

THE  
**ALCHEMY**  
OF  
**FINANCE**

READING THE MIND OF THE MARKET

**GEORGE SOROS**

NEW PREFACE • FOREWORD BY PAUL TUDOR JONES II



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**TO SUSAN,**  
*without whom this book would have been ready much sooner*

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## FOREWORD

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Four hundred seventy-three million to one. Those are the odds against George Soros compiling the investment record he did as the manager of the Quantum Fund from 1968 through 1993. His investing record is the most unimpeachable refutation of the random walk hypothesis ever!

As a trader coming of age in the latter half of the frenetic 1970s and the 1980s, *The Alchemy of Finance* was somewhat of a revolutionary book. Remember, this was the period when trend following and indexation were the vogue in investing. It was a time when technical analysis (the study of price movement as a forecasting tool) reached its zenith. Traders of my generation armed themselves with charts and computer-generated graphics that predicted future price direction. We sat day after day in front of screens, mesmerized by blinking lights and everchanging numbers in a deafening cacophony of information overload. With the possible exception of Elliott Wave Theory, an intellectual framework for understanding the course of social, political, and economic events was noticeably forgotten in favor of just making sure that one was part of the ever-quickenning process.

*The Alchemy of Finance* was a shot out of the dark for me. It let me take a giant step forward by first taking a step backwards, clarifying events that appeared so complex and so overwhelming. During an era when so much money was made in larger than life events, from the Hunt brothers' squeeze of the silver market in 1979 to KKR's takeover of RJR Nabisco in 1989, Mr. Soros's theory of reflexivity is the first modern, nontechnical effort to describe

and forecast the dynamic interplay between the participants in the process. That is the brilliance of this book. It describes the dynamics of the path between points of extreme valuation and equilibrium in the marketplace. This is particularly important for the average investor. How many times have we been correctly long near the bottom or short near the top of a major market move? But our staying power with these positions has been weak (as well as our returns) because of a lack of understanding of the path of big price moves. Soros gives us critical insight into that path and thus more confidence in our investments. This constitutes 70% of any successful investing campaign.

When I enter the inevitable losing streak that befalls every investor, I pick up *The Alchemy* and revisit Mr. Soros's campaigns. Studying how he coped with adversity provides an excellent tutorial for breaking the string of negative behaviors that occasionally besets any investor. Winning is infectious. And this book is replete with examples of trading behaviors all would want to emulate. Importantly, Mr. Soros's intellect gives him the confidence and strength of his own convictions to stay with his positions even during trying periods. In that sense, *The Alchemy* joins Edwin Lefèvre's *Reminiscences of a Stock Operator* as a timeless instructional guide of the marketplace. And as such, Soros should beware! In the World War II movie *Patton*, my favorite scene is when U.S. General George S. Patton has just spent weeks studying the writing of his Germany adversary Field Marshall Erwin Rommel and is crushing him in an epic tank battle in Tunisia. Patton, sensing victory as he peers onto the battle field from his command post, growls, "Rommel, you magnificent bastard. I read your *book!*" Enough said.

*The Alchemy* is also an excellent economic and political history of recent times. From unknowingly providing a blueprint as to how the savings and loan fiasco in the United States would be resolved six years in advance (page 124) to predicting the stock market crash of 1987 two years in advance (page 181), Soros reveals himself as the great market visionary of our time.

History will probably remember Mr. Soros as the speculator who tilted against the Bank of England in 1992 (and freed the English people from recession). His billion dollar score is simply too compelling a story for scribes to overlook. Mr. Soros himself would probably like to be remembered as a great economist or even scientist. But I am going to remember him for something even more important, for which he does not receive the credit he deserves. He is

someone who genuinely cares about the state of the human condition and tries to better it. His myriad and monumental philanthropical efforts will qualify him as one of history's great benefactors. Even today at age 62, he pursues the activities of his six foundations with the vigor and work ethic of a young turk on the way up the financial ladder, working 18-hour days around the globe on behalf of his causes. He does not just write checks, which any wealthy person can do. He is a hands-on workaholic who materially impacts the quality of the lives of people less fortunate than he. Now this, this is a sign of greatness.

Paul Tudor Jones II



## PREFACE

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Seven eventful years have passed since *The Alchemy of Finance* was first published. My investment fund, the Quantum Fund, has continued to flourish: Shareholders have enjoyed average annual gains of 35.8% in the last seven years in spite of a setback in the crash of 1987. Quantum Fund has also spawned a number of offspring, some of which are doing even better than the goose that is laying the golden eggs. Starting in 1989, we decided to distribute a portion of our earnings to shareholders, either in cash or in shares of the newly created funds. As a result, we now manage seven funds with combined equity of over \$10 billion.

I have become progressively less active in the management of the funds. I was fortunate in meeting Stanley Druckenmiller through *The Alchemy of Finance*. He was managing another fund at the time, and he sought me out because he was intrigued by my book. We started talking and, eventually, he joined my firm. At the beginning, he found it difficult to work with me. Although I gave him a great deal of authority, he was inhibited by my presence and felt that he was not doing as well as he had before joining my firm. Fortunately, I was becoming increasingly involved in the revolutionary process that led to the collapse of communism. I was establishing a network of foundations throughout the communist world and it involved travelling in places where communications were rather poor. In the summer of 1989, I told Stan that he must take full charge of running the Fund. Since then we have had no difficulties.

I became the coach, and he became the competitor. Our performance improved and we embarked on a period of sustained growth. In each of the last three years, we chalked up gains in excess of

50%. Although we have had two similar periods of prosperity previously, this qualifies as an exceptional performance in view of our outlandish size. Druckenmiller is not only a good fund manager, he is also a good partner. Under his leadership, we have been able to enlarge and improve our management team so that it now has a depth which it never had before. So it happened that I found the reward for my philanthropic activities in the prosperity of my business. That prosperity enabled me to expand the foundation network at a breakneck speed.

My participation in the collapse of communism is a different story which has to be told in a different place. In fact, I have already written two books on the subject, *Opening the Soviet System* in 1990, and *Underwriting Democracy* in 1991. The point that needs to be made here is that I was guided by exactly the same philosophy in my philanthropic activities in Eastern Europe as in the financial markets. As the reader will learn, I treat developments in financial markets as a historical process. That makes my theory eminently applicable to a historical process such as the collapse of communism. I did apply my theory and on the whole it enabled me to anticipate events better than most people. As I discovered, there is a great deal of similarity between a boom-bust process in the financial markets and the rise and fall of the Soviet system.

