W.E. UPJOHN INSTITUTE FOR EMPLOYMENT RESEARCH

1-1-1989

The State of Economic Science: Views of Six Nobel Laureates

Werner Sichel
Western Michigan University

Citation

Sichel Werner, ed. 1989. The State of Economic Science: Views of Six Nobel Laureates. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

The STATE of ECONOMIC SCIENCE

Views of Six Nobel Laureates

Kenneth Arrow
1972 Recipient

James Buchanan
1986 Recipient

Lawrence Klein
1980 Recipient

Herbert Simon
1978 Recipient

Robert Solow
1987 Recipient

James Tobin
1981 Recipient

Werner Sichel
Editor

The STATE of ECONOMIC SCIENCE

Views of Six Nobel Laureates

Kenneth Arrow
1972 Recipient

Herbert Simon
1978 Recipient

James Buchanan
1986 Recipient

Robert Solow
1987 Recipient

Lawrence Klein
1980 Recipient

James Tobin
1981 Recipient

Werner Sichel
Editor

1989

W. E. UPJOHN INSTITUTE for Employment Research
Kalamazoo, Michigan

Library of Congress Cataloging-in-Publication Data

The State of economic science : views of six nobel laureates / edited
by Werner Sichel.
p. cm.
Includes bibliographical references.
ISBN 0-88099-084-8. — ISBN 0-88099-083-X (pbk.)
1. Economics—History—20th century. I. Sichel, Werner.
II. W.E. Upjohn Institute for Employment Research.
HB87.S64 1989
330'.09'04—dc20 89-27443
CIP

THE INSTITUTE, a nonprofit research organization was established on July 1, 1945. It is an activity of the W. E. Upjohn Unemployment Trustee Corporation, which was formed in 1932 to administer a fund set aside by the late Dr. W. E. Upjohn for the purpose of carrying on "research into the causes and effects of unemployment and measures for the alleviation of unemployment."

Cover design by J.R. Underhill

To Diether H. Haenicke

for his unfailing support

PREFACE

The essays in this book are based on lectures presented at Western Michigan University during the academic year, 1988-89. The occasion was the 25th anniversary of an annual lecture series under the direction of the faculty of the Department of Economics at Western Michigan University.

For each of the preceding 24 years, the Department had invited six distinguished economists to discuss a particular topic. These topics ranged from "Freedom and Capitalism" (1963-64) to "The Economics of Environmental Problems" (1971-72) to "The Economics of International Migration" (1984-85). In order to properly celebrate the 25-year milestone, it seemed fitting to extend ourselves to examine the state of the entire discipline and to invite the most noted economists of our time—those who have been awarded the Nobel Prize in Economics—to do the examining.

There are a number of people to whom I am very grateful. First, to the Nobel Laureates who were willing to participate. Second, to my colleagues at the W. E. Upjohn Institute for Employment Research and especially to its director, Robert Spiegelman, for generous support. Third, to my "bosses" at the University, including President Diether H. Haenicke (to whom this volume is dedicated), Provost George M. Dennison, and Deans A. Bruce Clarke and David O. Lyon. And finally, but certainly not least, to my colleagues in the Economics Department, especially my co-authors in another venture, Martin Bronfenbrenner (visiting professor) and Wayland Gardner, who have taught me a great deal of macroeconomics, and Myron H. Ross and Raymond E. Zelder, who served on my committee.

Werner Sichel
June 1989
Kalamazoo, Michigan

CONTENTS

WERNER SICHEL is Professor of Economics and Chair of the Department of Economics at Western Michigan University. His field of specialty is industrial organization.

Professor Sichel earned a B.S. degree from New York University and M.A. and Ph.D. degrees from Northwestern University. In 1968–69 he was a Fulbright-Hays senior lecturer at the University of Belgrade in Yugoslavia and in 1984–85 was a Visiting Scholar at the Hoover Institution, Stanford University.

Dr. Sichel is a past president of the Economics Society of Michigan and is the current president of the Midwest Business Economics Association. For the past decade he has served as a consultant to a major law firm with regard to antitrust litigation. He presently serves on the Editorial Advisory Board of the *Antitrust Law and Economics Review.*

Professor Sichel has published a number of articles in scholarly journals and books and is the author or editor of about 15 books. His books include a popular principles text, *Economics,* coauthored with Martin Bronfenbrenner and Wayland Gardner; *Basic Economic Concepts,* coauthored with Peter Eckstein, which has been translated into Spanish and Chinese and is currently the most widely used "Western Economics" text in China, and *Economics Journals and Serials: An Analytical Guide,* coauthored with his wife, Beatrice. He has edited three books in the field of industrial organization, *Industrial Organization and Public Policy, Antitrust Policy and Economic Welfare,* and *The Economic Effects of Multinational Corporations.* He has also edited *Economic Advice and Executive Policy: Recommendations from Past Members of the Council of Economic Advisers* plus five different books in the area of public utility regulation.

Dr. Werner Sichel
Professor of Economics
Department of Economics
Western Michigan University

Introduction

Each year the President of the United States presents a "State of the Union" message. In it the President outlines accomplishments and challenges. While not completely analogous, we believe that the same approach, perhaps not on an annual basis, is appropriate with regard to a discipline. Economics is a particularly good candidate. It is a discipline very often maligned ("If you would lay economists end to end, they still wouldn't reach a conclusion.") yet simultaneously held in high esteem and accorded respect, as evidenced by how frequently economists are requested to advise government officials or business executives and how often they are sought for interviews on radio and TV and for writing opinion columns and editorials in newspapers and magazines.

What is the state of economic science as we begin the 1990s? Obviously, this is a normative question. It involves a value judgment. Whom shall we ask? Many people have an opinion. But who can provide the most authoritative answers? We believe that the respondents must be professional economists—practitioners rather than just observers. And who among economists? We suggest that they should be economists in that small select group who have been honored by being chosen to receive the top prize in economics—Nobel Laureates in economics.

The Alfred Nobel Memorial Prize in Economics is a "Johnny-come-lately." It was added nearly 70 years after the prizes in chemistry, literature, medicine or physiology, peace, and physics. The first one, shared by Ragnar Frisch of Norway and Jan Tinbergen of the Netherlands, was awarded in 1969. To date, only 26 economists have received the award. Fifteen of these were Americans. According to the rules established by the Central Bank of Sweden, the benefactor of the economics award, "The Prize shall be awarded annually to a person who has carried out a work in economic science of the eminent significance expressed in the Will of Alfred Nobel."

Having come to the conclusion that we wanted Nobel Laureate economists to present State of Economic Science messages, our next task was to attract six of them to do so. For pragmatic reasons, we limited our invitations to Americans. We were delighted with the results. Our invitations were graciously accepted by Kenneth J. Arrow, Robert M. Solow, Lawrence R. Klein, James Tobin, James M. Buchanan, and Herbert A. Simon. We believe they are an outstanding group, not only because of their eminence, but also because they provide a good representation of the spectrum of economic thought.

As most readers will correctly predict, the essays by Robert Solow, Lawrence Klein and James Tobin address the state of macroeconomics, while Kenneth Arrow, James Buchanan and Herbert Simon make more of an attempt to cover all of economics.

Kenneth J. Arrow, the recipient of the 1972 Nobel Prize in Economics, has for the past 35 years focused on and made major contributions to the theories of individual and social choice and general economic equilibrium. As the first speaker in the series and author of the first essay in this volume, he devotes some space to his perception of what economics is and does—studying the relation between the individual and the system as it pertains to exchange and the transformation of resources and products.

Arrow enumerates what he considers to be the three most significant developments in economics during the last 50 years: (1) the greater recognition of the importance of the time dimension in economic behavior; (2) the related appreciation of the need to study human behavior under conditions of uncertainty; and (3) the understanding that information and knowledge are significant economic variables.

According to Arrow, economic science now recognizes that a different set of expectations (whether rational or not) about the future leads to a different present world, but he laments that as yet there are few markets established for future purchases and sales. Economic science has introduced uncertainty—a consequence of being concerned about the future—into its theories. This has strengthened the concept of rationality because it tells us more about the economic behavior of actors to whom economists assign probability distributions to possible outcomes. Arrow, who defines "information" as any observation that changes a person's probability judgment, points out that economists must concern themselves with the consequences of informational inequalities.

Arrow sides with fellow Nobel Laureate, Herbert Simon (the final speaker in this series), in his admiration for the work of cognitive psychologists who have studied the abilities of humans to make rational choices. Arrow sees value for economics in their findings of systematic biases of a nonrational nature stemming from overconfidence, framing, and exaggerated response to information. He closes on the note that economic science, with the support of psychology and of computer science, faces the challenge of knowing better "how we come to acquire knowledge and form beliefs and how we act and can act on that knowledge."

The second essay is by Robert M. Solow, a much more recent (1987) recipient of the Nobel Prize. He is best known for his development of the theoretical foundation as well as the empirical measurement and estimation of the effect of technological change on output.

Solow asserts that "macroeconomics is what it is all about"—a view clearly shared by Klein and Tobin, whose essays follow. Solow provides an excellent historical review of macroeconomic thought, beginning with pre-Keynes business cycle theory and ending with new-classical theory on the one hand and with what he calls new American-Keynesian theory on the other. In between, of course, came the contributions by

Keynes himself, as well as those by monetarists, expectationists, and supply-siders.

In Solow's view, the new-classical approach of "grounding macroeconomic models in complete individual-agent-based microeconomic theory . . . has been a blind alley in practice . . ." with "no great empirical or predictive successes." Instead, he believes, economists ought to study the "macro foundations of microeconomics." (Buchanan—the author of the fifth essay in this volume—would take issue with that.) Solow finds that at least one group of new-Keynesians is making progress in this direction. This group places emphasis on imperfect competition, increasing returns to scale and trading externalities and concludes that the economy may be capable of *many* "self-sustaining equilibria" rather than just a single price-mediated market-clearing equilibrium. Solow sees macroeconomic science as in a state not yet fit for empirical application, but one where some economists are at least seeking to find the mechanisms that cause the economic system to malfunction. If they succeed, it may then be possible to formulate policies that would be beneficial. He opposes what he perceives to be the new-classical view—that the market failures in question are "mere aberrations" of the system. Solow maintains that they indeed *are* the system. What's wrong, Solow asks, with having a number of useful little models in the macroeconomist's arsenal and then choosing the relevant one for the particular condition that the economy is in? That is, of course, a rhetorical question. That is the sort of macroeconomic development to which he looks forward.

The third essay is written by Lawrence R. Klein, recipient of the 1980 Nobel Prize. He is the founder of Wharton Econometric Forecasting Associates and has long been considered the chief architect of the large econometric forecasting model.

Klein discusses the state of economic science as it pertains to his specialty, macroeconometrics—"the use of econometrics for the study of the macroeconomy." Klein's views concerning the history of macroeconomics—the contributions made by the various schools of thought and what has worked and what has not—differ little from those of Solow. Klein too recognizes Keynes as the great innovator, but admits that the Keynesian model has benefited from later contributors who have helped beef up the supply side and the financial sector. Monetarism, in his view, has not passed the econometric test. And as concerns the new classical macroeconomics, he acknowledges that macro theory may be constructed as a system built upon an aggregation of microeconomic relationships, but insists that macro relationships have "a life of their own" and can be specified directly. Once they are, data can be applied, the system can be estimated, and it can be used for policy analysis.

