likes to talk about telepathy and clairvoyance.” (Welles 1978, p. 61) Dennis Tito, the main competitor to BARRA (Barr Rosenberg and Associates) commented: “Despite all we're doing, he gets all the coverage. Somebody came up to me the other day and said ‘That Rosenberg's fantastic. Did you know he runs four miles a day?’ Well, I run four miles a day, too, and nobody knows about it. I'm sure he has an interesting lifestyle, but I have an interesting lifestyle also, and nobody knows about that either.” (Welles 1978, p. 66) Rosenberg was not simply applying the Capital Asset Pricing Model but deploying statistically more elaborate models of risk and return. (See Bernstein 1992, pp. 260–263.)

18. Source: O'Brien interview. Dennis Tito, who led Wilshire, had been an aerospace engineer at the Jet Propulsion Laboratory in Pasadena. He achieved worldwide fame in 2001 when he paid \$20 million to take part in a Soyuz mission to the International Space Station.

19. McQuown interview; Vasicek interview. Developments in this period at Wells Fargo are well described on pp. 233–252 of Bernstein 1992.

20. The initial Samsonite plan had not been an index fund per se, but a leveraged low-beta portfolio. (See chapter 4.) The index fund that replaced this plan sought to have equal amounts of capital invested in each of the stocks traded on the New York Stock Exchange, so replicating an “equal-dollar-weighted” index (Fouse interview; Jahnke and Skelton 1990, pp. 63–64). The problem with equal dollar weights, however, is that to maintain them as stock prices fluctuate requires frequent rebalancing of the portfolio, which incurs transaction costs. Later index funds tended to seek to track the S&P 500 index, which includes stocks not traded on the New York Stock Exchange and, crucially, weights them by market capitalization, so the need for rebalancing is greatly reduced.

21. LeBaron explains: “Batterymarch continued to have a good performing active equity strategy running in parallel with its indexing portfolios. . . . My point was if you wanted to invest in the mainstream equity styles, you should do [it] with indexing. But if you were willing to be different and, within that, try to be right, you had a chance to be a successful investor due to skill (yes, and luck also [played] a part).” (email message to D. MacKenzie, May 13, 2004)

22. Ring 1985, p. 1; Wurgler and Zhuravskaya 2002, p. 595. The estimates for 1988 and 1996 include only funds tracking the S&P 500 index.

## **Chapter 4**

1. I take the term “benchmark” from a reference to the proposition of dividend-policy irrelevance in a talk given by Thomas Hemmer during the London School of Economics' Accountancy Research Training Week in 2003.

2. If they cannot borrow, said Black in a later paper, investors who “want lots of market risk” because of the consequent high expected returns “will bid up the prices of high-beta stocks,” so reducing the returns on those stocks (Black 1993, p. 10).

3. See Black 1993, p. 10; Black and Scholes 1974, p. 409.

4. Letter to D. MacKenzie from William L. Fouse (May 17, 2004). Fouse criticized the idea of the leveraged low-beta fund within Wells Fargo, earning Black's ire: “I said ‘Well, Jeez, there's something wrong here, guys. We're going to end up leveraging utilities and food stocks, and the future earnings developments of these groups can wander away from what was expected and you're going to find yourself in trouble.’ . . . Fischer Black got so angry with me . . . he got up and left the conference room.” (Fouse interview)

5. The act was sponsored by Senator Carter Glass and Representative Henry Steagall.
6. On the act's interpretation by the Supreme Court, see Seligman 1982, p. 372. For some of the other difficulties of the Wells Fargo efforts, see Black and Scholes 1974.
7. Black 1993, p. 17. See also Amihud, Christensen, and Mendelson 1992; MacKinlay 1995.
8. Schwert 2002 is a useful review of the literature on anomalies.
9. Marc R. Reinganum (1981), also a Chicago Ph.D. student, also found a “small-firm” effect, but Banz's identification of it seems to have been earlier.
10. See, e.g., Barberis and Thaler 2002; Shefrin 2000; Shiller 2000, 2002; Shleifer 2000; Thaler 2000.
11. Banz's suggested “arbitrage” (the quotation marks are his) was to construct a portfolio of holdings of small stocks and short positions in large stocks that had a beta of 0 and required no net investment (1981, p. 14).
12. See Fama 1998 for a critical discussion of the evidence for anomalies.
13. Jay Ritter, one of the “assistant professors” involved, describes his and his colleagues' activities in Ritter 1996.
14. Source: Dimensional Fund Advisors' website, [www.dfaus.com](http://www.dfaus.com), accessed March 17, 2005.
15. Source: *ibid.*
16. An open-end fund (in the U.K., a unit trust) is a mutual fund in which investors can redeem their investments by application to the managers, who can create or eliminate units as required by buying or selling assets. A closed-end fund has a fixed quantity of stock, and investors wishing to liquidate their investments must do so by selling their stock to others.
17. One can only be tentative because there are difficult underlying issues here: see, for example, Stiglitz 1990 and the subsequent papers in the spring 1990 issue of the *Journal of Economic Perspectives*.
18. One possibility is that the Standard and Poor's Corporation, which rates bonds as well as maintaining the index, has private information on the likely longevity of corporations, and that this information affects its inclusion decisions (so addition of a stock to the S&P 500 might thus convey “good news”). For a partial test of this possibility, see Shleifer 1986, pp. 585–587. Shleifer believes it is unlikely to account for the effect.
19. See also Harris and Gurel 1986; Wurgler and Zhuravskaya 2002.
20. One way of expressing the underlying issue is via the slope of the demand curve for a corporation's stock. It is conventional in economics to express demand as a line or curve on a graph in which the price of a product is on the vertical axis and the

quantity of the product that will be purchased is on the horizontal axis. Most products then have downward-sloping demand curves: the lower the price, the more of the product will be purchased. On the efficient-market view expressed by Scholes, however, the demand curve for a corporation's stock is flat: "... the demand curve facing individual shareholders is essentially horizontal" (Scholes 1972, p. 182). Equivalently, the price elasticity of demand—"the percentage change in the quantity of a good demanded divided by the corresponding percentage change in its price" (Begg, Fischer, and Dornbusch 1997, p. 57)—is in the case of stock "virtually infinite" (Scholes 1970, p. 1). Above a certain price level, the stock would not be purchased at all; below that level, there would be effectively infinite demand for it. The view that the demand curve for a stock is flat seems to have been deeply counterintuitive to traditional finance scholars, and I suspect remains so to many practitioners, yet Scholes concluded that his Ph.D. research, an empirical study of "secondary distributions" (sales of large blocks of already-issued stock), was consistent with this view (Scholes 1970, 1972).