It is ironic that I became famous, not because of my activities in Eastern Europe, but because of the profit we made on sterling when Britain left the Exchange Rate Mechanism on September 16, 1992. I became an instant celebrity, first in Britain, then in the rest of the world. When it became known that the Quantum group of funds had bought a large block of Newmont Mines, the price of gold soared. Although I expressed no opinion on gold, all kinds of opinions were attributed to me. I made some attempts to rebut them, but to no avail. Although I had not sought guru status, I could not ignore it when it was thrust upon me. In fact, I welcomed it because I thought that it would be useful in having my voice heard on political issues. But that was not as simple as it seemed. When I said that the Bundesbank's high interest rate policy was becoming counterproductive, the markets responded by pushing down the German Mark. But when I inveighed against European policy on Bosnia, I was either ignored or told to stick to the field of my expertise. I fared particularly poorly in France, where I refrained from speculating against the franc because I did not want to be responsible for the collapse of what remained of the European Exchange Rate

Mechanism, but I was blamed for it anyhow. The French government resented my advice even more than it would have resented my speculative activities. It goes to show that speculators ought to speculate and keep their mouths shut.

My notoriety as a financial guru has created a tremendous demand for *The Alchemy of Finance*, hence this new edition. I must confess that my thinking has evolved a great deal since I wrote this book, but I have been concerned mainly with historical processes, not with financial ones. I cannot summarize my ideas in this preface—I need to write another book. I intend to do so as soon as time permits, but there is one important theoretical point I need to make in order to bring this book in line with my current thinking.

In *The Alchemy of Finance*, I put forward the theory of reflexivity as if it were relevant at all times. That is true in the sense that the two-way feedback mechanism that is the hallmark of reflexivity can come into play at any time, but it is not true in the sense that it is at play at all times. In fact, in most situations it is so feeble that it can be safely ignored. We may distinguish between near-equilibrium conditions where certain corrective mechanisms prevent perceptions and reality from drifting too far apart, and far-from-equilibrium conditions where a reflexive double-feedback mechanism is at work and there is no tendency for perceptions and reality to come close together without a significant change in the prevailing conditions, a change of regime. In the first case, classical economic theory applies and the divergence between perceptions and reality can be ignored as mere noise. In the second case, the theory of equilibrium becomes irrelevant and we are confronted with a one-directional historical process where changes in both perceptions and reality are irreversible. It is important to distinguish between these two different states of affairs because what is normal in one is abnormal in the other.

The idea of a distinction between near-equilibrium and far-from-equilibrium conditions is present in *The Alchemy of Finance*. At the end of Chapter 1, I distinguish between humdrum and historical change but I understate the importance of the distinction. I call it "tautological." I now consider this a mistake. The tautology arises only because I do not probe deeply enough and cover up with a tautology what is a fundamental difference in the structure of events.

In most phenomena investigated by scientific method, one set of conditions follows another irrespective of what anybody thinks about them. The phenomena studied by social sciences, which

include the financial markets, have thinking participants and this complicates matters. As I have tried to show, the participants' views are inherently biased. Instead of a direct line leading from one set of conditions to the next one, there is a constant criss-crossing between the objective, observable conditions and the participant's observations and vice versa: participants base their decisions not on objective conditions but on their interpretation of those conditions. This is an important point and it has far-reaching consequences. It introduces an element of indeterminacy which renders the subject matter less amenable to the kind of generalizations, predictions, and explanations that have given natural science its reputation. Exactly because it is so disruptive, the social sciences in general and economic theory in particular have done their best to eliminate or to ignore the element of indeterminacy. I have taken issue with that endeavor and tried to develop an alternative approach which takes the participants' bias as its starting point.

In retrospect, I may have overstated my case. There are many situations that can be fruitfully studied by taking the participants' bias as given and ignoring the element of indeterminacy which it may generate. It is only in certain respects and in certain special circumstances that the indeterminacy becomes significant. It comes into play when expectations about the future have a bearing on present behavior—which is the case in financial markets. But even there, some mechanism must be triggered for the participants' bias to affect not only market prices but the so-called fundamentals which are supposed to determine market prices. Apparently I have failed to make this point sufficiently clear. The message of my book is usually summed up by saying that the participants' value judgments are always biased and the prevailing bias affects market prices. If that is all I had to say it would be hardly worth writing a book about it. My point is that there are occasions when the bias affects not only market prices but also the so-called fundamentals. This is when reflexivity becomes important. It does not happen all the time but when it does, market prices follow a different pattern. They also play a different role: they do not merely reflect the so-called fundamentals; they themselves become one of the fundamentals which shape the evolution of prices. This recursive relationship renders the evolution of prices indeterminate and the so-called equilibrium price irrelevant.

Nobody would deny that individual participants operate with biased views; but the prevailing wisdom holds that the participants'

bias can be dismissed as temporary aberrations, so-called random walks. That is the point on which I disagree. I now believe this point can be more effectively made by drawing a distinction between near-equilibrium and far-from-equilibrium conditions than by proposing a general theory of history based on the constant cross-crossing between perceptions and reality as I have done in *The Alchemy of Finance*. That does not mean that there is anything wrong with the general theory; it means only that the concept of reflexivity becomes more significant if it is reserved for those cases where the double feedback mechanism is actually at work.

*The Alchemy of Finance* is devoted to the study of such cases. The most obvious example is equity leveraging where a temporary overvaluation of shares is converted into per-share earnings through the issue of shares at inflated prices. In most of the cases discussed, the participants' bias involves an actual error in their thinking. For instance, in the late 1970s international bankers lent too much money to developing countries because they failed to recognize that the so-called debt ratios they used to measure the creditworthiness of the borrowing countries were reflexive in the sense that they were affected by their own lending activity. But it is not necessary for the bias to involve an actual error. As I show in Chapter 3, a freely fluctuating exchange rate system is inherently unstable because of the influence of trend-following speculation, yet speculators follow the correct strategy by following the trend.

Judging by the public reaction—which consists mainly of comments by journalists who read the book superficially or not at all—I have not been successful in demonstrating the significance of reflexivity. Only the first part of my argument—that the prevailing bias affects market prices—seems to have registered. The second part—that the prevailing bias can in certain circumstances also affect the so-called fundamentals and changes in market prices cause changes in market prices—seems to have gone unnoticed.

The fault is at least partially mine. Since reflexivity changes the structure of events, I have tried to put forward a reflexive structure as the universally valid way of looking at the evolution of market prices—a kind of general theory à la Keynes in which the absence of reflexivity constitutes a special case. It would have been better to present reflexivity as the special case because what endows reflexivity with significance is the fact that it operates intermittently.

Once the significance of reflexivity has sunk in and the inadequacy of the prevailing wisdom has been recognized, the time would have been ripe for proposing a general theory of reflexivity.

I have my excuses. I did not observe reflexivity in financial markets but developed reflexivity as an abstract philosophical concept before I entered the financial markets. In other words, I failed as a philosophical speculator before I succeeded as a financial one. Apparently, my failure as a philosopher carried over into my book because I did not make the concept of reflexivity—which can be observed and converted into profit—as clear as it could be. When one discovers something new, one has an understandable inclination to exaggerate its importance. This is what I did with reflexivity. By proposing a general theory of reflexivity, I may have gone too far too soon. I claimed that economic theory is false and social science is a false metaphor. These are exaggerated claims. Since far-from-equilibrium conditions arise only intermittently, economic theory is only intermittently false. And the dividing line between natural and social science is not quite as hard and fast as I made it appear when I wrote the book. These qualifications render reflexivity more rather than less significant.