Klein disapproves of what he sees as a trend toward using smaller—more compact and simplified—econometric models. Instead of using "the smallest system that is capable of explaining the facts of life," he favors using "the largest system that can be managed."

Klein acknowledges that forecasting is difficult, but contends that it cannot and should not be avoided. Reaction is too slow to be effective. He is somewhat optimistic that macroeconometric forecasting will improve—but does not anticipate much help from those who rely on own-generated expectations. He sees the most promise in the use of "high-frequency" data to help adjust quarterly econometric models.

Klein ends his essay on the state of macroeconometrics on a mixed note. On the one hand, he is gratified to see some models, particularly the two-gap model, employed successfully in a number of centrally planned and/or developing economies. On the other hand, he sees no major breakthroughs "in the vast volume of research material that is being published" and finds that what is most popular with today's bright young scholars is really quite sterile.

The fourth essay is by James Tobin, recipient of the 1981 Nobel Prize in Economics. During the past 45 years, Tobin has focused on and made major contributions in the fields of macroeconomic theory, monetary theory and policy, portfolio theory, economic growth, and consumer behavior.

Tobin is very much in the same camp as Solow and Klein. He too is only interested in discussing the state of macroeconomics and concludes that Keynes is really not vulnerable to the attacks by the new-classicals who appear to prefer to deal with a caricature of the Keynesian theory of business fluctuations than with what Keynes actually professed and wrote.

Tobin focuses on what he calls the fundamental issue in macroeconomics—"the existence, reliability, strength, and speed of adjustments by which a market economy maintains or restores economywide equilibrium" between the supply and demand for labor and what it produces. He explains that Keynes did not assume wage rigidity, but rather only that workers are concerned with relative wage parity. They very much resist nominal wage cuts, but are willing to accept real wage cuts in the form of price increases.

Furthermore, Tobin argues that contemporary "anti-Keynesian 'New-Classical' counterrevolutionaries" do not understand that even if wages and prices were flexible, there would still be unemployment in the presence of inadequate real demand. Flexible prices do not fully absorb demand shocks instantaneously.

Tobin is concerned that the new-classicals have made macroeconomics a "babble of parables" and that their stories in many respects do not resemble the real world. He considers microeconomics to be "a framework of analysis" rather than "a source of specific conclusions about the signs and magnitudes of relationships among economic variables." Tobin, perhaps more than any of the other contributors to this volume, observes a worrisome state of macroeconomic science: "There is a big gulf between academic macroeconomics and the macroeconomics oriented to contemporary events and policies."

The fifth essay in this volume is contributed by James M. Buchanan, the 1986 recipient of the Nobel Prize. He is the modern developer of the theory of public choice and has made major contributions to the development of the contractual and constitutional bases for the theory of political decisionmaking and public economics.

Buchanan's essay differs from all the others in this volume, but most especially from the three (Solow, Klein, Tobin) that precede it. Not only does Buchanan not address the state of macroeconomic science, he asserts that there is no place for macroeconomics, either as a part of positive or normative economics. He considers Keynesian-inspired macroeconomics to be a "monumental misdirection of scientific effort" since it largely ignores the structure of the economy. In many respects, Buchanan also disagrees with Arrow because Buchanan rejects the commonly used methodology of maximizing objective functions subject to particular constraints. He believes that economists have been wasting their time focusing on scarcity, choice and value maximization. Buchanan also does not share many of Simon's ideas, but there does appear to be one important commonality between the two—both consider themselves "outsider" economists. In fact, one gets the impression that they enjoy that status.

Buchanan characterizes himself as a methodological and normative individualist, a radical subjectivist, a contractarian, and a constitutionalist. He views the economy as an order—a constitutional order. Thus he sees voluntary exchange as mutually utility-enhancing, since it is based upon agreement between the parties engaged in the exchange. Furthermore, he contends that, as an order, the economy enables performance to be evaluated in terms of results that are conceptually a part of the behavior of individuals acting within the order itself.

The state of economic science is not to Buchanan's liking. He would prefer to see economics (as a social science) concentrate on the topic of trade or exchange and the institutions that effect trade, such as contracts and "the whole realm of collective agreement on the constitutional rules of political society."

The final essay in this volume is contributed by Herbert A. Simon, who received the Nobel Prize in 1978. During the past 30 years he has focused on decisionmaking and problemsolving processes, using computers to simulate human thinking.

Simon's essay deals with the state of the methodology employed in economic science. He perceives it to be in great need of reform and makes a number of important observations and concrete suggestions. Simon has no doubt about economics being a science. He believes that the profession's poor economic forecasting record is not an indication of its unscientific nature. He generalizes that one should be wary of using prediction as a test of science since an understanding of mechanisms does not guarantee predictability.

Simon observes that economists agree a great deal more than is apparent to the general public. After all, most economists subscribe to a central core of theory, and even more important, to a way of reasoning about economic questions. The problem, he suggests, is that economists disagree about "auxiliary assumptions" concerning matters such as what information people have and how they deal with uncertainty. Simon strongly counsels that economists need to vigorously test their auxiliary hypotheses. He warns against "theory without measurement," and urges economists to grub for facts, to worry less about predictability and more about whether their assumptions are correct (a view opposite to that presented by fellow Nobel Laureate Milton Friedman in his well-known 1953 essay, "The Methodology of Positive Economics"), and to work with less aggregated data so that there is a better fit between theory and data.

As noted earlier in this introduction, Simon and Arrow both contend that economists should pay closer attention to the work done by cognitive psychologists who have developed both (1) a large body of empirically tested theory about decisionmaking and problemsolving, and (2) some techniques that use computers to simulate complex human thought processes.

Simon recommends that economists conduct more laboratory experiments and field studies and, in that context, learn how to obtain data about beliefs, attitudes and expectations. He is fairly confident that the reform he calls for will be forthcoming since "the inability of economics today to play the policy role to which it aspires is a major source of pressure toward reform."

In this introduction we have attempted to provide the reader with "coming attractions" that whet the appetite. Each of the six essays that follow deserves careful reading. While brief, this collection is packed with ideas and insights accumulated over many years by six of the most outstanding twentieth century scholars in economic science.

KENNETH J. ARROW is Consultant to the Rand Corporation and Senior Fellow by Courtesy of the Hoover Institution at Stanford University. During the past thirty-five years he has focused on and made major contributions to the theories of individual and social choice and of general economic equilibrium.

Professor Arrow earned a B.S. degree in Social Science from City University of New York and M.A. and Ph.D. degrees from Columbia University. He has been awarded more than a dozen honorary degrees from U.S. and foreign universities including Harvard, Chicago, Columbia, Yale, Pennsylvania, City University of New York, Hebrew University, and Cambridge. Professor Arrow taught for one year at the University of Chicago before accepting an appointment at Stanford University. Later, he taught for eleven years at Harvard University before returning to Stanford. He has also served as a visiting professor at MIT, Cambridge University, the Institute for Advanced Study in Vienna, and the European University Institute.

Dr. Arrow is a past president of the American Economic Association, the Econometric Society, the Institute of Management Sciences, the Western Economic Association, the International Economic Association and the International Society for Inventory Research. In 1957, he was awarded the John Bates Clark Medal by the American Economic Association. He is a member of the National Academy of Sciences, the American Philosophical Society, and the Institute of Medicine. He is also a Fellow of the American Academy of Arts and Sciences, the Institute of Mathematical Statistics, the American Statistical Association, and the American Association for the Advancement of Science. He recently received the von Neumann Prize of the Institute of Management Sciences and the Operations Research Society of America.

Professor Arrow is the author of about 20 books and over 165 articles in scholarly journals and books. Titles of Dr. Arrow's books that are indicative of his work include: *Social Choice and Individual Values, Studies in Linear and Non-Linear Programming, Aspects of the Theory of Risk-Bearing, General Competitive Analysis, The Limits of Organization,* and *Social Choice and Multicriterion Decision Making.* In addition, Harvard University Press has published six volumes of *Collected Papers of Kenneth J. Arrow.*

1

Dr. Kenneth J. Arrow
Joan Kenney Professor of Economics and Professor of Operations Research
Stanford University
Recipient of the 1972 Nobel Prize in Economics

In the context of a modern economy, economic science tries to explain what and how much all of us buy and sell, what prices we pay and receive, and the effects of taxes and expenditures by the government. These few words contain a tremendous variety of activities and phenomena. We humans buy and sell not only chickens, which yield immediate satisfaction, but also factories and machines, which yield outputs and revenues in the future. We buy and sell such sophisticated commodities as securities, i.e., bonds and stocks. These are obligations to pay and receive amounts of money (itself a stage removed from goods in a simple sense) at points in the future and, in the case of stocks, the amounts are not even prescribed but depend on future events and decisions not completely predictable at the time of the securities transfer. The prices we receive or pay include such abstractions as a rate of interest.

This is a very incomplete survey of the subject matter that economics seeks to explain. Economics is the attempt to systematize all these phenomena, to find underlying regularities and patterns in the relations among the prices and quantities it studies. It seeks to understand the basic motives that guide the economic agents in their decisions, and it tries to draw the implications of these motives for the evolution of prices and quantities. It is fair to say that economics was given its present orientation by Adam Smith, whose book, *An Enquiry Concerning the Wealth of Nations*, was published in 1776—a date otherwise of considerable significance. It was his great insight that there is a mutual interaction between the workings of the economic system and the actions of every individual. The overall magnitudes, totals sold and bought, prices paid and received, are the result of the actions of individuals, but in turn the system magnitudes control the actions of individuals. Even though everyone is trying to act to his or her own benefit, the results may correspond to no one's intent. For example, a firm will seek to maximize its profits (that is, make them as large as possible) taking as given the prices it pays for inputs and receives for outputs. But competition among many firms, each maximizing profits, results in minimum profits. The elaboration of the interconnectedness of the economy and the reciprocity between the system and the individual has been a fine example of international scientific intercourse over more than two centuries, as what the Scotsman Smith introduced was further developed by the Frenchman, Leon Walras, and the Italian, Vilfredo Pareto.

The economic system performs several functions, but the one that is most stressed by modern economists is the allocation of resources. Goods flow from place to place. They start from farm or mine through various stages of transformation to end in the hands of their ultimate consumers. Where and to whom they go and what processes are performed are the result of myriad individual economic decisions all profoundly influenced by the conditions of the market. Goods will not be produced if the prices received do not cover the costs of production or if there is no one who will buy them.

What is remarkable about the process by which the market system allocates resources is that it requires surprisingly little knowledge of the entire system by any one individual. The seller need only know there is a buyer willing to pay a suitable price and does not need to know (and usually does not know) why the buyer wants the product, whether to resell or to use in further production. This economy of vision is not merely the product of a modern complex society. In Athens of the 5th century B.C., Herodotus, the "Father of History" and certainly an unusually well-informed man of his day, did not know the ultimate source of the tin which the Greeks imported to make bronze. All he knew was that the Greek settlers in what is now Marseilles bought tin from merchants who brought it down the Rhone. We know now that it ultimately came from Britain, a land whose very existence had only legendary meaning to Herodotus.