21. I draw the distinction between "mild" and "wild" randomness from Mandelbrot (1997, p. 9). In addition to the specific sources cited, this section overall draws on an extremely helpful response by Benoit Mandelbrot to its first draft (private communication to D. MacKenzie, June 24, 2004).

22. See also Izquierdo 1998.

23. See also Barcellos 1985; Gleick 1988; Mandelbrot and Hudson 2004.

24. Mandelbrot interview. The quotation re Piaget is from p. 212 of Barcellos 1985.

25. See, for example, Lakatos 1976, pp. 21–22.

26. A simple definition of "polyhedron"—too simple, as Lakatos's account shows—is a three-dimensional figure each of the faces of which is a polygon (a plane figure bounded by straight lines).

27. The theorem is Euler's: that the numbers of vertices ( $V$ ), edges ( $E$ ), and faces ( $F$ ) of any polyhedron are related by  $V - E + F = 2$ .

28. Zipf's law is an example of a "power law." If  $r$  is a word's rank ( $r = 1$  for the most common word in a language,  $r = 2$  for the second most common, etc.) and  $p$  is the probability of its occurrence, then "for ... large samples ... and for words other than the most frequent ones one has, whichever the language in which a text was written  $p(r) = Pr^{-B}$ , where  $P$  and  $B$  are some constants" (Mandelbrot 1961, p. 194). A simple version (with  $B = 1$ ) appears in Paulos 2004: "In English, for example, the word 'the' appears most frequently and is said to have rank order 1; the words of rank 2, 3, and 4 are 'of,' 'and,' and 'to,' respectively. Zipf's Law relates the frequency of a word to its rank order  $r$  and states that a word's frequency in a written text is proportional to  $1/r^1$ ; that is, inversely proportional to the first power of  $r$ . (Thus 'of' occurs half as frequently as 'the,' 'and' a third as frequently as 'the'—and 'synecdoche' hardly at all.)" (In this quotation, Paulos's  $k$  has been replaced by  $r$ .)

29. Pareto's law of the distribution of income, as expressed by Mandelbrot, is as follows. Let  $u$  be an income level, and  $P(u)$  the proportion of people whose incomes are greater than  $u$ . Then, for large  $u$ ,  $P(u) \sim Cu^{-\alpha}$ , where  $C$  and  $\alpha$  are constants, and " $\sim$ "



means “behaves like”; that is,  $P(u)/Cu^{-\alpha}$  tends to 1 as  $u$  tends to infinity. See Mandelbrot 1960, p. 81; 1963b, p. 421; 1997, p. 79.

30. In the 1960s and the 1970s, other authors often followed Mandelbrot in referring to Lévy distributions as “stable Paretian.” I follow the more recent invocation by Mandelbrot of “Lévy” because (as Mandelbrot made clear in his 1963a paper) it is in Lévy’s work, not in Pareto’s, that the family of distributions was developed.

31. D. MacKenzie, notes on the University of Edinburgh courses Statistics 3 (academic year 1970–71) and Statistics 4 (1971–72).

32. Zolotarev 1986, p. 3.

33. See the work cited on p. 394 of Mandelbrot 1963a.

34. Mandelbrot 1987 is a revised version of a lecture (the original text of which I have unfortunately not seen) on “The Epistemology of Chance in Certain Newer Sciences” delivered by Mandelbrot at the International Congress for Logic, Methodology and the Philosophy of Science in Jerusalem in September 1964.

35. Among those who encouraged Mandelbrot to encompass physics as well as economics was the Harvard sociologist Harrison White, who attended a seminar given by Mandelbrot while a visiting professor of economics at Harvard in 1962–63 (Mandelbrot 1997, p. 103).

36. Thus the sample should have contained only three to four events per stock beyond three standard deviations, but “the actual numbers range from six to twenty-three” (Fama 1965, p. 49).

37. Of course, the stocks that made up the Dow Jones were not typical of all stocks, but Fama argued that any bias from the choice of these stable “blue chip” stocks was likely to be unfavorable from the viewpoint of the Lévy hypothesis (Fama 1965, p. 46).

38. Roll 1970, p. 81.

39. See Sent 1998, 1999.

40. A later line of empirical criticism of Lévy distributions is based on work estimating the values of the power-law “tail indices” of the distributions of price changes. This work seems generally to find values incompatible with Lévy distributions. See, for example, Lux 1996 and the papers cited therein.

41. Mandelbrot interview; Mandelbrot, personal communication to D. MacKenzie, June 24, 2004. In the latter he continues: “By exquisite irony, it is during the same year 1972 that I published my first full article on multifractals, which amounted to moving on from ‘simple’ scaling to a new and a more sophisticated form. Officer happens to have observed empirically something I was predicting with the new more general model. (True, I did not publish this prediction until much later.)”

42. The standard errors of Officer’s estimates of the alpha values were 0.09 for daily returns and 0.15 for returns summed over 20 days (Officer 1972, p. 810).