Once the concept of reflexivity is established, the range of its applicability seems to widen. It is possible to treat the evolution of prices in all financial markets taken together as a reflexive, historical process. I have done so in *The Alchemy of Finance* when I analyzed Reagan's "Imperial Circle," and I have found other examples since the book was published, such as the German Imperial Circle after the fall of the Berlin Wall. (See appendix: "The Prospect of European Disintegration.") But there is a danger in pushing the concept of reflexivity too far, as I have learned at my own expense. There are long fallow periods when the movements in financial markets do not seem to follow a reflexive tune but rather resemble the random walks mandated by the efficient market theory. In these circumstances, it is better to do nothing than to pursue a reflexive hypothesis.

Treating reflexivity as an intermittent phenomenon rather than as a universally valid condition opens up fertile fields for investigation. For instance, the question poses itself: How can near- and far-from-equilibrium conditions be distinguished from each other? What is the criterion of demarcation? I have done a lot of thinking on that question and I have the beginnings of an answer. Whether

I can formulate it properly remains to be seen in my next book. It revolves around a question of values and it is relevant for society in general, not only for financial markets. My next book, if it is ever written, will be a theory of history, not a theory of finance. I am providing an example of how the boom-bust pattern of financial markets can be applied to larger historical processes in the appendix where I reproduce a lecture I delivered on September 29, 1993, entitled "Prospect for European Disintegration."

## INTRODUCTION

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In a very real sense, this book is my life's work. It touches on many of my most abiding interests and it brings together the two main strands in my intellectual development: one abstract and one practical.

The abstract came first. Ever since I became conscious of my existence I have had a passionate interest in understanding it, and I regarded my own understanding as the central problem that needed to be understood. To understand oneself—*gnote aucton; nosce te ipsum*—is an impossible task. To achieve anything resembling knowledge we must be able to draw a distinction between subject and object; yet in this case the two are the same. What one thinks is part of what one thinks about; therefore, one's thinking lacks an independent point of reference by which it can be judged—it lacks objectivity.

As an undergraduate I studied economics, but I found economic theory highly unsatisfactory because it failed to come to grips with this problem; indeed, it went through great contortions to avoid it. Economics seeks to be a science. Science is supposed to be objective and it is difficult to be scientific when the subject matter, the participant in the economic process, lacks objectivity.

I was greatly influenced at the time by Karl Popper's ideas on scientific method. I accepted most of his views, with one major exception. He argued in favor of what he called "unity of method"<sup>1</sup>—that is, the methods and criteria that apply to the study of natural phenomena also apply to the study of social events. I felt that there was a fundamental difference between the



two: the events studied by the social sciences have thinking participants; natural phenomena do not. The participants' thinking creates problems that have no counterpart in natural science. The closest analogy is in quantum physics, where scientific observation gives rise to Heisenberg's uncertainty principle; but in social events it is the participants' thinking that is responsible for the element of uncertainty, not the outside observer.

Natural science studies events that consist of a sequence of facts. When events have thinking participants, the subject matter is no longer confined to facts but also includes the participants' perceptions. The chain of causation does not lead directly from fact to fact but from fact to perception and from perception to fact. This would not create any insuperable difficulties if there were some kind of correspondence or equivalence between facts and perceptions. Unfortunately, that is impossible because the participants' perceptions do not relate to facts, but to a situation that is contingent on their own perceptions and therefore cannot be treated as a fact.

Economic theory tries to sidestep the issue by introducing the assumption of rational behavior. People are assumed to act by choosing the best of the available alternatives, but somehow the distinction between perceived alternatives and facts is assumed away. The result is a theoretical construction of great elegance that resembles natural science but does not resemble reality. It relates to an ideal world in which participants act on the basis of perfect knowledge and it produces a theoretical equilibrium in which the allocation of resources is at an optimum. It has little relevance to the real world in which people act on the basis of imperfect understanding and equilibrium is beyond reach.

The relationship between the participants' understanding and the situation in which they participate continued to preoccupy me long after I left college. My first priority was to try and make a living but in my spare time I wrote a philosophical treatise on the subject with the catchy title "The Burden of Consciousness." Unfortunately, the title was the best part of it. By the time I finished, I disagreed with my own presentation. I spent three years revising it. One day I reread what I had written the day before and I could not make head or tail of it. It made me realize that I had reached a dead end, and I decided to give it up. That was when the practical streak in me began to dominate my intellectual development.

If I had to sum up my practical skills, I would use one word: survival. When I was an adolescent, the Second World War gave

me a lesson that I have never forgotten. I was fortunate enough to have a father who was highly skilled in the art of survival, having lived through the Russian revolution as an escaped prisoner of war. Under his tutelage the Second World War served as an advanced course at a tender age. As the reader shall see, the investment vehicle I created a quarter of a century later drew heavily on skills I learned as an adolescent.

After leaving college I had a number of false starts and finally became an international arbitrage trader in stocks, first in London and then in New York. When the European Common Market was formed in 1957, American investors became interested in European shares. I became a security analyst advising American institutions on their European investments and for a brief period I ruled as a one-eyed king among the blind. My glory came to an abrupt end when President Kennedy introduced a so-called interest equalization tax which effectively stopped purchases of foreign securities. I decided to put my money-making activities on the back burner and spent three years, from 1963 to 1966, revising "The Burden of Consciousness."

When I finally decided to return to the land of the living I started a model portfolio that became a hedge fund (a mutual fund that employs leverage and uses various techniques of hedging) in 1969. I have been in charge of that fund ever since, although I delegated much of the responsibility to others between September 1981 and September 1984. The fund has grown from about \$4 million at inception to nearly \$2 billion and most of the growth has been internally generated. Original investors have seen the value of their shares multiply 300-fold. No investment fund has ever produced comparable results.

In the first ten years of my business career I had not much use for anything I had learned in college and there was an almost total separation between my practical activities and my theoretical interests. Selling and trading in securities was a game I played without putting my true self on the line.

All this changed when I became a fund manager. I was putting my money where my mouth was and I could not afford to dissociate myself from my investment decisions. I had to use all my intellectual resources and I discovered, to my great surprise and gratification, that my abstract ideas came in very handy. It would be an exaggeration to say that they accounted for my success; but there can be no doubt that they gave me an edge.

I developed my own peculiar approach to investing, which was

at loggerheads with the prevailing wisdom. The generally accepted view is that markets are always right—that is, market prices tend to discount future developments accurately even when it is unclear what those developments are. I start with the opposite point of view. I believe that market prices are always wrong in the sense that they present a biased view of the future. But distortion works in both directions: not only do market participants operate with a bias, but their bias can also influence the course of events. This may create the impression that markets anticipate future developments accurately, but in fact it is not present expectations that correspond to future events but future events that are shaped by present expectations. The participants' perceptions are inherently flawed, and there is a two-way connection between flawed perceptions and the actual course of events, which results in a lack of correspondence between the two. I call this two-way connection "reflexivity."