As economists see it, the chief coordinating instruments are prices. A price is an incentive to sellers or producers and a penalty to buyers. If there is a serious imbalance between supply and demand, prices rise or fall to bring the two into balance. Thus, if there is more supply than demand at a given price, competition among sellers brings prices down, both reducing supply and increasing demand. An *equilibrium* is a set of prices for which the corresponding inducements to sellers and buyers lead to equality of supply and demand on each market. The price system explains the limited need for knowledge by any one participant; it is necessary to know only the prices of the commodities in which he or she deals.

Economists differ as to the degree to which equilibria are actually attained. Some argue that the economy is very nearly in equilibrium all of the time; others, like myself, point to recurrent unemployment and to the nonexistence of markets for future sale and delivery as serious deficiencies in the equilibrium account of the economic world. But all agree that the tendencies toward equilibrium are real and important.

With this sketch of what economics is all about, let me turn to what I regard as the most significant and indeed dramatic developments of the last 50 years: the fuller and deeper exploration of the time dimension in economic behavior, the importance of uncertainty, and the recognition of information and knowledge as significant economic variables. These are interrelated developments, as we shall see.

Exchange and transformation of goods are the key economic phenomena, as has been emphasized. But these can occur not only over space or across industries but also over time. Individuals live for extended periods of time. They have concern, not only for their own futures but for their children and others beyond their own lifetime. Production takes time. The farmer plants first and harvests only after a period of time. Factory production also takes time. The production of a given commodity is not only a time-consuming process but also requires the use of instruments, whether machines or buildings, which wear out only gradually and over very long periods of time. Inventories are essential for the smooth running of production and have to be held for some period of time, though usually brief. Wine must be held for several years to achieve its full potential, and the greater the wine the more it will improve with age. Electric power generating plants have effective lifetimes of 30 years and more; dams for power or for irrigation may be useful for a century.

The choices of the amounts and durabilities of investment projects are based on two values. One is the individual desire to protect oneself for the future, that is, to save; the other is the usefulness of investment in creating new valuable products in the more or less distant future. The saver will use the money not expended on current consumption to purchase bonds or stocks, whose sale finances investment. More fundamentally, we can see that for the economy as a whole the resources diverted from consumption by the individuals' desire to save are made available for investment.

How are these desires on the parts of different sectors of the economy coordinated or, in technical language, brought into equilibrium? There are now two periods in which transactions must take place, in the present and in the future, when the product resulting from the investment becomes available. Today we have current markets for bonds or securities or other instruments of saving, and these operate like other markets. There are prices for these securities; in the case of bonds, the price effectively determines the rate of interest. But in the future, there is no such simple step. The product to be produced has to find a market. But there is not today any market for future sales and purchases of goods, with some few exceptions, and therefore no prices for them.

The profitability of an investment, therefore, can never be calculated from market data at the time of investment. Future prices are "expected" or "anticipated." We move from the concrete world of markets and market prices to a less solid realm of expectation. Now forming expectations and acting on them are surely among the most characteristic of human actions. Shakespeare put this observation in the mouth of that most reflective character Hamlet: "Sure, he that made us with such large discourse,/ Looking *before* and after, gave us not/ That capability and god-like reason/ To fust in us unused." (*Hamlet,* Act IV, Scene 4). It is not only future prices that one must look "before"; more generally, the future conditions under which production and consumption will take place must be anticipated, for they are not currently known. Family size, market conditions, innovations whether to facilitate one's productivity or to create competitors, and weather conditions are among the innumerable conditions which will shape our decisions tomorrow and which therefore affect today's decisions to save or to invest.

The image or expectation of the future, therefore, shapes the present. What we expect for the future will affect the amount we wish to save and how we distribute that amount among alternative ways of saving. What we expect for the future will also determine the directions in which we plan investments and therefore take at least the first steps to embody our plans in concrete and metal. But these decisions have concrete effects today. A high willingness to save reduces current consumption. It may cause workers, therefore, to shift from consumption goods industries to investment goods industries, from garments to construction. Workers may have to shift geographically to implement the interindustrial shift. A different set of expectations about the future leads to a different present world.

To digress for a minute, the principle here goes beyond economics. Images of future peace or war determine our present armaments and military preparations and may indeed lead to war or peace now. Expectations of good or bad future lives can influence our present attitude towards life, towards having children, towards developing or not developing social and cultural skills.

The elaboration of this picture of the economic world, in which the anticipations of the future affect the present and, of course, our present actions in investment and savings, help to determine the future, which I take to be perhaps the leading development in economic theory and analysis in the last 50 or 60 years. Let me mention by name the great pioneers of the 1930s, Ragnar Frisch, of Norway, and John R. Hicks, who died at the time these words were being written. These names are not household words like John Maynard Keynes, yet I would hold that their works are even today more influential in the practice of economics. Subsequent elaboration has resulted in an increasingly sophisticated set of models of economic action over time, followed by empirical implementation of at least certain aspects, most noticeably in the securities markets, to which I will return a bit later.

I have emphasized the future, but the future always brings uncertainty. Perhaps the greatest intellectual step of all in our understanding of the role of time in economics has been the explicit recognition of uncertainty as an economic fact and as a factor recognized by individuals in their market behavior. Investors and savers alike are aware of the universality of uncertainty. They are uncertain about future prices, about future technology, about future preferences for goods.

Yet it is only after 1947 that economists and decision analysts developed explicit methods for assisting individuals, firms and governments in making decisions under uncertainty and to study the equilibrium configuration of the economy when agents are uncertain about the future and know they are uncertain. The study of this topic has transformed the content of economics, more perhaps than might be expected on first consideration.

The study of human behavior under uncertainty deepened and intensified our understanding of one of the basic and most enduring themes in economic analysis, the notion of rationality. Implicitly in Adam Smith, explicitly in the economic theorists of the last quarter of the nineteenth century who in many ways set the basic structure of modern economics, the actors in the economy are considered to be rational beings in their choices.

First of all, firms are supposed to be rational in the sense that they seek, successfully, to maximize their profits under the conditions they face. This implies, for example, that each firm chooses methods of production that make as small as possible the cost of production of whatever amount is produced and produces that quantity which will yield the largest profit possible, that is, the excess of revenue over cost is maximized. Rationality is identified with maximization; a rational firm will not rest content with one level of profits if by changing either its methods of production or its quantity produced it can make higher profits. Second, each consumer or household is assumed to choose the quantities of the different goods consumed (and amount of labor supplied) so as to maximize some measure of the satisfaction the consumer receives, given the prices the household faces and the income it has.

These have been the traditional criteria for economic rationality. Under conditions of uncertainty, however, the concept of rationality becomes stronger, that is, says more about the behavior of economic actors. It is required that there be consistency among choices made under different conditions of uncertainty. To state in detail these criteria and their justification would take more space than is deserved here; but they can be made very persuasive indeed. They lead to the following standard formulation: We do not know which of several different possible outcomes will occur, but we can assign probabilities to each possible outcome. The actions taken by firms and households are like bets, in that the outcome of any such action (investment, act of saving) depends both on the action taken and the outcome in fact realized (e.g., which horse will win in a racetrack bet, what future product prices will be). In this language, I give one example of a rationality assumption: If you are willing to bet on an event, and the odds change in your favor, you are still willing to make the bet.

Earlier, when discussing the way resources are allocated over time, I stressed the importance of expectations of future prices. When uncertainty is recognized, the concept of expectations is broadened. There is no single price expected; the investor or saver is uncertain about future prices and knows he or she is uncertain. Economic actors hold probability distributions of future prices for each commodity and each time period in the future. Put a slightly different way, there is a set of *contingent* prices, one for each commodity in each time period under each of the possible states of affairs that may prevail between now and the time in question. For example, we might have a price for wheat next year contingent on the weather that prevails in the intervening period. Actually, in fuller detail, we should have a price for wheat next year contingent on the weather, demand conditions here and in foreign countries, technological innovations in the milling industry, new information about the health implications of bread and of rival commodities, and so forth. Each contingent price is a well- defined number, but the contingencies themselves are uncertain. Each actor in the economy assigns probabilities to the alternative possible contingencies.

Let us suppose that these contingent prices and the probabilities attached to the contingencies are rationally developed by the participants in the economy. Then, if all the relevant information is publicly known, all individuals will have the same probability judgments. This theory may seem very general, but in fact it has striking implications for the pricing of securities. Namely, on the best available information, the average change will be zero (corrected for dividend payments). That is, the price of the security tomorrow is not known today. All investors assign the same probabilities to the different possible values of tomorrow's price. Then the average of these different possible values, calculated using these probabilities, must be today's price. The reason for this conclusion is that if it were not so, if there were some predictable tendency for the price to rise from today to tomorrow on the average, then buyers would bid up the price of the stock today and so remove the profit opportunity.

This exposition contains in a nutshell what has come to be known as the "rational expectations" theory of the movement of prices and quantities over time. No doubt there is a slightly absurd aspect in assigning complete rationality and complete foresight within the limits of ineliminable uncertainty, a point illustrated by a parable widely repeated among economists: Two economists are walking down the street. One says, "Look, there's a $20 bill lying on the street," to which the other replies, "There can't be; if there were, someone else would have already picked it up." Nevertheless, the hypothesis that securities price changes are unpredictable from current prices has been subject to a good deal of empirical test. Many studies have confirmed the hypothesis very well; others have found minor variations.

Recognizing the importance of uncertainty in forward-looking economic behavior has led to another crucial extension of our perspectives, understanding information as an economic variable. Information can be looked at broadly as any observation which *changes* one's probability judgments. Up to this point, I have made explicitly the assumption that all information is public, that it is freely available to all individuals equally. In fact, of course, different agents have different information. Certainly, each firm, for example, knows its own production possibilities better than other firms and better than possible investors;

similarly, consumers know their own future needs better than others and particularly better than potential lenders to them. In a complex world with much more knowledge than any one individual can have, knowledge is specialized. Indeed, increasingly what workers of all kinds are selling is knowledge of a kind that others do not have; physicians, lawyers, and professors are no doubt extreme examples of what is, however, a very widespread characteristic. But such a world is one in which differences in information not only exist but are the very reason for the existence of economic transactions.

There are many implications of this new viewpoint. One is that prices, particularly of securities or other assets, themselves convey information, for they reveal something of what other people know. If one investor sees a rise in the price of a security for no reason known to him, it might at first be concluded that he or she will sell. In fact, the investor might be better off to infer that someone else has received favorable news and acted on that and therefore to conclude that the security is worth more than originally thought.

There are many other implications of differing information among economic agents. In a transaction between a less and a more informed person, the former cannot be sure the latter is using the information available in the former's interest. This leads to provisions in contracts and in the nature of industrial organization which depart considerably from the simple model of buying and selling services and goods at fixed prices. The new analysis has explained the many complex systems of rewards and long-term contractual relations as responses to the possibility of exploitation of special informational inequalities.

I have given to this point a fairly glowing picture of the success of economic theory in grasping more realistically many aspects of the economic relations among individuals: transactions over time, the presence of uncertainty, and the existence of private and uncommunicated information. I must add a sense of caution and limits to the accomplishments, not only about the state of theory but even at the level of fully understanding what the questions are.