43. Mandelbrot, personal communication to D. MacKenzie, June 24, 2004.

44. The role of Clark's work is emphasized by Mirowski, who draws on an interview with Sargent by Sent to note that it was a particular influence on Sargent (Mirowski 1995, p. 587). For Mandelbrot's comments on Clark's work and on its relations to Mandelbrot and Taylor 1967, see Mandelbrot 1973; Mandelbrot 1997, pp. 523–525.

45. Vasicek interview; email message to D. MacKenzie from Oldrich Vasicek, April 28, 2004. The same feature was pointed out as an objection to infinite-variance Lévy models by Merton (1976, p. 127).

46. Mandelbrot interview; Mandelbrot, personal communications to D. MacKenzie, June 24, September 23, and September 24, 2004. In the personal communications he elaborates: "Critics should not focus on infinite variance because the basic novelty that must be faced is far broader. More general recent models of mine allow variance to be either infinite or finite, but in every case something old and familiar must diverge. To hide the 'monster's fangs' is easy but does not make them less biting. . . . No one would have reacted badly to a distribution with an infinite fifth moment; nevertheless its exponential would also have an infinite expectation. Altogether, it used to be safe for scientists to believe that the universe is organized for simplicity and the conveniences of the mathematicians. But my fifty-year long study of complexity suggests otherwise." In regard to the meaning of moments (the expected value, the variance, and so on) he added: "For example, what can you deduce from knowing the average sales of a US software firm in 2003? It is the ratio with a numerator somewhat higher than the sales of Microsoft and a denominator that is meaningless because it is overly dependent on the smallest sales used in taking the average. What about the expectation of a 'nearly' log-normal variable? It is so extremely sensitive to inevitable deviations from lognormality that it hardly matters. This being granted, I know that any form of divergence tends to inspire horror. Long ago, Pareto has identified the unpleasant fact that in every power-law distribution moments of sufficiently high order are infinite. He recognized this feature as unpleasant and suggested truncating the offending tail, for example, by multiplying it by a slowly decaying exponential factor. The moments become finite, true, but their values are altogether meaningless because they depend on the artificial truncation. An exponential cutoff has also been proposed by physicists upset by the divergences that follow from inverse square Newtonian attraction. But this exponential decay has proved to be sterile and progress in gravitation became possible when divergences were not hidden but conquered. On the other hand, a divergence called 'ultra-violet catastrophe' was eliminated by a cutoff, the quantum, that proved to be of physical reality. The lesson for finance: divergences may or may not be significant, but must not be papered over in advance."

47. See the works collected in Mandelbrot 1997.

48. Mandelbrot, personal communication to D. MacKenzie, June 24, 2004.

## **Chapter 5**

1. Michie 1999, pp. 22 and 49; Preda 2001a, p. 214.

2. Castelli 1877, pp. 7–8, 74–77.

3. Another author of the period who discussed options was Henri Lefèvre, private secretary to financier James Rothschild (Jovanovic 2000, p. 398). Lefèvre introduced what has become known as the “payoff diagram,” a simple graphical representation of the value of an option at its expiration (Preda 2004a).

4. This is noted by Bachelier’s translator, A. James Boness (Bachelier 1964, p. 76).

5. The curves are of course specific to a particular warrant, but as well as providing their readers with Kassouf’s formula for calculating them, Thorp and Kassouf (1967, pp. 78–79) provided standardized “average” curves based on the prices of 1964–1966.

6. As Thorp explained (1973, p. 526), “to sell warrants short [and] buy stocks, and yet achieve the riskless rate of return  $r$  requires a higher warrant short sale price than for the corresponding call [option]” when the latter is analyzed according to the assumptions made by Black and Scholes. Thorp had also been selling options in the New York market, where the seller *did* receive the sale price immediately (minus “margin” retained by the broker), but the price discrepancies he was exploiting were gross (so gross he felt able to proceed without hedging in stock), and thus the requisite discount factor was not a salient consideration.

7. I am grateful to Jack Treynor for providing me with this and other unpublished papers of his from this period.

8. For a useful analysis of the different variants of the Capital Asset Pricing Model, see French 2002.

9. The dating of the original typescript of Treynor’s paper to 1962 rather than 1961—the date given by Bernstein (1992, p. 328)—is based on a private communication to D. MacKenzie from Jack Treynor, March 4, 2003.

10. Treynor interview; Black (1989, p. 5). Treynor and Black did not publish their work immediately: it eventually appeared in 1976. The corrected differential equation is equation 2 of Treynor and Black 1976.

11. Unfortunately, I have been unable to find any contemporaneous documentary record of this initial phase of Black’s work on option pricing, and it may be that none survives. Black (1989, p. 5) said he had “notes containing the differential equation that are dated June 1969,” but I have not been able to locate these notes. The earliest extant version of Black’s work appears to date from August 1970 (Black and Scholes 1970a), and is in the personal files of Stewart Myers at MIT. (I am grateful to Perry Mehrling for a copy of this paper.) There is an October 1970 version in Fischer Black’s papers (Black and Scholes 1970b). Black’s own account of the history of option formula (Black 1989, p. 5), contains only a verbal description of the initial phase of his work. It seems clear, however, that what is being described is the “alternative derivation” of the October paper (Black and Scholes 1970b, p. 10–12): the main derivation in that paper and in Black and Scholes 1970a is the hedged portfolio derivation described below, which was a later development.

12. Thus on pp. 8–9 of Black and Scholes 1970b they show that the covariance of the hedged portfolio with the overall return on the market was zero, assuming that in small enough time intervals changes in stock price and overall market returns have a joint

normal distribution. Using the Taylor expansion of  $w$ , Black and Scholes showed that the covariance between the change in the value of the hedged portfolio and in the market portfolio ( $\Delta m$ ) is  $(1/2)(\partial w^2 / \partial x^2) \text{cov}(\Delta x^2, \Delta m)$ , where  $\text{cov}$  indicates covariance. If  $\Delta x$  and  $\Delta m$  are jointly normally distributed over small time periods,  $\text{cov}(\Delta x^2, \Delta m)$  is the covariance of the square of a normal variable with a normal variable, and thus is zero. With a zero covariance with the market, the hedged portfolio must, according to the Capital Asset Pricing Model, earn the riskless rate of interest.