In the course of my investment activities, I discovered that financial markets operate on a principle that is somehow akin to scientific method. Making an investment decision is like formulating a scientific hypothesis and submitting it to a practical test. The main difference is that the hypothesis that underlies an investment decision is intended to make money and not to establish a universally valid generalization. Both activities involve significant risk, and success brings a corresponding reward—monetary in one case and scientific in the other. Taking this view, it is possible to see financial markets as a laboratory for testing hypotheses, albeit not strictly scientific ones. The truth is, successful investing is a kind of alchemy.

Most market participants do not view markets in this light. That means that they do not know what hypotheses are being tested; it also means that most of the hypotheses that are submitted to market testing are quite banal. Usually they amount to nothing more than the assertion that a particular stock is going to outperform the market averages.

I had a certain advantage over other investors because at least I had an idea about the way financial markets operate. I would be lying, however, if I claimed that I could always formulate worthwhile hypotheses on the basis of my theoretical framework. Sometimes there were no reflexive processes to be found; sometimes I failed to find them; and, what was the most painful of all, sometimes I got them wrong. One way or another, I often invested

without a worthwhile hypothesis and my activities were not very different from a random walk. But I was attuned to reflexive processes in financial markets and my major successes came from exploiting the opportunities they presented.

My approach to the market was not as abstract as it sounds. It took an intensely personal, emotional form: testing was closely associated with pain and success with relief. When I asserted that "markets are always biased" I was giving expression to a deeply felt attitude: I had a very low regard for the sagacity of professional investors and the more influential their position the less I considered them capable of making the right decisions. My partner and I took a malicious pleasure in making money by selling short stocks that were institutional favorites. But we differed in our attitudes to our own activities. He regarded only the other participants' views as flawed, while I thought that we had as good a chance of being wrong as anyone else. The assumption of inherently flawed perceptions suited my self-critical attitude.

Operating a hedge fund utilized my training in survival to the fullest. Using leverage can produce superior results when the going is good, but it can wipe you out when events fail to conform to your expectations. One of the hardest things to judge is what level of risk is safe. There are no universally valid yardsticks: each situation needs to be judged on its own merit. In the final analysis you must rely on your instincts for survival. Thus my engagement in running a hedge fund brought together both my abstract interests and my practical skills.

I did not play the financial markets according to a particular set of rules; I was always more interested in understanding the changes that occur in the rules of the game. I started with hypotheses relating to individual companies; with the passage of time my interests veered increasingly toward macroeconomic themes. This was due partly to the growth of the fund and partly to the growing instability of the macroeconomic environment. For instance, exchange rates were fixed until 1973; subsequently, they became a fertile field for speculation.

For the past four or five years I have had a growing sense of impending financial disaster. I felt that a long-lasting expansionary cycle was becoming increasingly unsound and unsustainable and we were getting ready for a bust. That was one of the reasons I distanced myself from the active management of the fund in 1981 and reduced its overall level of exposure. My interest shifted

from my own survival to the survival of the system. I made a study of the international debt problem and published some papers on the subject. I used the same theoretical framework as in my investment activities and my analysis was not without merit. Unfortunately, the more complex the system, the greater the room for error. I made some mistakes in my analysis that detracted from the accuracy of my predictions; they also had a detrimental effect on my investment results until I revised my views in the course of writing this book.

The more successful I had been in applying my ideas in financial markets, the keener I became to express them in theoretical form. I continued to cherish the fantasy that the concept of reflexivity constitutes a major contribution to our understanding of the world in which we live. I believed that the participants' bias is the key to an understanding of all historical processes that have thinking participants, just as genetic mutation is the key to biological evolution. But a satisfactory formulation of the theory of reflexivity continued to elude me. I always ran into trouble when I tried to define what I meant by the imperfect understanding of the participants. To speak accurately of a distortion we must know what the situation would be if it were not distorted by the participants' perceptions. Unfortunately that does not seem possible because the participants' thinking is an integral part of the situation they have to think about. It is not surprising that the concept of reflexivity should present extreme difficulties; if it were an easier concept to work with, economists and other social scientists would not have gone to such lengths to banish it from their subject matter.

This book is a final attempt to explore the implications of reflexivity. I have tried to circumvent the difficulties I encountered in the past by approaching the subject from the opposite direction. Instead of getting bogged down in abstract theory, I am going to draw on my experimental, practical findings to the greatest possible extent. I cannot avoid an abstract discussion altogether, but I have confined it to a single chapter. In exploring the practical implications, I start with the simplest cases and gradually lead up to more complex ones. This approach happens to coincide with the historical order in which I encountered reflexive developments in practice: first the stock market, then the currency market, then the international debt problem, and finally what may be called the credit cycle.

The stock market provides some pure examples of a boom and bust pattern; freely floating currency rates allow me to explore well-formed wave patterns. The boom and bust in international bank lending is part of a more complex, historical process of credit expansion and eventual credit contraction. It has given rise to the configuration that I have dubbed "Reagan's Imperial Circle." The configuration prevailed from the international debt crisis of 1982 until the first half of 1985 but it was inherently unstable. How the instability will be resolved is one of the main questions considered in this book.

The experimental approach has borne unexpected results. I made two major discoveries in the course of writing: one is a reflexive connection between credit and collateral; the other is a reflexive relationship between the regulators and the economy they regulate.

It has long been assumed that monetary values are a passive reflection of the state of affairs in the real world. Classical economics focused on the real world and neglected the problems connected with money and credit; even Keynes couched his general theory in real terms. Monetarists sought to stand the relationship on its head: they argue that it is possible to control inflation by controlling the growth of the money supply.

In my opinion, all these views are based on a fundamental misconception. Money values do not simply mirror the state of affairs in the real world; valuation is a positive act that makes an impact on the course of events. Monetary and real phenomena are connected in a reflexive fashion; that is, they influence each other mutually. The reflexive relationship manifests itself most clearly in the use and abuse of credit.

Loans are based on the lender's estimation of the borrower's ability to service his debt. The valuation of the collateral is supposed to be independent of the act of lending; but in actual fact the act of lending can affect the value of the collateral. This is true of the individual case and of the economy as a whole. Credit expansion stimulates the economy and enhances collateral values; the repayment or contraction of credit has a depressing influence both on the economy and on the valuation of the collateral. The connection between credit and economic activity is anything but constant—for instance, credit for building a new factory has quite a different effect from credit for a leveraged buyout. This makes it difficult to quantify the connection between credit and

economic activity. Yet it is a mistake to ignore it. The monetarist school has done so, with disastrous consequences.

The reflexive interaction between the act of lending and collateral values has led me to postulate a pattern in which a period of gradual, slowly accelerating credit expansion is followed by a short period of credit contraction—the classic sequence of boom and bust. The bust is compressed in time because the attempt to liquidate loans causes a sudden implosion of collateral values.