It has in fact been a long-standing complaint against standard economic theory that it depends too much on the assumption of rationality. The complaint was raised by Thorstein Veblen, a famous dissenting economist at the turn of the century, against a theory of rational behavior in circumstances much less complex than those now studied, a world of certainty and predictability. Economic agents do not and indeed cannot perform all the calculations demanded by the theory. To anticipate prices rationally, they have in effect to understand a correct model of the economy and use it to project future prices or, more precisely, what future prices would be under a great variety of possible contingencies. The impossibility of carrying out such calculations is manifest from everyday observation and confirmed by the inability of economists using our theory and our computing power to make good forecasts (even good contingent forecasts). The theory of computation shows that the necessary computations have a high degree of complexity.

In recent years, cognitive psychologists have studied the abilities of humans to make rational choices and form probability judgments rationally in experimental situations far less complex than the real economy. Not only are the assumptions of rational behavior strongly contradicted, but systematic kinds of bias are found. Two are of special interest because of their potential implications for economics. One is a tendency to overconfidence. People usually rate their own abilities above average; obviously, this cannot be true of half of them. Thus, the buyer of a security is not so likely to ask himself or herself what the seller knows that motivates the sale; the buyer simply assumes his or her superiority of understanding. A second is that choices that are objectively the same are looked at differently according to the context, a phenomenon known as "framing." The same outcome can usually be thought of as a loss or a gain, depending on what the outcome is being compared to. For example, a profit on the sale of a stock could be thought of as a gain compared to the purchase price and a loss compared with what was expected or what could have been made in an alternative investment. Rational behavior implies that these comparisons are irrelevant to a decision to sell the stock. But a considerable body of both experimental and field evidence in psychology implies that action does indeed depend on framing the decision as a gain or a loss.

There are many phenomena observed in the market which can be interpreted to confirm lack of rationality. These are best drawn from the securities markets, not so much because they are central to the economy but because they can be far better observed than other economic phenomena, such as investments in plant and equipment. There is considerable evidence that stock and bond prices fluctuate far more than is consistent with rational behavior on the part of the participants. The theory would imply that the current price of a security is the present value of its future returns averaged over all contingencies. Then the price should change only as the probabilities of different contingencies change in response to new information. But important new information is rare. The price of a given stock frequently changes by 5 percent in a single day with no significant news about its prospects. This undue response to small changes conforms to some generalizations found experimentally by cognitive psychologists.

Another observation is the large volume of transactions on organized markets. The assumptions of rational behavior, including rationality in deducing changes in the information of others from changes in market prices, would imply that no transactions would take place simply because of some change in the private information of some individual, although the price would alter. Transactions would be motivated by hedging or changes in other circumstances of the individual (aging or other changes in wealth or need prospects). For example, on the foreign exchange markets, a purchase or sale would be rational as an accompaniment to a sale or purchase abroad, to hedge against changes in the value of the payment. Hence the volume of transactions would be at most equal to the volume of foreign trade. In fact, the transaction volume is hundreds of times greater.

What can we conclude? The image of the future, cloudy though it be, powerfully influences the current state of the economic world (and indeed the social and political world). The formation of this image owes something indeed to our individual and collective efforts to use our experience and our preconceptions to that end. We are not without sense and reason, but we are subject to the necessity of oversimplification and to biases built into us. We can effectively use only part of the knowledge that is or could be available to us. But even if we used all we could, our prevision would be deficient because so much can happen that will necessarily be a total surprise.

An important task of economic analysis today, in conjunction with recent work in psychology and in computer science, is to know better how we come to acquire knowledge and form beliefs and how we act and can act on that knowledge.

In a series of papers, beginning in the mid-1950s, **ROBERT M. SOLOW** focused on factors affecting the long-term growth of national income. He developed the theoretical foundation as well as the key to the empirical measurement and estimation of the effect of technological change on output.

Professor Solow holds B.A., M.A., and Ph.D. degrees from Harvard University. He has been awarded honorary degrees from U.S. and foreign universities including the University of Chicago, Yale, Brown, Tulane, the Sorbonne in France, the University of Warwick in England, and the University of Geneva in Switzerland. Professor Solow has been associated with MIT for his entire professional career. On leave from MIT, Dr. Solow was a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford University, Marshall Lecturer at Cambridge University, Eastman Visiting Professor at Oxford University, Overseas Fellow at Cambridge University, Devries Lecturer at the Netherlands School of Economics, Wicksell Lecturer at the University of Stockholm, Mackintosh Lecturer at Queen's University, Mitsui Lecturer at the University of Birmingham, and Visiting Lecturer at Warwick and Manchester.

Dr. Solow is a past president of the American Economic Association and the Econometric Society. In 1961 he was awarded the John Bates Clark Medal by the American Economic Association. Dr. Solow is a member of the National Academy of Sciences, the American Philosophical Society, the American Academy of Arts and Sciences, and a fellow of the British Academy and the Academia dei Lincei in Rome. Dr. Solow has been appointed by two U.S. presidents to serve on various commissions, has served as a senior economist with the Council of Economic Advisers, and has been Chairman of the Board of Directors of the Federal Reserve Bank of Boston.

Professor Solow has written several books and more than 100 articles in scholarly economics journals. Titles of Dr. Solow's books that are indicative of his work include: *Linear Programming and Economic Analysis*, *The Nature and Causes of Unemployment in the United States*, *Capital Theory and the Rate of Return*, and *Growth Theory*.

2

Dr. Robert M. Solow
Institute Professor and Professor of Economics
Massachusetts Institute of Technology
Recipient of the 1987 Nobel Prize in Economics

A Nobel prize—rather like James Bond's 007—appears to be a license to have an opinion about anything. But economics was founded by Adam Smith on the rock of the Division of Labor, and specialization is the name of the game. So I am going to specialize on the state of macroeconomics, mostly theoretical but with a few glances at applications. Distinguished and clever economists have been heard to remark knowingly that they understand microeconomics perfectly well and know what they think about this or that, but do not understand macroeconomics at all and find it a mystery. I have no patience with that ploy. Macroeconomics is what it is all about. If you do not understand the business cycle, unemployment, inflation, the real exchange rate, well, you do not understand economics at all. Microeconomics is easier, of course; it does not set itself such hard problems or aim at passing such hard tests.

My goal is to describe the current state of macro theory and to reflect on how it got there, its relevance for practical policy, and its possible evolution. I would like eventually to reach some understanding of why there is so much disagreement in public on what appear to be fundamental issues, with equally able and eminent economists taking contradictory positions. This situation gives rise to rude jokes and it explains, no doubt, the occasional coy attempt to dissociate oneself from the whole embarrassing exhibition. In considering these questions I am not going to try always to be judicious. That would be dull for all of us. I have strong prejudices on these matters and I will express them freely, probably sounding less tentative than I feel.

Debates about the fundamentals of macroeconomics usually structure themselves as arguments for and against "Keynesian economics." One reason for this formulation is that macroeconomics as we know it really begins with Keynes and the *General Theory.* Before that there was "Business Cycle Theory." Economists, like other people, observed that there was some alternation of good times and bad times. The question arose, then: Why does it happen just that way? Business cycle theory looked for behavior patterns and market mechanisms that could be shown to be capable of generating and propagating cyclical fluctuations in the whole economy. They found plenty. Some of them were interesting, and remain interesting, just as economics is. The business cycle theory that I learned and taught did not quite amount to macroeconomics, however. It lacked a comprehensive theory of the determination of the level of economic activity, a theory of "output as a whole" as Keynes later called it. This is not a merely aesthetic complaint either. As the Great Depression of the 1930s showed, a model of regular repetitive cycles is not an adequate representation of the aggregate economy. Things happen that do not lie comfortably in that sort of model.

So macroeconomics in the modern sense really dates from the *General Theory* and that is one reason why it remains the focus of so much contemporary argument. The people who wrote the first reviews were my teachers, and some of them are still functioning today. There is an additional reason: it was a provocative book, an intentionally provocative book, and it still provokes. It was also an undigested book, in the sense that it contained several distinct story lines. These are not well integrated with one another and indeed they are not always compatible with one another. This protean character makes it a good subject for debate, not only with its enemies but also among its avowed friends.

Eventually, maybe by some process of natural selection, an Authorized Version evolved. It is sometimes described as "American Keynesianism," although two of its main sources were famous articles by John Hicks and Franco Modigliani. The main components of this standard model were and are an aggregate demand side derived from some version of IS-LM, and a theory of the price level, sometimes anchored in a given, inflexible, nominal wage, but not always. In practice, this model has been used with the presumption that most of the time the economy is operating below its potential for employment and output, with the realized levels of employment and output determined mainly by the demand side. For a long time the implicit belief was that the demand side is more volatile than the supply side. That may once have been a valid induction from history. More recent events have taught a contrary lesson. Nowadays supply shocks get at least equal billing, and movements of the price level and/or changes in the rate of inflation, and expectations about those things, play a more prominent role than they used to. There is no need for me to provide any detail, because you will recognize that I have described the model that is embalmed in most elementary and intermediate textbooks of macroeconomics, and embodied in the big complete econometric models.

The original controversy between monetarism and Keynesianism was carried on within this framework. There were really two separate issues. One was quite specific: it had to do with the nature of the demand for money, especially its interest-elasticity. Within the model, this boils down to questions about the shape and stability of the LM-curve. The second issue was considerably broader; it had to do with the strength of the forces pulling the aggregate economy back toward its potential for output and employment after a disturbance. Within the model, this boils down to questions about the flexibility of wages and prices and their relation to employment and output. These issues are separate in the sense that the answer to one does not determine the answer to the other. But they are related: the first has to do with the way a monetary shock sorts itself out between velocity and nominal demand and the second with the way a shock to nominal demand sorts itself out between output and the price level.

There was, of course, an argument about policy lurking behind the analytical issues. One side believed that steady growth of the money supply (or the monetary base) was the best and only necessary macroeconomic policy; the other believed that activist fiscal and monetary policy could improve macroeconomic performance. It is hard to shake the notion that there was an ideological fire behind all that intellectual energy. I suggested that the two analytical issues were in principle separate. So there are four possible positions one could take, but only two of the four boxes were ever seriously occupied.

In the past 10 or 15 years, macroeconomic theory has revolved around a slightly different axis, though the genealogy is pretty clear. Monetarism evolved into "new-classical" macroeconomics and American Keynesianism into something for which I have no catchy nickname. As is so often the case with macroeconomics, theoretical developments have both external and internal roots. They are in part a response to events out there in the real economy, and in part a response to gaps and anomalies that show up in the working-out of the theory itself.

The main external event was the inflation of the 1970s. I think of it as having been set off by OPEC and raw-material inflation generally. Others, especially from the new-classical school, would regard that attribution as a typically shoddy piece of Keynesian *ad hockery.* It is not my job right now to analyze that inflationary episode. I am discussing the recent evolution of macroeconomic thought. From that point of view, what was important about the post-OPEC inflation was the appearance of a major sudden economic impulse that did not originate on the side of aggregate demand. For that reason alone there was no ready analysis from the Keynesian consensus. The embarrassment was compounded by the sharp role played by inflationary expectations, beginning with the late phases of the Vietnam War, especially expectations about future public policy and its consequences.