13. A quadratic utility function has the form  $U(y) = l + my + ny^2$ , where  $l$ ,  $m$ , and  $n$  are constant.  $n$  must be negative if, as will in general be the case, “the investor prefers smaller standard deviation to larger standard deviation (expected return remaining the same)” (Markowitz 1959, p. 288), and negative  $n$  implies that above a threshold value utility will diminish with increasing returns. Markowitz’s position is that while quadratic utility cannot reasonably be assumed, a quadratic function centered on expected return is a good approximation to a wide range of utility functions: see Levy and Markowitz 1979.

14. Fischer Black, interviewed by Zvi Bodie, July 1989. I am grateful to Professor Bodie for a copy of the transcript of this unpublished interview.

15. Paul A. Samuelson, letter to D. MacKenzie, February 23, 2000.

16. Ibid.

17. Kuhn 1970, p. 175. See Barnes 1982.

18. The core of arbitrage pricing theory is well described by Perry Mehrling. In the theory, “a capital asset . . . represents a portfolio of exposures to different risk factors whose prices are determined by the preferences of wealth holders. The price of the capital asset is then, so goes the argument, equal to the weighted sum of the risk factor prices, with the weights proportional to the exposures to each factor. The argument depends on something called the *principle of no arbitrage*. If the capital asset were at any other price, then some component risk factor must have an implied price that is different from its price as implied by other capital assets, and riskless arbitrage profits must be attainable by buying the risk factor at the lower price and selling it at the higher price” (Mehrling 2000, p. 83). The risk factors are not specified in the arbitrage pricing model itself, but in its empirical applications they are identified by statistical techniques. The model is in a sense a generalization of the Capital Asset Pricing Model’s single risk factor, and it predicts an overall equilibrium relationship of an analogous form: “every equilibrium will be characterized by a linear relationship between each asset’s expected return and its return’s response amplitudes, or loadings, on the common factors” (Roll and Ross 1980, p. 1074).

19. The first derivation of the Black-Scholes equation that Harrison and Kreps would allow as reasonably rigorous is in Merton 1977. This paper explicitly responded to queries that had been raised about the original derivation. For example, Smith (1976, p. 23) had noticed that the option price,  $w$ , is, in the original work, assumed but not proved “to be twice differentiable everywhere.”

20. The application of the notion of “martingale” to finance is a little more complicated than might be suggested by the basic definition of a martingale as “a zero drift

stochastic process” (Hull 2000, p. 522). Mostly, prices of securities are modeled as martingales with respect to the price of some other security or “numeraire,” such as a security that pays the riskless rate of interest with certainty. See, for example, Hull 2000, pp. 507–529.

21. I am grateful to Andreas Kyprianou, then of the University of Edinburgh Department of Mathematics and Statistics, for giving me a copy of his notes for his 1998–99 course “Stochastic Processes I.”

## **Chapter 6**

1. See, for example, Klينenberg 2002.

2. In 1994, Melamed, whose political sympathies are generally Republican, investigated the records of Hillary Clinton’s trading at the request of the White House, and found no evidence of wrongdoing on her part (Melamed and Tamarkin 1996, pp. 437–444).

3. Melamed and Tamarkin 1996, p. 101.

4. See Swan 2000 for an introduction to the patchwork of U.S. law governing futures.

5. See Melamed and Tamarkin 1996, pp. 174–175. The International Commerce Exchange was the new name for New York’s venerable Produce Exchange, for which see Emery 1896.

6. Friedman 1971, p. 2. I am grateful to Katie Dishman, archivist of the Chicago Mercantile Exchange, for providing me with a copy of this and other unpublished material from the Merc’s archives.

7. Cohen’s speech to the 1968 *Institutional Investor* conference on money management is transcribed in Hontchar 1968 (pp. 68–75, quotations at pp. 73–74).

8. Bernstein 2001, p. 7; [www.nathaninc.com](http://www.nathaninc.com), accessed February 22, 2005.

9. I am extremely grateful to Emily Schmitz of the Chicago Board Options Exchange for providing me with a copy of the Nathan Report. Like Baumol, Malkiel, and Quandt, Merton Miller also contributed an appendix to the Nathan Report. He felt that some of the case being made for options trading—such as Malkiel and Quandt’s conclusion that institutions such as university endowments could earn enhanced investment returns by selling options on stocks they held—violated the efficient-market hypothesis. “Organized option trading in stocks is not the greatest invention since sliced bread as some of the puffing by spokesmen for the Board [of Trade] often seems to suggest,” wrote Miller. Such trading was only “a modest marginal improvement in the structure of the capital markets,” albeit “one still very much worth making” (Nathan Associates 1969, vol. 2, p. 51).

10. I am grateful to Yuval Millo for access to the transcript of this interview, which took place in Chicago on April 6, 2000.