Economic history has been punctuated by booms and busts. Nevertheless, the concept of a credit cycle is too simplistic to explain the course of events. For one thing, the connection between credit and economic activity is too tenuous and variable to yield a regular pattern. For another, the sequence of events is greatly complicated by the influence of economic policy. Periodic busts have been so devastating that strenuous efforts have been made to prevent them. These efforts have led to the evolution of central banking and of other mechanisms for controlling credit and regulating economic activity.

To understand the role of the regulators it must be realized that they are also participants: their understanding is inherently imperfect and their actions have unintended consequences. The relationship between the regulators and the economy is reflexive and it also exhibits cyclical characteristics in the sense that it tends to swing from one extreme to another.

What is the connection between the regulatory cycle and the credit cycle? At this point, my views become very tentative. I believe that the two cycles broadly overlap in time, that the minimum of regulations tends to coincide with the maximum of credit expansion and vice versa. But within this chronological coincidence there is constant interaction between the two cycles that influences the shape and duration of both. The interaction between the two cycles yields a unique path that cannot be fitted into any regular or repetitive pattern.

I have tried to apply this complex and tentative framework to an interpretation of recent economic and financial history. Needless to say, a great many factors come into play; but my focus is on the twin cycles in credit and regulation. The main topics I deal with are the transformation of banking from a highly regulated to a less regulated industry, the boom and bust in international lending, mergermania, and international capital movements.

Until 1982, the story is a fairly straightforward case of boom

and bust, but after 1982 the situation gets very complicated. If events had been allowed to take their course, the uncontrolled credit expansion of the 1970s would have come to an unhappy end; but exactly because the consequences would have been so disastrous, the financial authorities came to the rescue and successfully avoided a bust. Since then, we have been passing through uncharted waters. The great boom exhausted itself some time ago but its life span has been extended by artificial means in order to avoid a great bust.

I try to trace the unique path that events have taken: the preservation of the accumulated burden of bad international debt through the formation of what I call the "Collective" system of lending and the emergence of the United States government as the "borrower of last resort." Both of these are unprecedented developments. They gave rise to this strange constellation that I have called the Imperial Circle: a benign circle at the center and a vicious circle at the periphery of a worldwide system based on a strong dollar, a strong U.S. economy, a growing budget deficit, a growing trade deficit, and high real interest rates. The Imperial Circle held the international economic and financial system together but it was inherently unstable because the strong dollar and high real interest rates were bound to outweigh the stimulating effect of the budget deficit and weaken the U.S. economy. The Imperial Circle could not last indefinitely. What would happen next?

To answer that question, I conducted an experiment from August 1985 onward. In effect, I kept a diary in which I recorded the thoughts that went into my investment decisions on a real-time basis. Since I considered the future of the Imperial Circle of paramount importance, the experiment served as a test of my ability to predict the future course of events, using the framework developed in the book. The experiment was a roaring success in financial terms—my fund never did better. It also produced a surprising result: I came out of the experiment with quite different expectations about the future.

I started with the presumption that the benign circle was in danger of reversing itself: a weak dollar and a weak economy would combine to keep interest rates higher than they ought to be, and without any scope for further monetary or fiscal stimulus the decline of both the economy and the dollar would become irreversible. But the situation was once again saved by the inter-



vention of the monetary authorities. By changing from a system of freely floating exchange rates to a "dirty float," the decline of the dollar was cushioned, and, with the help of lower interest rates and booming financial markets, the economy was prevented from slipping into recession. We entered a new phase which I describe, with only a modicum of irony, as the "Golden Age of Capitalism."

It can be seen that this book seeks to accomplish a number of things at the same time. It propounds not just one general theory—the theory of reflexivity—but also another specific theory, that of a credit-cum-regulatory cycle. The latter idea is so tentative that it hardly qualifies as a theory. Yet I try to apply it not only to explain contemporary history but also to predict the outcome, and in the course of doing so I provide a practical demonstration of the fundamental difference between explaining and predicting reflexive phenomena. I also try to draw some general conclusions from the analysis. The most important ones are, first, that it is credit that matters, not money (in other words, monetarism is a false ideology), and, second, that the concept of a general equilibrium has no relevance to the real world (in other words, classical economics is an exercise in futility). Financial markets are inherently unstable; that leads to a third conclusion that is better stated as a question than an assertion: what policy measures are needed to reestablish the stability of our economic system?

The book would be easier to read if it tried to make just one point at a time. Unfortunately, that is not possible, because the various points are interconnected. If the theory of reflexivity were widely known, I could have taken it for granted and concentrated on an exploration of the credit and regulatory cycles. Similarly, if the fact that financial markets are inherently unstable were generally recognized, I need not have spent so much time on demonstrating that the concept of equilibrium as used in economic theory is totally unrealistic. As it is, I had to try to make several points more or less simultaneously.

To make matters worse, the book does not qualify as a finished product. When I started writing it, I thought I had a theory of reflexivity to present and my difficulties related only to its presentation. As I tried to apply the theory to various situations, I discovered that I do not actually have a well-formed theory. The idea that the participants' biases play an important causal role in historical events is both valid and interesting, but it is too general to

qualify as a theory that can help to explain and predict the course of events. The boom/bust pattern I have established applies to some developments but not to others. To try to fit every initially self-reinforcing and eventually self-defeating development into its mold can give rise to serious distortions. I feel like the early astronomers who tried to describe the elliptical paths of the planets in terms of circles and semicircles; the only difference is that the path of reflexive events is irregular to start with.

My fantasy was to present a general theory of reflexivity that would explain the great bust of the 1980s in the same way that Keynes's *General Theory of Employment, Interest and Money* explained the Great Depression of the 1930s. As it turns out, we do not have a great bust and I do not have a general theory. What I have is an approach that can help to illuminate the present precarious state of our financial system. It cannot explain and predict the course of events in the manner to which we have become accustomed during our long love affair with natural science for the simple reason that reflexive processes cannot be explained and predicted in that manner. A different approach is needed and this book is an attempt to develop one. It is best regarded as part of a process of discovery rather than the finished product.

All this makes for a difficult, dense book, although I can promise the reader that nothing in the rest of the book is quite as dense as this introduction. I explore a complex subject. I bring a complex mind to bear on it. I can argue in my defense that the complexity of my thinking mirrors the complexity of the financial markets rather well, as demonstrated by the financial outcome of the real-time experiment. There is, therefore, at least a *prima facie* case for giving me a hearing. I shall try not to abuse the privilege.

It may be helpful if I sketch out the structure of the book. Part I propounds the theory. The first chapter deals with the concept of reflexivity in general terms and explores the difficulties in understanding reflexive phenomena. In particular, it argues that the symmetry between explanation and prediction that characterizes the laws of natural science is not attainable. The next three chapters apply the theory to the financial markets: Chapter 2 to the stock market and Chapter 3 to the currency market, with Chapter 4 outlining a credit and regulatory cycle.

Part II seeks to explain contemporary economic and financial history using the hypotheses outlined in Chapter 4. The history

is, of necessity, selective, concentrating on those developments that are relevant to the concept of a credit and regulatory cycle. My main topics are banking, international lending, and mergermania.