Neither development, neither supply shocks nor inflationary expectations, is incompatible with Keynesian macroeconomics. Contemporary textbooks like those of Dornbusch and Fischer and Gordon handle them as a matter of course, within a framework that is recognizably American-Keynesian. But the consensus was caught napping, to put it mildly. It took a while to recover. In the meanwhile, and even afterwards, the Keynesian consensus discovered that it had no adequate policy tools for meeting a supply-side-induced inflation compounded by entrenched inflationary expectations. That is perhaps not the fault of the theory; no one else has a good policy answer either. But there is at best cold comfort in that excuse. We had allowed ourselves to become too optimistic about the tightness of our analysis and our capacity to guide the economy. The grip of the consensus was weakened.

The internal impulse that triggered and reinforced the new- classical movement was quite different. It was the conviction that macroeconomic theory ought to have "microeconomic foundations." I think that this conception has been subtly misread, however. So far as I can see, macroeconomic arguments have always been justified by an appeal to microeconomic convention or knowledge. Just think of the way the consumption function and its variations are expounded in textbooks, or the way everyone introduces the chapter on the aggregate investment function by explaining the maximization of present value. The new school insisted on something much more formal, the grounding of macroeconomic models in complete individual-agent-based microeconomic theory. That demand certainly resonated in the profession at large. There is nothing wrong with it in principle, but I think that it has been a blind alley in practice.

The reason is that, in practice, the demand for micro foundations almost had to become a demand to build macroeconomic models on Walrasian foundations. If the felt need was for a formal connection, if a sound macro model had to be the aggregation of a complete, developed micro model, then Walras was all we had available off-the-shelf. The trouble is that Walrasian general equilibrium theory begins by assuming away all of the problems that make macroeconomics interesting. (The *Elements* is not a book about business cycles, after all, but precisely the opposite.) The consequence of this historical accident has been that much high-caliber mental effort has gone into elaborate attempts to prove that unemployment is either nonexistent or healthy.

It will be noticed that I have not used the phrase "rational expectations" to characterize new-classical macroeconomics. That is because I think that the assumption of rational expectations is neither necessary nor sufficient for new-classical results. What is characteristic of the school arises even without rational expectations. It depends rather on two other, apparently less plausible, assumptions: that all markets are smoothly cleared by flexible prices, and that all business decisions are merely the carrying-out of the atemporal and intertemporal wishes of the households that own the firms. (That would account for the popularity of representative-agent models in this tradition.) Conversely, if you start with, say, a Benassy-Malinvaud fixed-price model, in which markets are foredoomed not to clear, then adding in rational expectations can easily reinforce its Keynesian air. The main thing adding rational expectations does for a modeler is to allow multiple equilibria, and that is not especially good news for new-classicism.

A sharp version of the basic new-classical claim is that there is no specifically macroeconomic problem distinct from the general economic problem of scarcity. Presumably there could be, but in our world it does not happen. What you are seeing when you look at business cycle fluctuations is an economy adjusting optimally to exogenous real shocks to taste and technology, to changes in the weather, for instance. (I am describing the later version of this stance, usually called "real business cycle theory" rather than the earlier version that was more akin to monetarism in locating the main source of disturbance in monetary shocks.) Thus if there are cycles, they are adaptive rather than dysfunctional (apart perhaps from some unavoidable noise). Of course we all wish we were richer (i.e., more productive) and we all wish we could predict the future better, but neither is a meaningful object of macroeconomic policy on the business cycle time scale.

Let me be even more explicit. You are asked to believe that the real economy behaves as if a single immortal consumer were making optimal resource allocation decisions, intratemporally and intertemporally, constrained only by technology and available information. This already assumes that the production economy simply responds to the consumer's wishes. You must decide if that is a credible assertion.

I must tell you that there are no great empirical or predictive successes associated with this theory. If it said anything, it said that the disinflation of 1979-83 would be accomplished without a recession. That turned out to be false, of course. Is there a defense? Yes: it can be said that the conditions for a controlled test of the theory were not met, that the disinflationary monetary policy was not credible, for example. The practical man's comment that we are not likely ever to get a more credible disinflationary monetary policy can be shrugged off. The trouble is that it is always possible to claim that the conditions for a controlled experiment have not been adequately met, so to say it in this instance is not to say much.

New-classical macroeconomics likes to take a rather different view of empirical verification. It prefers to set itself the task of reproducing, at least qualitatively, the pattern of variances and covariances in observed time series, and it pats itself on the back whenever it succeeds in doing so to an acceptable approximation. I think this is a misleading procedure; but the point I want to make is a general one and it applies on my own side of the fence as well. The trouble with judging the empirical validity of theories in this way is that the test almost certainly has low power against many interesting alternatives. That is to say: many other models of the economy can do just about as well in fitting that limited class of facts. The conclusion is that even "success" of this kind provides reason to accept new-classical macroeconomics only if it is preferred for other reasons. If you find it implausible, as I do, then you are not in the slightest obliged to accept it on its own empirical grounds. And of course the same goes for other theories, unless they can produce empirical tests with considerably more discriminatory power than these.

Here, I think, is an important contribution to understanding the widespread, perpetual, and apparently endemic disagreement that characterizes macroeconomics to the distress of all of us. Deep down, we all know that, as soon as we come to truly subtle questions, econometrics does not substitute well for the controlled experiment as a device for discriminating between competing theories. One has the uncomfortable feeling that if you try hard enough—always on subtle and complicated matters—you can find data, functional forms, statistical techniques, lag structures, that will tell you what you want to hear. Economics is not alone in this, by the way. A tuned-in person can see the same thing happening with global climate models as they look for traces of the theoretically reasonable greenhouse effect. Their problem is much the same as ours. The questions are subtle, the data are noisy, and there are many forces at work simultaneously.

There is a lesson here for macroeconomics, I think. To begin with, we should stick to first-order questions and accept only robust answers, at least when we are being serious. I have no objection to playing around, trying things out; that is one of the ways we learn. But for public consumption, the standards should be different. Second, we are not only entitled to use common sense and to make judgments of plausibility based on general observation, but there is no sensible alternative to doing so. Such conclusions are vulnerable; that goes without saying. They have to be defended without rancor and without cant. And finally, in so doing, the broadest possible variety of evidence should be mobilized. There are other roads to knowledge besides formal statistical inference. They have to be used critically, as does formal econometrics for that matter, but we cannot afford to dismiss any bit of information the world offers.

You will have gathered that the new-classical way of going about macroeconomics is not my preferred way. But I would not advocate going back to the "hydraulic" Keynesianism of the 1960s even if that were possible. The key to doing better, I think, is to pay attention to the "macro foundations of microeconomics." I hope I mean something more that cuteness by that phrase: I mean, roughly, that we are entitled to ask what sorts of microeconomic mechanisms both look right and do justice to the nature of the general economic environment in which they are expected to function.

There is now a self-conscious "New Keynesian Macroeconomics" that tries to do just that. One wing of it tends to emphasize transactions costs, information asymmetries and similar imperfections, and shows that, in that kind of environment, the economy by itself can easily achieve unsatisfactory states (equilibria) which might be improved by corrective fiscal and monetary policy. There is another strand that places greater emphasis on imperfect competition, increasing returns to scale, and trading externalities—the tendency for optimistic (pessimistic) choices by some to validate optimistic (pessimistic) choices by others. These mechanisms lead to the conclusion that the economy may be capable of many self-sustaining equilibria, some much better than others. I have a fairly vague feeling that this second approach is on to something deeper than the first. That feeling—it is not much more than that—governs my choice of illustrative examples for non-Panglossian macroeconomics.

To start off, let me refer back to IS-LM-based American Keynesianism. Most of the time, as I mentioned, it rested on the hypothesis that the nominal wage was the sticky price that kept the labor market from clearing at full employment. (In the standard version, the price level for goods was taken as perfectly flexible. One of the advances made by the Benassy-Malinvaud fixed-price literature was to enlarge the picture by treating the goods market and the labor market more symmetrically.) Since the nominal wage is not permanently fixed, textbooks pointed out, and many still point out, that the nominal wage adjusts only slowly to the state of the labor market. The model is then one of disequilibrium, possibly prolonged. Underemployment lasts as long as the disequilibrium lasts.

Now Keynes himself certainly believed that the nominal wage was sticky in this sense in Britain during the 1920s and 1930s. He even suggested why that might be: because resistance to nominal wage cuts in a decentralized labor market is the only way that workers can defend their *relative* position in the wage structure. It is less well remembered that Keynes argued that wage stickiness was probably a good thing, that perfect wage and price flexibility could easily be destructive of real economic stability. His reasoning went like this. In a monetary economy, the nominal interest rate cannot be negative. Hence the *real* interest rate must be at least equal to the rate of deflation. (That is what holding cash would earn, after all.) If wages and prices were to fall freely after a contractionary shock, the real interest rate could become very large at just the wrong time, with adverse effects on investment. The induced secondary contraction would only worsen the situation.

I can report that Frank Hahn and I have verified Keynes's intuition within a model that is in every respect respectable. That is to say, we can exhibit situations in which complete wage flexibility, while maintaining full employment after a shock, drives the model economy off on completely unstable trajectories of pointlessly fluctuating output that never return to the original steady-state equilibrium. Somewhat slower wage adjustment would make things better, not worse. And there is a (complicated) monetary-fiscal policy that is in principle capable of nipping the whole process in the bud and getting over the initial shock with minimal disturbance.

The point of this exercise is not to demonstrate the wisdom of the Great Lama. It is much more devious. If perfect wage (and price) flexibility is not always the best way to run an economy, then it is perhaps less peculiar that economies should develop institutions that limit or discourage aggressive wage-cutting in times of moderate unemployment. Hahn and I have actually produced a formal model of just that kind. In it the labor market is modeled as a kind of repeated game involving workers and employers. We show that there is an equilibrium strategy for workers in which the unemployed refrain from competing for jobs, as long as the unemployment rate is not too high. What makes this an equilibrium strategy (i.e., one from which it pays no individual to depart unilaterally) is the threat that any violation of the norm will lead to a long period of unrestrained competition in the labor market. In that event no worker does better than the reservation wage, whereas adhering to the norm gives even currently unemployed workers the expectation of sooner or later acquiring a job at something higher than the reservation wage.

There is a general methodological lesson here, and it is what I am after. The model just described has many equilibria; in fact there will generally be a whole interval of wage rates and corresponding unemployment rates, any one of which could persist if once established. (Which one actually occurs may then be a matter of historical accident.) The point is that this multiplicity of equilibria arises easily as soon as one gets away from the notion that price-mediated market clearing is the only equilibrium concept worth discussing. Noncooperative game theory has taught us that the fundamental idea of an equilibrium is the "strategic" definition I have used here, a choice of behavior patterns that leaves no participant impelled to make a unilateral change. If it seems to you, as it does to me, that the current state of the economy could have been different—I am suggesting a thought-experiment about positions of rest, not about short-run dynamics—then the idea that there can be many self-sustaining equilibria should be your cup of tea.