11. See, e.g., Hull interview. Abolafia (1996) describes similar phenomena on the New York Mercantile Exchange.
12. William R. Power, email message to D. MacKenzie, February 22, 2004.
13. Falloon 1998, pp. 225–227.
14. Similar points were also made by Doherty (interview) and Power (interview). As noted in chapter 1, in the period discussed here prices were quoted in binary fractions. Options are denominated in lots of 100 shares, but the price quoted is per share. An option contract priced at  $\$5\frac{3}{8}$ , for example, actually cost  $\$537.50$ .
15. See, for example, Widick 2003; Zaloom 2003, 2004; Lynn 2004.
16. For a study of gender relations in open outcry pits, see Levin 2001.
17. For the state-of-the-art in the theoretical analysis of the pricing of American puts and of American calls on dividend-bearing stocks as it existed in 1973, see Merton 1973a, pp. 170–174. Merton (*ibid.*, pp. 143–144) showed, under quite general conditions, that the early exercise of an American call on a non-dividend-bearing stock is never optimal, so its theoretical value is equal to that of a European call.
18. “My initial estimates of volatility are based on 10 years of daily data on stock prices and dividends, with more weight on more recent data. Each month, I update the estimates. Roughly speaking, last month’s estimate gets four-fifths weight, and the most recent month’s actual volatility gets one-fifth weight. I also make some use of the changes in volatility on stocks generally, of the direction in which the stock price has been moving, and of the ‘market’s estimates’ of volatility, as suggested by the level of option prices for the stock” (Black 1975b, p. 5).
19. I am grateful for this information to Clay Struve, who as a MIT undergraduate in the 1970s earned money doing such tasks for Black’s option service.
20. See Gastineau 1975, p. 184.
21. See also *ibid.*, pp. 177–178.
22. The text discusses what Black (1975a, p. 39) calls a “money spread.” Spreaders also undertook “time spreads” (buying and selling options on the same underlying stock with identical strike prices but different expiration dates). I would conjecture that these “time spreads” improved the fit between market prices and the Black-Scholes-Merton model on the other main aspect of Rubinstein’s test, which examined the implied volatilities of options on the same stock with identical strike prices and different expirations.
23. Robert S. Plotkin to Richard L. Teberg, December 5, 1978, reproduced in SEC 1979 (pp. 776–778).
24. This episode will be discussed in Millo (forthcoming a). Stock bought as a hedge was eligible for 75 percent credit so long as the “option is not out-of-the-money by more than 5 percent” (SEC 1979, p. 679). A call is “out-of-the-money” if its strike or exercise price is above the stock price; a put is “out-of-the-money” if its strike price is below the stock price.

25. Consider, for example, these two positions on a stock that pays no dividends: (a) a call option with a strike price equal to the current stock price, plus cash equivalent to the stock price; and (b) buying the stock in question plus a put with the same strike price as the call. (The call and the put have the same expiration date, and both are European: they can be exercised only at their expiration.) The payoff of the two positions is equal. If the stock price rises above the strike price, the payoff from exercising the call is just the stock's gain in price, while if the stock price falls below the strike price, exercise of the put eliminates the loss on the stock. Factoring in the capacity to earn interest on the cash yields a simple equation connecting put and call prices. See, e.g., Hull 2000, p. 174.

26. For a realistic example—including transaction costs—of what market makers need to monitor in order to exploit violations of put-call parity, see Options Institute 1999, pp. 261–262.

27. The Amex data needed to test for breaches of parity are not public and may no longer exist.

28. See Millo (forthcoming b) on the Shad-Johnson Accord and how the dispute between the Commodity Futures Trading Commission and SEC shaped derivatives based on stock indices.

29.  $F_0 = S_0 e^{(r-q)T}$ , where  $F_0$  is the theoretical value of the future,  $S_0$  the current index level,  $r$  the riskless rate of interest,  $q$  the annualized dividend yield, and  $T$  the time remaining until the future's expiry (Hull 2000, p. 64).

30. For commodities, the issue of “convenience yield” (for example, “the ability to profit from temporary local shortages”) also arises. See Hull 2000, pp. 72–73.

31. The arbitrage's profitability was enhanced by a quirk of U.S. tax law, which meant that 60 percent of profits on futures were treated as long-term capital gains, “regardless of how long you hold them” (Weinberger interview).

## **Chapter 7**

1. *State Street Bank & Trust Company vs. Signature Financial Group*, 149 F.3d 1368 (U.S. Court of Appeals Federal Circuit, July 23, 1998) has changed the interpretation of U.S. patent law in this respect.

2. Typescript of remarks of David S. Ruder, chair of the SEC, to the Bond Club of Chicago, October 6, 1987, p. 13, summarizing SEC 1987. I am grateful to William L. Fouse, Chairman Emeritus, Mellon Capital Management, for this typescript of Ruder's speech and a large collection of other published and unpublished material on portfolio insurance. Fouse's group at Mellon, particularly Jeffrey P. Ricker, was perhaps the most influential set of critics of portfolio insurance before October 1987 (Fouse interview)

3. Stewart and Hertzberg 1987; Brady Commission 1988, p. 4; Hull 2000, p. 65.

4. Brady Commission 1988, pp. 40–41; Stewart and Hertzberg 1987, p. 23; Schwert 1990, p. 80.

5. Brady Commission 1988, p. 41.

6. In the analysis by Roll, “computer-designed trading” is judged to have been present in five of the 23 national markets examined: Canada, France, Japan, U.K., and U.S. (Roll 1988, pp. 29–30). This judgment, however, masks large differences in scale. For example, the Bank of England reported that: “In contrast to the US markets, the use of stock-related derivative products in the United Kingdom is very limited and the volume of stock-index-related business is very small” (Anonymous 1988a, p. 58).

7. Compare the opposite conclusions reached in this way by Roll (1988) and Jacobs (1999, p. 177).

8. Malliaris and Urrutia apply Granger causality tests, in which time series A is deemed to have been a cause of time series B if previous values of A improve predictions of values of B. Examining prices in New York, Tokyo, London, Hong Kong, Singapore, and Australia, they conclude that the Tokyo market played predominantly “a passive role” (was influenced by changes in New York and elsewhere rather than influencing them) and “no market led New York during the crash,” but that there were mutual, bi-directional, influences between New York and London and New York and Hong Kong (Malliaris and Urrutia 1992, p. 362).