Part III consists of a real-time experiment which is both test and prediction at the same time. As a test it does not qualify as a scientific one by the standards of natural science; but it may serve as an example of how theories about reflexive developments can be tested.

Part IV evaluates the results of the real-time experiment. Chapter 15 explores the scope for what I provocatively call alchemy. The real-time experiment can be regarded as an exercise in financial alchemy. Chapter 16 examines the limitations of social science.

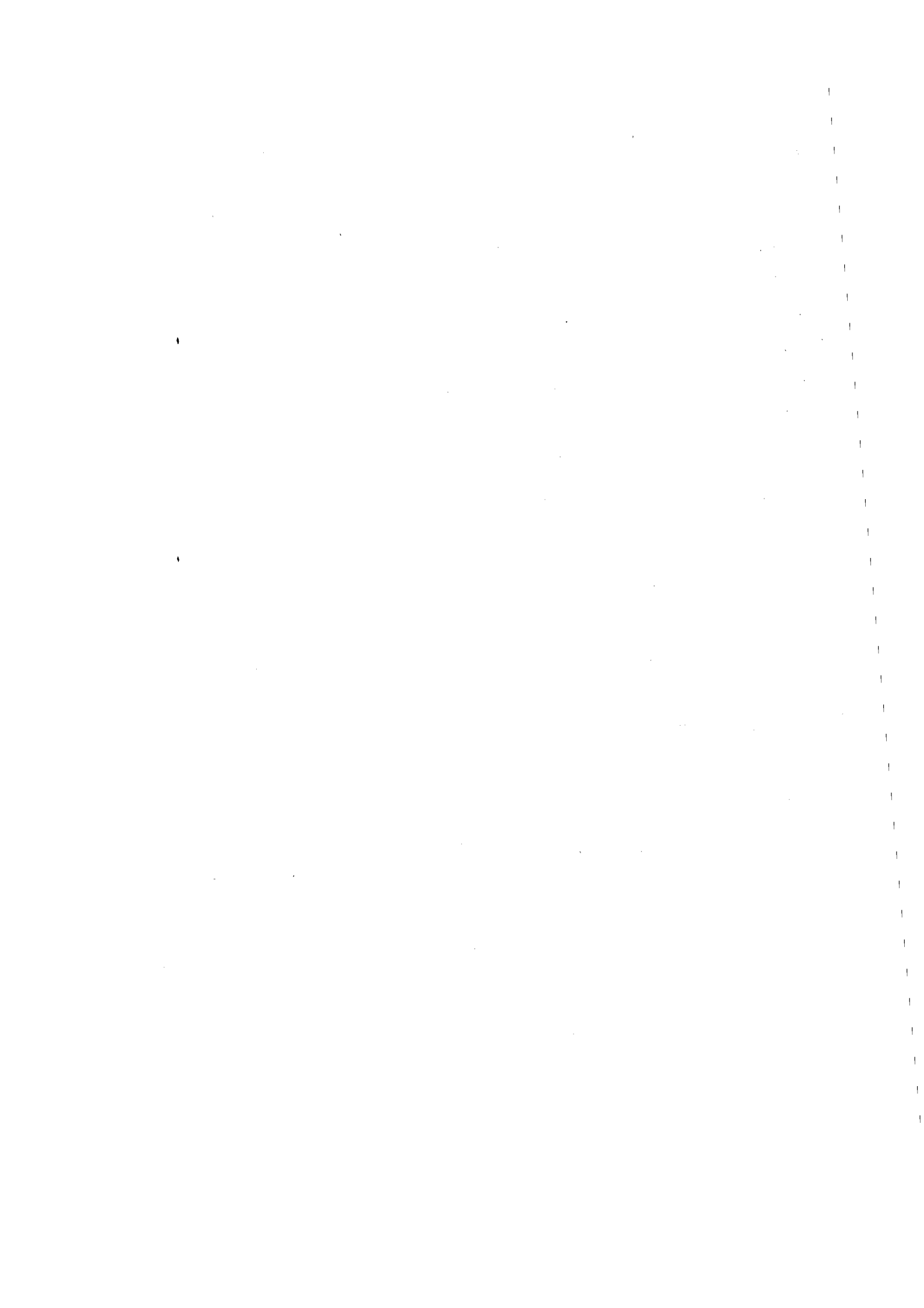
Part V seeks to provide some prescriptions for economic policy. Chapter 17 examines the relative merits of free markets and regulation, and Chapter 18 argues in favor of an international central bank. Since both the market mechanism and the attempts to regulate it are inherently flawed, it may be argued that all attempts at systemic reform are doomed to failure. I reject that argument in Chapter 19.

In the Epilogue I explore the implications of the concept of reflexivity outside the sphere of finance and in a final flight of fancy I attempt to provide my own answers to some age-old metaphysical questions.

Since my thinking has evolved in the course of writing, it may be helpful for the reader to know when the various chapters were written. The first part of the book was completed before I embarked on the real-time experiment in August 1985. Moreover, Chapters 5–9, which deal with recent history, preceded in time Chapter 4, which outlines the concept of the credit and regulatory cycle. Chapter 4 incorporates discoveries I made in the course of writing; that is why it is so tentative in character.

I should like to emphasize that this book is not meant to provide a practical guide to getting rich in the stock market. Most of what I know is in the book, at least in theoretical form. I have not kept anything deliberately hidden. But the chain of reasoning operates in the opposite direction: I am not trying to explain how to use my approach to make money; rather, I am using my experiences in the financial markets to develop an approach to the

study of historical processes in general and the present historical moment in particular. If I did not believe that my investment activities can serve that purpose, I would not want to write about them. As long as I am actively engaged in business, I would be better off to keep them a trade secret. But I would value it much more highly than any business success if I could contribute to an understanding of the world in which we live or, better yet, if I could help to preserve the economic and political system that has allowed me to flourish as a participant.



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PART ONE

THEORY



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## THE THEORY OF REFLEXIVITY

### **Anti-equilibrium**

Economic theory is devoted to the study of equilibrium positions. The concept of an equilibrium is very useful. It allows us to focus on the final outcome rather than on the process that leads up to it. But the concept is also very deceptive. It has the aura of something empirical: since the adjustment process is supposed to lead to an equilibrium, an equilibrium position seems somehow implicit in our observations. That is not true. Equilibrium itself has rarely been observed in real life—market prices have a notorious habit of fluctuating. The process that can be observed is supposed to move toward an equilibrium. Why is it that the equilibrium is never reached? It is true that market participants adjust to market prices but they may be adjusting to a constantly moving target. In that case, calling the participants' behavior an adjustment process may be a misnomer and equilibrium theory becomes irrelevant to the real world.

Equilibrium is the product of an axiomatic system. Economic theory is constructed like logic or mathematics: it is based on certain postulates and all of its conclusions are derived from them by logical manipulation. The possibility that equilibrium is never reached need not invalidate the logical construction, but when a hypothetical equilibrium is presented as a model of reality a significant distortion is introduced. If we lived in a world in which the angles of a triangle did not add up to 180 degrees, Euclidean geometry would constitute such a misleading model.