The non-Panglossian branch of modern macroeconomic theory has produced some other models that fall into this same category. Several of them rest on an idea that goes back into business cycle theory well before the *General Theory*, what I earlier called a trading externality. It is, in far too simple terms, the notion that widespread optimism is self-justifying, but so is widespread pessimism. Businesses and households who are optimistic about their own market prospects will make decisions that, taken together, create strong markets and thus validate their initial optimism. If they had all been pessimistic to begin with, they would have done things that validated their initial pessimism. If you can believe that then you believe that there are (at least) two equilibria, a high-level one and a low-level one. It would not be too far-fetched to think of one as prosperity and the other as recession.

Needless to say, that simple thought is not even a sketch of a sketch of a theory. All the economics remains to be done. But it has been done, several times in several contexts. Just by way of example, Walter P. Heller (the son of my old leader at the Council of Economic Advisers) has studied an economy consisting of two monopolistically competitive industries, each of which sells only to the employees of the other. (This artificial-sounding condition would seem quite natural if there were many industries. It is meant to serve a reasonable purpose.) As imperfect competitors, each firm has to form expectations about the location of its demand curve. In effect, then, it must form expectations about the production and employment decisions of the other industry. The other industry is meanwhile doing exactly the same thing. With a few unrestrictive conditions on demand-elasticities, Heller is able to show that the optimism-pessimism story actually holds in this set-up. There can indeed be two or more self-sustaining equilibria and it is no trick at all to describe reasonable conditions under which the high-level equilibrium is clearly better for everyone than the low-level one. This model has the amusing property that the government can bring about the high-level equilibrium simply by announcing in a convincing way that it will do so. If the announcement is believed, the government will never actually have to do anything. This economy has nothing to fear, one might say, but fear itself.

A different and in some ways more powerful conceptualization of the same general idea can be found in the model of "search equilibrium" proposed by my colleague Peter Diamond. In Diamond's story, people "accept productive opportunities," some of which are more advantageous than others, produce goods, and then look for buyers, who are people just like themselves, having produced something to sell. Buyers and sellers are completely symmetrical; we can call them traders. It is better to be a trader when there are lots of traders, because then it is easier to find a partner with whom a mutually profitable exchange can be carried out. If there are a lot of traders, then I will be inclined to accept somewhat less attractive productive opportunities. Better market prospects justify greater production. By acting like that, of course, I add to the number of traders out there and thus justify greater production by others. Diamond is able to show that this set-up, too, is very likely to provide two or more self-sustaining equilibria; when it does, the ones with more output and employment are better than the ones with less. In an added *tour de force,* Diamond and Fudenberg prove that this model can even produce regular business cycles in its primitive form of economic activity, each phase leading regularly to the next.

It is a fair criticism of the approach to macroeconomics that I have been describing (and favoring) that it seems to produce only a collection of fragments. The new-classical scheme at least produces a complete model that can be equipped with empirically based parameters and simulated. Its behavior can then be checked against selected characteristics of the world of observation. The older American Keynesianism went even further and culminated in the large econometric models that still grind out detailed forecasts month after month. (There are, of course, smaller models too.) The more recent shoots from the Keynesian tree have the character of examples, illustrations of possibilities. They are sometimes phrased in fanciful ways, as if to emphasize that they are not intended for econometric treatment.

There are two responses to this criticism. One is simply that it will take time to develop these newer possibilities into a form fit for empirical application. That may indeed be true; but it is not the response I want to make. To my mind, the role of macro theory (even, in a sense, applied macro theory) is not necessarily to make a single all-purpose model to represent the world. It is certainly not unconditional forecasting. It is rather the uncovering of mechanisms that cause the economic system to malfunction in significant ways, and then the analysis of kinds of policy measures, directions of policy even if not exact doses, that are potentially therapeutic. I would resist the notion that the market failures in question are "mere aberrations" of the system; they *are* the system. Nevertheless, my sort of macroeconomics is inevitably less monolithic than the other. This may explain the attractiveness of the new-classical model; it looks much more like a candidate for System of the World.

If neatness is your dominant concern, and I offer you a menu that includes a whole bunch of little models, not easily put together, a certain kind of mind will choose "None of the above" even if that answer violates common sense.

How might this version of macroeconomics evolve? One could imagine continued analytical study of these and other mechanisms that give rise to occasional recessions and bursts of inflation, along with an ongoing attempt to evaluate their importance in the modern economy. This effort would be partly econometric, partly institutional discussion, storytelling, educated judgment, all of those things, but I would certainly look for rough quantification. Notice how there will be room for differences of opinion even within this paradigm. Especially if the key concept is the multiplicity of equilibria, there will always be the empirical problem of characterizing the sort of equilibrium the economy is in at the moment, and choosing the relevant model.

Perhaps I am suggesting moving away from the image of economics as the physics of society toward the image of economics as something more like ecology or medicine or cell biology. I am not referring to any analogy of content, but just a view of scientific effort that is less formal and general and reductive, and more tolerant of a variety of models suited to a variety of problems and contexts. From that point of view the sort of model developed so elegantly and attractively by Lucas and Prescott is just one of many possible mechanisms; its applicability has to be argued anew in each concrete situation. In optimistic moments, I think that evolution has already started.

This apparently academic subject is actually real and relevant and contemporary. There seems to be general agreement that the probability of a recession in the United States before the end of 1990 is something like one-third. Suppose it happens. What will we do? What will the Democratic Congress think is the right stance for the federal government? What will the Republican President propose? What will the conservative but more professional Federal Reserve decide to do? Will there be any coordination among them? And how will the argument be conducted?

There are two main currents of thought that have their existence both inside and outside professional economics. One is generally *laissez faire.*

It says that the unfettered private enterprise economy is well-behaved and self-correcting. Whatever it does, however it behaves, is probably all for the best. The optimal government policy is to get out of the way. The recession will run its course, and anyway it is not really a recession. The other main attitude says that although there is nothing basically wrong with the free enterprise economy, there are certain areas where it is vulnerable to market failure. Some of these are "microeconomic"—like excessive pollution or misleading labels on food—and require regulation of some kind. But there is also macroeconomic market failure, a tendency occasionally to lapse into recession—a systemwide underproduction of goods and therefore underprovision of jobs—or inflation or stagflation. That calls for compensatory stabilization policy, and sooner is better than later.

This dichotomy does not need academic economics to keep it going. It has deep roots in ideology and self-interest. But it is reflected in an ongoing debate within academic economics, and especially within macroeconomics. The debate is not in the first instance about policy but about the correct "model of the economy." The balance of influence shifts from time to time, partly in response to what happens in the real world (not only in the economy but also in public opinion), and partly in response to which side seems to have the analytical upper hand. Within academic economics the two sides are often labeled Keynesian and anti-Keynesian (currently "new-classical"). As I have explained, that is because a great watershed in this debate—which has been going on for centuries—occurred in 1936 with the publication of the first model of the economy supporting the macro failure view ever to achieve academic respectability. The label sticks, even if today the detailed context has little to do with the original. Needless to say, in this typology I would be classified as a Keynesian, for good reason.

Should any of this matter to citizens? I offer a partisan but—I hope—not narrow-minded answer. When the next recession rolls around, do not be seduced by ideology into believing that government is necessarily part of the problem and cannot be part of the solution. Government often is part of the problem because it is often incompetent, often dominated by false beliefs about the world, often moved by ulterior motives. But there is no general theoretical truth that guides you one way or the other. You should try to listen as impartially as you can manage to analyses offered by economists of every persuasion, trying to get at the picture of the world, today's world, that underlies each diagnosis. Then you should fearlessly form judgments, fearlessly but tentatively. Above all, you should try not to be bored.

LAWRENCE R. KLEIN is the founder of Wharton Econometric Forecasting Associates, and former Chairman of the Scientific Advisory Board. He is also a principal investigator for Project LINK, an international research group for the statistical study of world trade and payments.

Professor Klein earned a B.A. degree from the University of California, Berkeley, and a Ph.D. degree from MIT. He has been awarded about twenty honorary degrees from universities in the United States and several foreign nations. He has taught at the University of Pennsylvania for the past thirty years. Before joining the Pennsylvania faculty he was associated with the University of Chicago, the National Bureau of Economic Research, the University of Michigan, and Oxford University. While at the University of Pennsylvania he has served as visiting professor at several U.S. universities including the University of California at Berkeley, Princeton, Stanford, and City University of New York and at foreign centers of learning in Japan, Austria, and Denmark.

Dr. Klein is a past president of the American Economic Association, the Eastern Economic Association, and the Econometric Society. In 1959, he was awarded the John Bates Clark Medal by the American Economic Association. He is a member of the American Academy of Arts and Sciences, the American Philosophical Society, and the National Academy of Sciences. Dr. Klein has been a frequent adviser to U.S. government agencies including the Federal Research Board and the Congressional Budget Office. He has also been a consultant for U.S. research institutes such as the Stanford Research Institute and the Brookings Institution as well as many international organizations.

Professor Klein is the author or editor of more than 25 books, specializing in econometrics. He has also written in excess of 250 scholarly articles on economic subjects. Titles of Dr. Klein's books that are indicative of his work include: *The Keynesian Revolution, An Econometric Model of the United States, 1929–1951, The Wharton Econometric Forecasting Model, The Brookings Model: Perspective and Recent Developments, Econometric Models as Guides for Decision Making, The Economics of Supply and Demand, Industrial Policies for Growth and Competitiveness,* and *Capital Flows and Exchange Rate Determination.*

3

Dr. Lawrence R. Klein
Benjamin Franklin Professor of Economics and Finance
University of Pennsylvania
Recipient of the 1980 Nobel Prize in Economics

Conceptual Issues

Econometrics blends the disciplines of economics, mathematics, and statistics. All too often the reciprocity of these three disciplines is neglected. It is my feeling that an appropriate combination of all three inputs can be extremely fruitful in gaining insight into the economy, and that concentration on one or two alone will be less useful for this purpose. I have spent more than 45 years looking at economic issues through the medium of econometrics: while I do not believe that this is the *only* approach, I do believe that it is the best approach for quantifiable matters. There are, of course, some important and interesting philosophical, historical, and qualitative problems in economics that do not lend themselves to the econometric method.

This essay is about the present state of macroeconomic theory and policy as it relates to econometrics. It will be concerned with the statistical basis for quantifying these subjects. The evolution and present state of techniques, applications, and the information set will be examined, and future tendencies will be indicated.

Econometrics is a broad subject, applicable to many specializations, but I shall limit this survey to macroeconometrics—the use of econometrics for the study of the macroeconomy. Econometrics is a branch of quantitative economics, and the macroeconomic data base—particularly the structure and availability of data—is very important.

Macroeconomic Theory and Policy

Economic analysis of theory and policy are often confused, particularly with regard to the implementation of policy in the real world. The worst manifestation of this confusion appears in media discussions about business, finance, and economics, but it occurs also in more serious presentations. For example, Keynesian economics is said to be concerned only with deficit spending or classical economics only with financial orthodoxy. It is true that Keynesian economics examines fiscal policy and, under some conditions, advocates deficit spending, but for a very long time Keynesians have recognized the limitations of a simplistic approach. Classical economics does not rigidly support orthodox monetary control for every macroeconomic deficiency.