9. For example, dividend discount models, in which stock prices are the discounted present value of the expected future income stream to which the stock is an entitlement (see chapter 2), are very sensitive to projected rates of dividend growth and choice of risk-adjusted discount rate. A small reduction in the former and small rise in the latter can cause a large drop in stock prices (see the example in Miller 1991, pp. 99–100). If the economic “climate” as well as the weather is persistent (serially correlated), small events—a slight rain shower after a prolonged drought (Mandelbrot 1966)—can rationally trigger “revisions in risk allowances and/or in long-run growth projections” (Miller 1991, p. 100). “What’s [serially] correlated is the feeling that these are good times or bad times and if there’s a slight chill in the air, it says ‘Look, the good years are over and we’re now heading into a period of bad times.’ Bam! The market will crash and it should, and that’s perfect Mandelbrot, although it’ll be very hard to show it in specific numbers.” (Miller interview) Another possibility is that the events on October 14–16 led to “a sudden extremely large upward shift” in estimates of future volatility “that may have convinced the most risk-sensitive investors to exit the market on Monday” (Rubinstein 2001, p. 26; see also Black 1988b).

10. Again, I am grateful to William L. Fouse for a copy of this typescript.

11. While Rubinstein entertains the explanation discussed in the text, he places less weight on portfolio insurance, and suggests mechanisms such as increases in volatility estimates and the effects of short-sale constraints (discussed in chapter 9) in which portfolio insurance would not have played an essential role (Rubinstein interview; Rubinstein 2001, p. 26). Jacklin, Kleidon, and Pfleiderer (1992) develop a model similar to that put forward by Gennotte and Leland (1990) but in which it is earlier portfolio



insurance purchases that are misunderstood as being the result of positive information. This is also in principle plausible, but its empirical significance is reduced by the fact that as the market rose before October 1987 the clients of portfolio insurers often instructed the latter to raise their floors. This required sales of futures and may have outweighed the purchases necessitated in a rising market by programs with unchanged floors (Rubinstein interview).

12. See also Fabian Muniesa's interview with Steven Wunsch, quoted on p. 334 of Muniesa 2003.

13. For the contrasting case of France, see Muniesa 2003.

14. For useful reviews of what has become one of the most active areas of research in financial econometrics, see Bates 2003 and Garcia, Ghysels, and Renault 2004.

15. See, for example, the survey by Skiadopoulos (2001) of non-Black-Scholes option-pricing models that are consistent with the skew.

16. As was noted in chapter 4, a "leptokurtic" distribution has a high peak and fat tails.

17. Financial economists refer to the possibility that prices may reflect the subjective probability of an event that does not in fact take place in the time period studied as a "peso problem." The reference is to the influence on the prices of Mexican assets in the period of managed exchange rates of the possibility of a sudden, large devaluation of the peso.

18. On the profitability of the sale of puts, see also Ederington and Guan 2000.

19. Patterns of option pricing in markets outside the U.S. have not been studied with anything like the intensity of the study of the U.S., so one has to be more tentative in regard to those non-U.S. markets.

20. The German options market did manifest a skew after 1987, but it was opposite in direction to that in the U.S. (Bates 2000, p. 225).

21. Power (interview) and Wenman (interview 2) were particularly eloquent in respect to the dangers of selling puts, but the sentiment seemed widely shared.

22. On the market for catastrophe risk, see Froot 2001.

23. Ederington and Guan (2000, p. 4) note that if the sellers of index puts are scrupulous in their delta hedging, their frequent rebalancing incurs "substantial transaction costs which quickly eat away the profits" of put sales.

24. That is, puts with strike prices well below current stock prices or index levels.

25. Tim Hinkes, email message to D. MacKenzie, August 27, 2001. The alpha levels quoted here are daily values (Hinkes, email to MacKenzie, June 28, 2004).

26. See, for example, *Financial Stability Review* no 15 (December 2003), p. 19, chart 9. The 1998 peak was around 40 percent, not the 170 percent of 1987. In part the difference is because the 1998 crisis was played out primarily in the bond and bond-

derivatives markets, rather than in the markets for stock and exchange-traded stock derivatives, as in 1987. The difference also indicates, however, that 1987 was much the more serious crisis.

## **Chapter 8**

1. Strictly, the fund was the investment vehicle (Long-Term Capital Portfolio) that LTCM managed, but to avoid complication I shall refer to both as LTCM.

2. See, for example, the examination of the efficiency of the market in Treasury bills (bonds with short maturities) by Roll (1970).

3. There may be other factors that affect the premium for on-the-run bonds, notably the capacity to earn a small extra return by lending them to others for short sale (Krishnamurthy 2002, p. 466).

4. Two important early papers were Vasicek 1977 and Cox, Ingersoll, and Ross 1985. The latter was available in preprint form at the end of the 1970s.

5. As noted in previous chapters, the funds from a short sale are normally held by the lender as security against default. Lenders can take advantage of a favorable market situation by refusing to pass on any of the interest earned by these funds.

6. See Anonymous 2000. As is standard practice, LTCM typically exited a swap position not by negotiating an end to the contract but by entering into a new, equivalent but opposite, swap. As is conventional, swaps are not included in calculations of LTCM's assets as in the previous paragraph.

7. Bank for International Settlements 2004, p. 10, table A2. Many swaps "cancel out" other swaps (see the previous note), and the replacement cost of a swap is usually much less than the notional principal, which does not change hands. So chapter 1's caveats about the way in which total notional amounts overstate the economic importance of derivatives certainly apply to swaps.

8. Total returns are calculated from the data on p. A19 of Perold 1999. Returns net of fees are taken from p. A2 of the same source.

9. Although the founders of LTCM were largely drawn from Salomon, Kaplanis (who was based in London, not, as they had been, in New York) was not part of this group, and indeed differed from it on a number of important issues, such as the applicability of yield curve models based on American experience to the European bond markets (Kaplanis interview).

10. A particular idiosyncrasy identified by LTCM was that restrictions on many Italian banks meant that they had to hold Italian government short-maturity bills, Buoni Ordinari del Tesoro (BOTs), which were therefore anomalously expensive. LTCM expected this difference to reduce in size and entered into swap trades accordingly.

11. In June 2005, the shareholders of the two component companies agreed that the Royal Dutch/Shell group would become a single company, with headquarters in the Netherlands but with its primary listing on the London Stock Exchange.