The crowning achievement of the axiomatic approach is the theory of perfect competition. Although it was first propounded nearly two hundred years ago, it has never been superseded; only the method of analysis has been refined. The theory holds that under certain specified circumstances the unrestrained pursuit of self-interest leads to the optimum allocation of resources. The equilibrium point is reached when each firm produces at a level where its marginal cost equals the market price and each consumer buys an amount whose marginal "utility" equals the market price. Analysis shows that the equilibrium position maximizes the benefit of all participants, provided no individual buyer or seller can influence market prices. It is this line of argument that has served as the theoretical underpinning for the laissez-faire policies of the nineteenth century, and it is also the basis of the current belief in the "magic of the marketplace."

Let us examine the main assumptions of the theory of perfect competition. Those that are spelled out include perfect knowledge; homogeneous and divisible products; and a large enough number of participants so that no single participant can influence the market price.

The assumption of perfect knowledge is suspect because understanding a situation in which one participates cannot qualify as knowledge. That was the assumption that I found so unacceptable as a student. I have no doubt that classical economists used the assumption in exactly that sense in which I found it objectionable because nineteenth-century thinkers were less aware of the limitations of knowledge than we are today. As the epistemological problems began to surface, exponents of the theory found that they could get by using a more modest word: information. In its modern formulation the theory merely postulates perfect information.<sup>1</sup>

Unfortunately, this assumption is not quite sufficient to support the construction of the theory. To make up for the deficiency, modern economists resorted to an ingenious device: they insisted that the demand and supply curves should be taken as given. They did not present this as a postulate; rather, they based their claim on methodological grounds. They argued that the task of economics is to study the relationship between supply and demand and not either by itself. Demand may be a suitable subject for psychologists, supply may be the province of engineers or management scientists; both are beyond the scope of economics.<sup>2</sup> Therefore, both must be taken as given.

Yet, if we stop to ask what it means that the conditions of supply and demand are independently given, it is clear that an additional assumption has been introduced. Otherwise, where would those curves come from? We are dealing with an assumption disguised as a methodological device. Participants are supposed to choose between alternatives in accordance with their scale of preferences. The unspoken assumption is that the participants know what those preferences and alternatives are.

As I shall try to show, this assumption is untenable. The shape of the supply and demand curves cannot be taken as independently given, because both of them incorporate the participants' expectations about events that are shaped by their own expectations.

Nowhere is the role of expectations more clearly visible than in financial markets. Buy and sell decisions are based on expectations about future prices, and future prices, in turn, are contingent on present buy and sell decisions. To speak of supply and demand as if they were determined by forces that are independent of the market participants' expectations is quite misleading. The situation is not quite so clear-cut in the case of commodities, where supply is largely dependent on production and demand on consumption. But the price that determines the amounts produced and consumed is not necessarily the present price. On the contrary, market participants are more likely to be guided by future prices, either as expressed in futures markets or as anticipated by themselves. In either case, it is inappropriate to speak of independently given supply and demand curves because both curves incorporate the participants' expectations about future prices.

The very idea that events in the marketplace may affect the shape of the demand and supply curves seems incongruous to those who have been reared on classical economics. The demand and supply curves are supposed to determine the market price. If they were themselves subject to market influences, prices would cease to be uniquely determined. Instead of equilibrium, we would be left with fluctuating prices. This would be a devastating state of affairs. All the conclusions of economic theory would lose their relevance to the real world.

It is to prevent this outcome that the methodological device that treats the supply and demand curves as independently given was introduced. Yet there is something insidious about using a meth-

odological device to obscure an assumption that would be untenable if it were spelled out. To preserve the integrity of economic theory as an axiomatic system, its assumptions ought to be explicitly stated. We may then conclude that economic theory is no more relevant to the real world than non-Euclidean geometry, but at least we would know where we stand. Instead, we have been deceived by a methodological subterfuge. The demand and supply curves are presented in textbooks as though they were grounded in empirical evidence. But there is scant evidence for independently given demand and supply curves. Anyone who trades in markets where prices are continuously changing knows that participants are very much influenced by market developments. Rising prices often attract buyers and vice versa. How could self-reinforcing trends persist if supply and demand curves were independent of market prices? Yet, even a cursory look at commodity, stock, and currency markets confirms that trends are the rule rather than the exception.

The theory of perfect competition could be defended by arguing that the trends we can observe in commodity and financial markets are merely temporary aberrations which will be eliminated in the long run by the "fundamental" forces of supply and demand. It should be remembered that the theory of perfect competition does not claim to define the path of adjustment; it merely analyzes the situation after all the adjustments have taken place. The trouble with the argument is that there can be no assurance that "fundamental" forces will correct "speculative" excesses. It is just as possible that speculation will alter the supposedly fundamental conditions of supply and demand.

In the normal course of events, a speculative price rise provokes countervailing forces: supply is increased and demand reduced, and the temporary excess is corrected with the passage of time. But there are exceptions. In foreign exchange, for example, a sustained price movement can be self-validating, because of its impact on domestic price levels. The same is true in the stock market where the performance of a stock may affect the performance of the company in question in a number of ways. And in examining the recent history of international lending we shall find that excessive lending first increased the borrowing capacity of debtor countries, as measured by their debt ratios, and then, when the banks wanted to be repaid, the debtor countries' ability to do so evaporated. Generally speaking, we shall find that the expansion

and contraction of credit can affect the debtors' ability and willingness to pay.

Are these exceptions that confirm the rule, or do they necessitate a revision of accepted theory? The answer depends on the frequency and severity of their occurrence. If we are dealing with an isolated instance, we can treat it as a paradox; but if one incident follows another, we must question the theory.

I contend that such paradoxical behavior is typical of all financial markets that serve as a discounting mechanism for future developments, notably stock markets, foreign exchange markets, banking, and all forms of credit. Microeconomic theory may continue to ignore it, because there are large areas of economic activity where it occurs only occasionally or not at all; but we cannot expect to understand macroeconomic developments without taking the phenomenon into account. A world of fluctuating exchange rates and large-scale capital movements is characterized by vicious and benign circles in which the "normal" pattern of causation, as defined by classical economics, seems to be reversed: market developments dictate the evolution of the conditions of supply and demand, not the other way around.

If the process of adjustment does not lead to an equilibrium, what happens to the conclusions of economic theory? The answer is that they remain valid as deductions but they lose their relevance to the real world. If we want to understand the real world, we must divert our gaze from a hypothetical final outcome and concentrate our attention on the process of change that we can observe all around us.

This will require a radical shift in our thinking. A process of change is much more difficult to understand than a static equilibrium. We shall have to revise many of our preconceived ideas about the kind of understanding that is attainable and satisfy ourselves with conclusions that are far less definite than those that economic theory sought to provide.