First, we must make a distinction between theory and policy. Macroeconomic theory involves the construction of a system of thought that describes *behavior* of the economy as a whole—the aggregate economy. One approach is to build that system up from the aggregation of microeconomic relationships. This approach is very attractive but not *unique*. Macroeconomic relationships also may be considered to have a life of their own and can be specified directly, without intricate appeal to microeconomics and the theory of aggregation. The resulting system is also not *unique*, as in the "adding-up" approach.

Having established a macroeconomic theoretical framework and specified it mathematically, we may then confront it with relevant data, estimate the system, and use it for policy analysis. That is what macroeconometrics is all about. The use of such empirical systems for policy analysis results in conclusions that are very rich and encompass many more alternatives than manipulations of fiscal deficits or money supply alone. Such analysis need not be econometric, but noneconometricians tend to oversimplify, because they find it awkward to handle many complex interrelationships simultaneously. It is from this oversimplification and also from expository diagrams of pedagogical treatment of the subject that popular writers have come to consider policy in terms of crude stereotypes—usually limited to two variables at a time.

Macroeconomic Theory

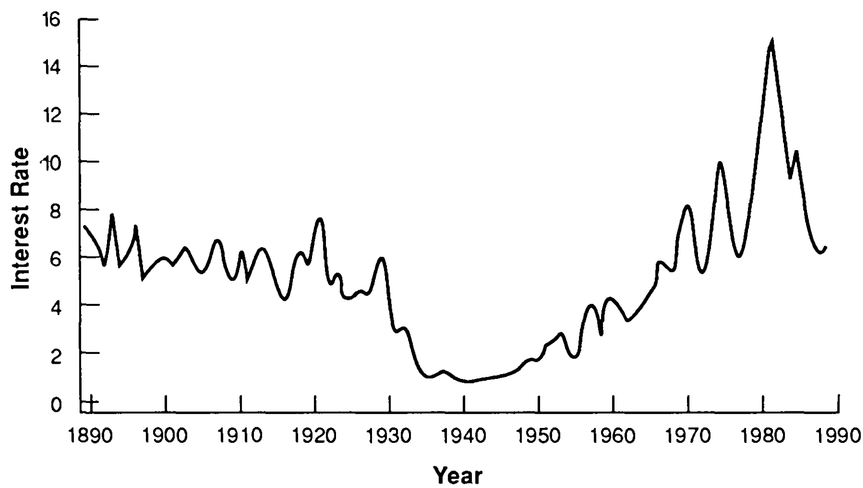
It is difficult for present-day students and research scholars to appreciate just how innovative the Keynesian theory of macroeconomics was some 50 years ago, when it provided an extremely simple explanation of the overall levels of activity and employment that seemed to correspond with the facts of the period. It is legitimate to criticize early practitioners for oversimplifying the relationships involving the fiscal multiplier and overemphasizing the concept of *effective demand.* It soon became clear that the Keynesian macro model needed supply and financial sectors. Financial relationships of an extremely simple sort were incorporated from the beginning, but the technological laws of production, marginal productivity, labor market clearance, and (absolute) price determination were inadequately handled. The outcome for theory development was that these neglected aspects were eventually incorporated into a system that became the *Keynesian-neoclassical synthesis.* That long-winded title is appropriate, since Keynes was truly a student of Marshall and reasoned strictly along the neoclassical lines that permeated economics in Cambridge, England. The extension of the crude Keynesian theory along neoclassical lines was appropriate but inadequate. There was only one price, one interest rate, and excessive aggregation in other markets. The explanation of workers' money illusion with respect to wage bargaining was contrived. In order to handle these problems, I developed the Phillips curve for the labor market and built larger theoretical systems that included many sectors of activity, culminating in the Keynes-Leontief model that has a full supply side. Aspects of income distribution and disaggregation in financial markets were also introduced, the latter through the term structure of interest rates and later through the analysis of the flow-of-funds accounts.

The distinction between classical economics *cum* monetarism and the Keynes-Leontief-neoclassical synthesis became clear through the treatment of the demand for money. The monetarists base their analysis on the *a priori* predictability of velocity (either a constant, simple trend, or function of smooth variables). The extended Keynesians argue, implicitly, that velocity depends on interest rates, which, we have learned, can be extremely volatile.

There is one other important aspect of Keynesian economic theory that needs to be examined. It was originally put forward as a macroeconomic theory for a closed economy. This is most peculiar, given Britain's deep economic involvement in the world and extreme openness. Also, Keynes was obviously deeply concerned about international economics at the time of his death, as he was one of the architects of Bretton Woods. It may have been the bad influence of North American Keynesians, who took the lead in the theoretical development of macroeconomics and reasoned, instinctively, in closed-economy terms. In the 1930s and the early part of the postwar period, the U.S. economy may have been approximately closed, but that characteristic gradually withered away. Most of the economic problems of this country in the last 30 years have been predominantly or wholly international. I believe that it is a grave deficiency of our elementary textbooks that they develop macroeconomics first as a self-contained explanation of a closed system, and then add international considerations as an afterthought, much in the form of an amendment. The international problems of an open economy should be introduced in the first chapter dealing with macroeconomics and pervade the analysis in a meaningful, essential way for the whole exposition. I find it unconscionable that authors of elementary American textbooks (''Principles'') have not done this on a broad scale.

The problems that arose, in whole or in part, from crude Keynesian theory were early fascination with the stagnation thesis, acceptance of euthanasia of the rentier, introduction of an inflationary bias, and an exaggerated belief in accuracy of the model. Economists frequently generalize a few years of recent experience into decades or eras. In the graph of interest rates, it is evident that there was a brief spell, beginning in the early 1930s, when short-term interest rates were low—below 2 percent—but they soon picked up and reached astronomical heights in the United States. Rentiers who had the good perception to ''lock-in'' to the high rates on long-term debt securities which yielded returns of 14-16 percent at the beginning of this decade realized *euphoria* instead of *euthanasia.*

4-6 Month Commercial Paper Rate (1890 – 1987)



The long-term demographic predictions were disastrous. The vanishing of the frontier and the maturation of technology were equally ill-conceived hypotheses. The baby boom, the jet engine, the spread of electronic devices, and fluctuations in financial market rates (not to mention many other pieces of evidence) quickly showed that there was no solid basis for applying the oversimplified Keynesian theory to medium- or long-run macroeconomic development.

The Keynesian theory was developed for European and North American economies, but there was a degree of generality about the system of thought. Some scholars may have made oversimplified applications to developing countries, but, fortunately, specialists on Third-

World development produced more appropriate systems. The Keynes-Leontief model is, indeed, quite suitable for study of developing countries where the data base is adequate.

The study of market-clearing and formation of relative prices by classical economics, supplemented by the quantity theory of money, provided the main alternative macroeconomic theory, under the title of monetarism. Strictly speaking, monetarism deals only with the aggregate relationships that determine money balances (and price levels), but implicitly it is associated with the classical model for the real economy. From a logical point of view the theory is consistent and self-contained, but does it work? This is an econometric question.

The fact that the builders of the St. Louis Model, a macroeconometric version of monetarism, must introduce the world oil price in order to get a satisfactory explanation of recent developments, indicates to me that strict monetarism does not fit the facts. The time curve of velocity does not exhibit steadiness or predictability, and I do find that velocity is significantly related to interest rates. Moreover, the most recent unpredicted (by monetarists) movements in velocity are generally related to the occurrence of *financial innovation.* Changes in hardware, software, and operational characteristics of financial markets have disrupted the underlying monetarist relationships. They have a low degree of *autonomy.*

Now, let us turn to the modern theory of macroeconomics. Among most recent developments are those associated with expectations formation and greater attention to supply considerations. Modern macroeconomic theory appears to be well aware of international dimensions, but the treatment has not yet reached the level of introductory textbooks.

In general, there is much more attachment to market processes, but that is mainly a matter of degree. If the Keynes-Leontief-neoclassical synthesis is fully pursued, all the supply-side characteristics are covered. While I endorse this approach, I reach the conclusion that a full realization of the power and implications of such a system must necessarily lead us to the realm of very large-scale models, with hundreds and—more likely—thousands of simultaneous relationships. Such intricate structures can only be handled by computer analysis of large-scale empirical models. The modern generation, however, appears to be moving in another direction, namely, towards very compact, simplified systems. To some extent, they reason according to a law of parsimony. The smallest system capable of explaining the facts of life is the one to be chosen. The problem with this reasoning is that the "facts of life" are very narrowly prescribed. Little attention is paid to a very wide range of facts, especially potential future events that are not present at the time of the investigations. They are not ready for embargoes, droughts, OPEC power, financial innovation, debt default, or other contingencies. In place of the rule of parsimony, my preference is for:

> the largest system that can be managed, given human and data resource limitations, and one that provides estimates of the main aggregates at least as good as those from any smaller system, since it also gives information on many other magnitudes.

Apart from using smaller systems with less comprehensive coverage of detail and less attention to the intricacies of the data problem, modern macroeconomic theory pays a great deal of attention to expectations. On the surface this is eminently desirable and potentially constructive. Expectations have for years and years been the subject of copious research in macroeconomic theory. Keynes devoted an entire—and insightful—chapter to the subject.[2] G. L. S. Shackle, who admired the Keynesian theory, wrote a book on the subject in order to advance knowledge by building on established lines.[3] Macroeconomic analyses of wage flexibility, especially in recessionary conditions, used implicit reasoning about expectations to refute the reliance on this feature of the labor market to guarantee return to full-employment equilibrium when the economy was deflected from that state. Macroeconometric modeling always relied heavily on the use of expectations, and results from sample surveys of households and firms were consistently brought to bear on the measurement of expectations.

Modern theorists then turned to own-model-generated expectations, misleadingly called *rational* expectations, to generate results by a purely hypothetical intellectual process without even considering whether they have any behavioral meaning. From a statistical point of view, they require the unknown model to provide estimates of expectations data based on parameter estimates that also depend on the expectations data. They surely overwork the sample, asking it to provide both the extra data needed and the parameter estimates based on those data.

The estimates can be computed, if the model is identified, which is not always the case, but the estimates may not even be unique.[4] Models that are so estimated give different response characteristics, in general, but they have not been tested to see if they provide more accurate extrapolations than mainstream models. I doubt that forecast tests will show that such different models are superior. Direct empirical tests of so-called rational expectations are not kind to the concept; adaptive expectations appear to stand up better to the data.[5]

An advocate of own-model-generated expectations is R. Lucas, who has argued on occasion that economic decisionmakers and policymaking authorities will be using the same model and coming to the same conclusion about expected values; therefore policies set by the latter will already have been taken into account by the former, thus making policy interventions futile. There are many arguments against this view besides the unreality of own-model-generated expectations. Models differ widely among the population, and noise elements obscure common signals.

In another line of argument, Lucas asserts (without testing) that parameters in macroeconomic models are functions of policy instruments: when instruments are changed, people's reactions will automatically change and possibly negate the intended changes sought by the policymakers. Of course, variable parameters have long been investigated in macroeconomic models. Either they contribute to nonlinearities, which can be unfolded back to stable parameters, or they are random parameters, or they vary according to a wide range of factors that are not necessarily closely related to policy instruments.

Lucas' approach is a contrived theory that is known, in advance, to recommend noninterventionism. It is not known, however, to be related at all to the actual formation of expectations.