12. Of course, as noted in the text, the Royal/Dutch Shell trade did not take place in a Modigliani-Miller “perfect market.” Taxes and trading “frictions” are two reasons for the discrepancy in the prices of the two sets of shares.

13. Muehring (1996, p. 81) quotes one portfolio manager as saying “any investor [in LTCM] has to realize they could lose it all.” The same manager, however, also commented: “I would put my money with them.”

14. For an interpretation of the crisis in terms of the “liquidity premium,” see Scholes 2000.

15. In 1996, the restriction was relaxed somewhat to allow phased withdrawal of capital (Perold 1999, p. A14).

16. The process described in this and the following paragraphs has been modeled by Morris and Shin (2004) and Peffer (n.d.). The model of Shleifer and Vishny (1997) also captures a central aspect of the process.

17. Data from Bank of England, *Financial Stability Review* no. 12 (June 2002), p. 73, chart 124.

18. Jorion (2002) notes, for example, that in 1998 commercial banks were not close to minimum capital ratios, and that the latter reflect changing value-at-risk only slowly. That does not, however, rule out the possibility that senior management may view sharp increases in value-at-risk with alarm.

19. Like LTCM, D. E. Shaw and Co. is a hedge fund manager, but with a focus that differs from LTCM’s: it is essentially a statistical arbitrageur (see chapter 4).

20. My source is a file of price data kindly provided by JWM Partners.

21. See also the data on correlations in Committee on the Global Financial System (1999, table 5).

22. My information on the decline in the price of hurricane bonds comes from an interview.

23. Because the market makers’ and clearing firms’ “identifiers” supplied to Cai were encrypted, there is an element of presumption in the identification of the firm involved as Bear Stearns. It seems that market makers may have continued to act in the way described in the text for too long: the trade became loss making after the recapitalization of LTCM (Cai 2003, pp. 4, 23).

24. I am deeply grateful to JWM Partners for the data in tables 8.1 and 8.2, and for the data underlying table 8.3. Thanks, too, to Lang Tran for invaluable assistance in the analysis of the price movements of 1998.

25. The particular bonds in table 8.2 are chosen so as best to represent LTCM’s position in the various markets.

26. For Germany, see the data on ten-year swap spreads in Bank of England *Financial Stability Review*, no. 7 (November 1999), p. 50, chart 57; data for Sweden from JWM Partners.
27. Bank of England, *Financial Stability Review* no. 11 (December 2001), pp. 84–85.

## Chapter 9

1. The view of technological change as a path-dependent cascade has been put forward most influentially by Brian Arthur (e.g. Arthur 1984) and Paul David (e.g. David 1992).
2. Derman was a colleague of Black at Goldman Sachs and an influential Wall Street “quant.” See Derman 2004.
3. See Hagstrom 1965, a classic work on the reward system of science.
4. The following works on economic models more generally have informed the view of finance-theory models put forward here: Earl 1983; Colander 1988; Breslau and Yonay 1999; Yonay and Breslau 2001.
5. “[There] seems little evidence of the ‘publish or perish’ syndrome in this area [finance] before 1960” (Whitley 1986b, p. 173). Regression-based analysis of the determinants of academic economists’ salaries found that in the 1980s “a publication in the top journal was worth an increase in salary of \$1602,” an amount equivalent to roughly 4 percent of the average salary of an academic economist in the period (Colander 1989, p. 143, drawing on Sauer 1998). The top journal identified by Sauer was the *Journal of Political Economy*; other studies suggest that the *American Economic Review* held first rank (Grubel and Boland 1986, p. 425). These data are for U.S. academic economics in general, not financial economics specifically, but I suspect that a similarly strong publication-salary link would be found there too.
6. Edward O. Thorp, email to D. MacKenzie, October 19, 2001.
7. That markets cannot achieve full informational efficiency—that “there is an equilibrium degree of disequilibrium”—is the claim of Grossman and Stiglitz (1980, p. 393), but their work was not orthodox financial economics.
8. John Shelton impressed on me the pervasiveness of this practitioners’ understanding of “beta” in a letter of June 2, 2004, and in a telephone conversation on June 10, 2004. For an example of this understanding, see pp. 40–41 of Eccles, Herz, Keegan, and Phillips 2001.
9. There is a sense in which it was the Capital Asset Pricing Model that “ought” to have inspired index funds (because it posits that the market portfolio is mean-variance efficient), but, as noted in chapter 3, in practice the inspiration tended to be the simpler intuition that “you can’t systematically beat the market.”
10. See Michaud 1989, p. 31. There was, of course, a sense in which the Capital Asset Pricing Model rendered elaborate use of Markowitz’s method unnecessary by positing

that the market portfolio was mean-variance efficient. “The market” thus did all the cumbersome estimation and computation for one, so to speak.

11. Martin 2002. This is, of course, not the only possible explanation for the popularity of stock re-purchases—see Dittmar 2000; Grullon and Ikenberry 2000; Zajac and Westphal 2004; Zuckerman 2004.

12. Bruck 1989, pp. 11, 28; Milken 2000; Hickman 1958; Atkinson 1967. The one piece of academic research by Milken that I have been able to trace is a Wharton School working paper (Walter and Milken 1973), which Celso Brunetti kindly located for me. It discusses capital structure, but does not cite Modigliani and Miller. A representative of Mr. Milken declined my request for an interview with him.

13. *Federal Power Commission et al. vs. Hope Natural Gas Company*, 320 U.S. 591 (1949), as quoted by Breen and Lerner (1972, p. 612).

14. My attention was first drawn to it by an email message from Peter Bailey of the University of Bath School of Management, June 1, 2001.

15. See, for example, Fligstein 1990; Fligstein and Markowitz 1993; Davis, Diekmann, and Tinsley 1994; Zorn and Dobbin 2003; Zorn 2004.