### **The Problem of Imperfect Understanding**

The understanding of the actual course of events, as distinct from a hypothetical equilibrium, poses problems that have not been properly appreciated. The problems arise because participants

base their decisions on an inherently imperfect understanding of the situation in which they participate. There are two related sets of problems to be considered: the imperfect understanding of the participant and the imperfect understanding of the social scientist. We must be careful not to confuse the two.

In this section I shall try to explain why the participants' understanding is inherently imperfect. In the next section I shall examine why the imperfect understanding of the participants poses difficulties for the social sciences.

The imperfect understanding of the participant is a difficult concept to define and an even more difficult one to work with. I shall try to approach it by comparing the position of the participant with that of a natural scientist. (I have to specify a natural scientist, because social scientists are confronted with special problems arising out of the imperfect understanding of participants. These will be dealt with in the next section.) The purpose of the comparison is to establish a standard in terms of which the understanding of the participant can be called imperfect. What makes the comparison tricky is that the natural scientist is not capable of perfect understanding either. Far from it. As Karl Popper has shown,<sup>3</sup> it is a cardinal principle of scientific method that perfect knowledge is not attainable. Scientists work by constantly testing plausible hypotheses and propounding new ones. If they did not treat all conclusions as provisional and subject to improvement, natural science could not have reached its present state of development and it could not progress any further. Although it is far from perfect, the knowledge attained by natural scientists provides a standard in terms of which the participants' understanding can be called imperfect.

Natural scientists have one great advantage over participants: they deal with phenomena that occur independently of what anybody says or thinks about them. The phenomena belong to one universe, the scientists' statements to another. The phenomena then serve as an independent, objective criterion by which the truth or validity of scientific statements can be judged. Statements that correspond to the facts are true; those that do not are false. To the extent that the correspondence can be established, the scientist's understanding qualifies as knowledge. We do not need to go into the various difficulties that stand in the way of establishing the correspondence. The important point is that scientists have an objective criterion at their disposal.

By contrast, the situation to which the participants' thinking relates is not independently given: it is contingent on their own decisions. As an objective criterion for establishing the truth or validity of the participants' views, it is deficient. It does provide a criterion of sorts: some expectations are validated by subsequent events, others are not. But the process of validation leaves something to be desired: one can never be sure whether it is the expectation that corresponds to the subsequent event or the subsequent event that conforms to the expectation. The segregation between thoughts and events that prevails in natural science is simply missing.

Thinking plays a dual role. On the one hand, participants seek to understand the situation in which they participate; on the other, their understanding serves as the basis of decisions which influence the course of events. The two roles interfere with each other. Neither role is performed as well as it could be if it were performed separately. If the course of events were independent of the participants' decisions, the participants' understanding could equal that of a natural scientist; and if participants could base their decisions on knowledge, however provisional, the results of their actions would have a better chance of corresponding to their intentions. As it is, participants act on the basis of imperfect understanding and the course of events bears the imprint of that imperfection.

In a milder form, the lack of separation between the subject matter and the act of thinking may also arise in natural science. The most celebrated example is in quantum physics where the act of observation interferes with the observed phenomenon. It has given rise to Heisenberg's uncertainty principle which, in effect, establishes a limit to the scientist's ability to attain knowledge. But in natural science the problem occurs only at the limit, whereas for the participant it is at the very center of his thinking. For one thing, the scientist makes a deliberate attempt not to interfere with his subject matter, whereas the participant's primary purpose is to mold the situation in which he participates to his own satisfaction. More important, in quantum physics it is only the act of observation which interferes with the subject matter, not the theory of uncertainty, whereas in the case of thinking participants their own thoughts form part of the subject matter to which they relate. The positive accomplishments of natural science are confined to the area where thinking and events are effec-

tively segregated. When events have thinking participants that area shrinks to the vanishing point.

### The Problem of the Social Sciences

We are now in a position to examine the problems of the social sciences. Again, there are two distinct issues to be considered. One relates to the subject matter, the other to the observer.

Scientific method is designed to deal with facts; but, as we have seen, events which have thinking participants do not consist of facts alone. The participant's thinking plays a causal role, yet it does not correspond to the facts for the simple reason that it does not relate to facts. Participants have to deal with a situation that is contingent on their own decisions; their thinking constitutes an indispensable ingredient in that situation. Whether we treat it as a fact of a special kind or something other than a fact, the participants' thinking introduces an element of uncertainty into the subject matter. This element is absent in the natural sciences. As we have seen, there is some similarity between the uncertainty introduced by the participants' thinking and Heisenberg's uncertainty principle in quantum physics but, as we shall soon see, the parallel is misleading.

Now for the role of the scientific observer: it is much easier to maintain the required segregation between facts and statements when the subject matter itself does not contain any statements, observations, or thoughts of any kind.

Most discussions about the shortcomings of the social sciences have focused on the second issue. Expressions like "self-fulfilling prophecies" or "self-defeating experiments" are widely used but usually they relate to the would-be scientist. Yet it is the self-influencing character of the participants' thinking that is responsible for the element of uncertainty (or indeterminacy) I mentioned before. The difficulties of scientific observation pale into insignificance when compared with the indeterminacy of the subject matter. The indeterminacy would remain even if all the problems relating to the observer were resolved, whereas the problems of the observer can be directly attributed to the indeterminacy of the subject matter. Thus the problem of the social sciences is not merely methodological but inherent in the subject matter.

The undue emphasis on the role of the scientific observer can

be attributed to the false analogy with Heisenberg's uncertainty principle. I am no expert in quantum physics but, as I understand it, the principle holds that the mass and velocity of quantum particles cannot be measured at the same time because the act of measurement interferes with the object that is being measured. In this case, the element of uncertainty is introduced by the outside observer. (Whether the behavior of quantum particles is inherently random is a separate question.) The parallel with the social sciences is misleading because in the latter case the indeterminacy (uncertainty) is caused by the participants. Only if quantum particles behaved as thinking participants would the analogy hold.

I shall try to reverse the discussion to its proper order: the uncertainty in the subject matter first, the role of the scientist second.

The idea that there is a fundamental difference in the subject matter of the natural and social sciences has not been generally recognized. On the contrary, Karl Popper, whom I otherwise admire, enunciated what he called the "doctrine of the unity of science";<sup>4</sup> that is, the same methods and criteria apply in both the natural and social sciences. Although it has not been universally accepted, it has not been conclusively refuted either. I shall try to do so here.

In order to appreciate the problem posed by thinking participants, let us take a closer look at the way scientific method operates. For this purpose I am invoking Karl Popper's scheme of scientific method, described in technical terms as the "deductivonomological," or "D-N," model. Like every model, it presents a simplified and idealized version of a more complex reality, but exactly because it is simple and elegant it suits my purpose very well.

The model is built on three kinds of statements: specific initial conditions, specific final conditions, and generalizations of universal validity. Combining a set of generalizations with known initial conditions yields predictions; combining them with known final conditions provides explanations; and matching known initial with known final conditions serves as testing for the generalizations involved. It can be seen that there is a symmetry between predictions and explanations; they are logically reversible. Testing is different, because no amount of testing can prove that a generalization is universally valid. Scientific theories