16. The prominent financial economists Michael Jensen and Stephen Ross played leading roles in the development of agency theory. (See, e.g., Jensen and Meckling 1976; Ross 1973.) The theory posited that agents (in this context, managers) could be relied upon only to pursue their own interests, not those of their principals (the stockholders). The solution was thus to design contracts in such a way that managers would maximize their rewards by doing what was in their principals’ interest. Consultants seized on agency theory—in particular on an article in the *Harvard Business Review* (Jensen and Murphy 1990) that called for tighter links between managers’ performance and their rewards—and drew from it the lesson that alignment of interests could be achieved by making a larger proportion of managers’ rewards take the form of the stock of the corporations they managed or options to buy that stock (Lowenstein 2004, pp. 13–21). To the extent that rewards geared to stock prices fueled the managerial deceit and wrongdoing of the 1990s and early 2000s, agency theory might be seen as having a perverse performative effect: forms of reward based on the agency-theory presumption that managers would pursue their own self-interest without scruples may have helped to generate a context in which managers did just that.

17. Weather derivatives are the subject of what promises to be a fascinating Ph.D. thesis by Samuel Randalls of the University of Birmingham.

18. Metallgesellschaft’s hedging program was in principle perfectly feasible, and (as in the case of LTCM) it appears as if the firm’s losses would largely have been temporary had it not liquidated its futures market positions. However, price fluctuations generated large margin calls and cash outlays. These damaged Metallgesellschaft’s credit rating and—under accounting practice in Germany—they had to be reported as huge losses (rather than deferred as they could have been under U.S. “hedge accounting” rules). The large size and predictability of the corporation’s futures market activities also seem to have made those activities vulnerable to exploitation by other market participants. See Marthinsen 2005, pp. 103–125.

19. Miller's own theory of "virtualism" suggests a certain performative power of "economics and other abstract models . . . to transform actual economic practices, making them accord more with these same models," but ultimately he retreats to a sharp distinction between economics as a "culture of representation" and "ordinary economic . . . practice" (D. Miller 2002, pp. 229–230). Miller's comments on the applicability to finance of his theory of "virtualism"—the roots of which lie in his work on consumption—are ambivalent (see D. Miller 1998, p. 210).

20. A landmark article, especially in respect to networks, was Granovetter 1985.

21. Although there are passages such as this in Callon's work that suggest that "framing" and "disentanglement" indeed create *Homo economicus*, other passages are more dialectical, noting the generation by framing and disentanglement of non-calculative overflowing and the "inextricable mixture" of non-calculative and calculative agencies "where we least expect it: at the very heart of the financial institutions" (Callon 1998, p. 40). In his more recent writing (e.g. Callon and Muniesa 2002) the construction of *Homo economicus* is deemphasized, and more dialectical formulations prevail.

22. As was noted in chapter 2, this argument was made most forcefully by Herbert Simon (in, e.g., Simon 1955). For a review of work in this tradition within economics, see Conlisk 1996.

23. See, for example, Shefrin 2000; Shiller 2000, 2002; Shleifer 2000; Thaler 2000; Barberis and Thaler 2002.

24. Although Mirowski is a critic of Callon, both view markets as "a set of diverse imperfectly linked calculative entities" (Mirowski and Nik-Khah 2004, p. 14).

25. A process similar to this is modeled by Liu and Longstaff (2000).

26. See, especially, Shleifer and Vishny 1997. For earlier contributions to this line of work, see De Long, Shleifer, Summers, and Waldman 1990 and Shleifer and Summers 1990. For Shleifer's own comments on LTCM, see Shleifer 2000, pp. 107–111.

27. See Chen, Hong, and Stein 2002. Also see subsequent papers in the same double issue of the *Journal of Financial Economics* (66, no. 2–3).

28. Negative opinions can also be registered by buying puts or selling futures, but this has also encountered barriers. The trading of futures on individual stocks has, for example, only recently become legal in the U.S.

29. See, for example, Partnoy 2003. For a discussion of some of the issues involved, see MacKenzie 2003a.

30. The mechanism seems first to have been suggested by E. M. Miller (1977). For a useful history of the idea, see Rubinstein 2004.

31. Ofek and Richardson (2003) also suggest that the limited amounts of dotcom stocks that could be borrowed made short sales difficult and expensive (as well as being risky, for the reason discussed in the text).

32. For a useful interdisciplinary collection of articles on imitation, see Hurley and Chater 2005.
33. See White 1981, p. 530; White 2001, pp. 139–144.
34. The attitude that short-selling is un-American is nicely parodied in Galbraith 1990.
35. See, for example, R. Miller 2002; McMillan 2002, 2003.

### ***Appendix A***

1. Modigliani and Miller also provided a simplified version of their proof in their 1969 paper. See also Rubinstein 2003.

### ***Appendix B***

1. To avoid confusion, I have made minor alterations (e.g. interchanging letters) to the notation used by the authors whose work is described in this and subsequent appendixes, and have sometimes slightly rearranged the terms in equations.

### ***Appendix D***

1. I have not been able to locate a documentary record of this initial phase of Black's work, so there is an element of inference in my presumption that what Black (1989, p. 5) is describing is what became the "alternative derivation" of his work with Scholes.

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## ***Sources of Unpublished Documents***

Black papers (Fischer Black papers, MC505, Institute Archives and Special Collections, MIT Libraries)

Chicago Mercantile Exchange archive

Fouse papers (private papers of William Fouse, Chairman Emeritus, Mellon Capital Management)

Myers papers (private papers of Stewart Myers, MIT)

Rubinstein papers (private papers of Mark Rubinstein, University of California at Berkeley)

Samuelson papers (private papers of Paul Samuelson, MIT)

Treynor papers (private papers of Jack Treynor, Treynor Capital Management, Inc.)

Items from the Black papers are located by box number and file name. Material from the other sources was provided by Katie Dishman, archivist of the Chicago Mercantile Exchange, by Perry Mehrling, and by the individuals concerned.





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