A difficulty with the modern approach is that if it does not automatically make policy intervention futile, it does make it very indefinite depending on just how the expectations are generated by the own-model.

A verifiable theory of expectations can, however, be measured, estimated for model use, and studied for policy response that turns out to be more definite, apart from the fact that the findings are subject to error and should be surrounded by confidence intervals.

Macroeconomic Policy

Keynesian macroeconomic theory was originally developed as a basis for policy intervention to save the Western market economies that had lapsed into severe recession in the late 1920s and 1930s. Apart from the ill-considered stagnation thesis and other longer-range perspectives, the theory was reasonably well conceived for the recovery policies promoted through fiscal stimuli. Classical-minded economists warned against inflation, but that concern was not actually realized until the major economies had recovered to high employment levels for a sustained period—during the reconstruction phase after the Second World War. Both tax and expenditure policies were actively used, and monetary policy was introduced as interest rates were allowed or encouraged to rise. An early bias in favor of fiscal rather than monetary policy for stabilization, among Keynesians, was soon abandoned, and both were used in tandem. In addition, econometric simulation of models soon gave evidence that fiscal policy needed to be accommodated by monetary policy; otherwise a fiscal stimulus or restraint would peter out or turn perverse. There was no problem among neo-Keynesian theoreticians and econometricians in developing the analysis in this direction and seeking *balanced* policies in which fiscal and monetary policies reinforce each other.

Just as there was little concern about inflation as long as there was significant excess supply during the 1930s, there was little concern about international economic affairs—particularly exchange-rate movements—as long as the Bretton Woods agreements were in effect during the 1950s and 1960s. Inflation and flooding the world with dollars as a result of poor policy implementation of Vietnam War finance caused the Bretton Woods system of fixed parities to break down. During the 1970s, policymakers had to cope with supply-side shocks from food and fuel prices, general inflation, and fluctuating exchange rates. A major policy prescription was to control inflation through the use of social contracts—incomes policies. Here we came to a standoff in terms of economic analysis of policy. The monetarists argued that strict control of the money supply was the *only* way to deal with inflation, while many Keynesians argued for incomes policies.

Monetarists argued that incomes policies never worked, and, at the end of the 1970s, policies of orthodox monetary tightening were adopted, both in the United States and in Europe—in Germany on the continent, and in Britain. It should be noted that Austria had successfully used an incomes policy and held inflation in check while growing nicely.[6]

Monetarism prevailed and brought down inflation, but the cost was very high. The world went through the severest recession of the postwar period. Apart from the fact that incomes policies were not given a proper trial, the blunt application of monetarist policy failed to give appropriate note to the decline in commodity prices that began in 1981-82. The United States and other countries had adjusted to the change in the terms of trade for energy. Conservation was impressive, new discoveries were made, and interfuel substitution relieved many shortages. These developments started to break the power of OPEC. Grain supplies became ample and commodity prices, besides fuel and food, leveled off or declined. Belatedly, the U.S. Secretary of the Treasury and Federal Reserve governors recognized that inflation is closely related to world commodity price movements, but not in time to save the world from overkill in 1981-82.[7]

Economic policy in the 1970s and 1980s became much more market-oriented. Economists became conservative, turning away from intervention, along the lines of Lucas and the monetarists, and towards deregulation. The point was repeatedly made that the market process led to better judgments than could be implemented by government policymakers. This stalled and eventually killed an effective energy policy. It was used as an argument against industrial policy, which had been so successful in the expansion of the Japanese economy during the 1960s. In general, the economics profession rejected indicative planning or industrial policy and argued for deregulation. There was near-complete deregulation of airlines and partial deregulation in the financial sector, particularly in banking. While there have been some impressive gains from deregulation, there also have been some very serious side effects. A lax attitude towards regulation, encouragement of deregulation, and implicit faith in the power of the market have contributed to a severely weakened financial sector, hundreds of bank failures, excessive activity in mergers and acquisitions, large debt structures to burden corporations, and an atmosphere that culminated in Black Monday (October 19, 1987).

It is not difficult to see why politicians might be willing to take risks and expose the economy to increased variability and fluctuation. They come and go, according to voter preference, but it is not easy for me to understand why professional economists have become so tolerant of policies that lead to social risk.

The macroeconomy is presently functioning well in terms of GNP growth, unemployment, inflation and other indicators. The laity and politicians claim that it is, in fact, a period of prosperity. But the economy has reached this position amid serious problems—the internal and external deficits, widely fluctuating exchange rates, an exposed banking system, debt burdening some of our most important trading partners in the Third World, and high-level unemployment in Europe. These foreign problems loom large for us because the United States is so involved in the world economy. In making projections of the ability of our government to overcome the internal fiscal deficit position, steady growth at a modest level is assumed to occur, year after year, as though the business cycle had been outlawed. Such presumptions have proved to be disastrous in the past, yet economist advisors to policymakers are loathe to forecast cyclical downturns, in spite of the fact that they have occurred with a fair degree of regularity for more than 100 years.

Keynesian macroeconomic theory was designed to deal with a cyclical depression, and dynamic extensions of the theory generated regular cycles. It was recognized at an early stage that credible forecasts would have to be made in order to implement the theory's policy recommendations. The forecasting techniques are difficult, frustrating, and tedious, so much so that practitioners frequently give up and profess to "feel their way" blindly in reacting to events. I agree that forecasting is difficult, but I feel that it cannot be avoided. The reactive procedure will consistently be too slow to be effective. Both contemporary policy (one year or shorter horizon) and medium-term policy (up to 10 years ahead) require a forward look in order to make policy fit the factual situation when it is realized. It is true that unreliability remains, but econometric forecasting methods are our only tools, and they do work at least as well as personal judgment. In the short run, they provide fairly accurate guidelines. In the longer run, two procedures come to our assistance. Rolling forecasts should be made for the medium-term horizon. Every quarter or year, forecasters should update a new look-ahead for an extended horizon. The medium-term outlook will change, but this procedure should enable policymakers to stay in touch with the changing situation.

Naturally, the further ahead one tries to forecast, the less certain is the *point* forecast. The error band may grow substantially—enough to discourage the policymaker—but all economic choices must be made in an environment disturbed by "noise" in the economy. The situation can be made more manageable by discounting both the point projection and the error or confidence bands back to the present. If the discount factor is, let us say, 10 percent and if the error bands grow no faster than 10 percent annually, the margin of uncertainty can be restrained to a manageable interval that is not so large as to render the policymaker uncomfortable.

Will economic forecasts ever improve? Paul Samuelson once remarked that we have perhaps reached the asymptotic level of precision that we can expect to attain.[8] This may be true, but Stephen McNees has indicated that the precision of macroeconometric forecasts has gradually improved between the 1950s and 1980s, with a detour during the turbulent 1970s.[9] No other systematic study over such a long time has ever been made, but the record for macroeconometric forecasts does look promising; that is another reason why I say that this is our only tool.

We have tried using sample surveys, quarterly instead of annual data, time series models (ARIMA or VAR), but nothing has shown the decisive power of a breakthrough. I do not think that models with own-generated expectations will produce impressive results. There is, however, some hope in this information age. The use of high-frequency data, which are becoming plentiful at daily, weekly, and monthly intervals, looks promising. There is inherently much serial dependence in economic data, and this can be exploited for short-run forecasts, from one to six months ahead. These short-run forecasts can then be used to calibrate or adjust quarterly econometric models of the economy as a whole. An objectively adjusted model that combines time series information at high frequency with the customary frequency of prevailing models may well lead to significant improvement of forecasts.[10] Further use of sample survey information in connection with modeling may enhance such improvements. This is the most promising lead at the moment for improving economic forecasts and making policy formation sounder.

I noted earlier that the Keynesian theoretical model was fully compatible with both monetary and fiscal policy, although early on there was an emphasis on fiscal policy. By contrast, classical monetarism would rely exclusively on monetary policy and use the tax system simply to collect enough revenue to pay for necessary government expenditures, which, according to their tastes, should be as small as possible.

The Keynesian mixture of monetary and fiscal policy should aim for balance. In this way the economy is not disturbed, and the burden of macroeconomic adjustment is equitably distributed.

At the beginning of the 1980s a new brand of economic policy came on the scene, labeled "supply-side economics." The theoretical base for this policy was simply conventional, neoclassical economics. The theoretical base was not at fault, but it was carelessly applied to the problems at hand. Proponents claimed that if tax rates were lowered, there would be a surge of activity that would bring in more taxes and keep fiscal policy in budgetary balance. On the supply side it was argued that people would save more and work harder if marginal tax rates were lowered. Another aspect of this brand of supply-side economics was the assertion that deregulation would improve economic efficiency. The end result of supply-side fiscal policies would be to avoid a recession and implement an anti-inflation program. As a novel theory for accomplishing all these fine macroeconomic objectives, this populist type of supply-side economics has been fully discredited. Savings did not rise; federal budgets were not balanced; productivity did not improve more than usual; and, as was mentioned already, the quality of deregulated services deteriorated—so much so that financial services' activity nearly brought on a world crisis of enormous dimensions.

Another consequence of ill-considered applications of supply- side economics was that policy became seriously unbalanced. The strong reduction in tax rates created such a fiscal deficit that monetary policy was the only viable tool for maintaining economic order. This pressure on the application of monetary policy led to extremely high interest rates, which imposed a severe burden on the housing market and on borrowers in the developing world, not to mention bank lenders both abroad and at home—particularly in sensitive markets such as Oklahoma, Texas, and California.

These unbalanced policies induced a recovery from the recession, and the ensuing deficits were so large that the ordinary Keynesian stimulus of deficit spending led the U.S. economy into a decent revival in the real sector. Financial sectors remained in disarray in the aftermath.

Macroeconometric methods monitored this policy path quite well; that is why Stephen McNees' findings show improved forecasts in the 1980s. These methods also indicated at the very beginning that tax coefficients associated with saving, labor supply, and investment would not be strong enough to bring about the gains being sought for savings and work effort.

The deregulation aspect of supply-side economics, as it is being practiced, relies increasingly on the market mechanism. This is not necessary for emphasis on the supply side, for industrial policy provides a supply-side approach to policy, without being free-market-oriented. Similarly, the structure of centrally planned and developing economies focuses on the supply side, but the presence or absence of free markets is irrelevant. In their treatment of the debt problem in the Third World, U.S. authorities since 1980 have tied concessionary treatment to the ideology of free-market economics. Although this approach has not yet been successful, many economists support the free-market emphasis.

In official negotiations with centrally planned economies in Europe and Asia, both the United States and other Western countries have strongly supported economic reforms that introduce more market mechanisms. These events have much to do with political economy, but they are also related to the subject of this paper, namely, developments in macroeconometrics. Macroeconometric model building is very much alive in China and is used to throw light on the reform process. It is also used in Eastern Europe—especially in Poland and Hungary—although the effort there is academic rather than oriented towards official policy. The macroeconometric modeling of the USSR was originally done in the United States and Japan, but it does seem to be gaining ground now in the Soviet Union as a result of reform efforts.[11]

These models track the economy well and show the inflationary pressures. In North America and Europe, as well as in Japan, Australia, and New Zealand, the paradigm model is in the IS-LM framework, with the usual challenges from the monetarist side. Small macro models can be constructed directly from aggregative data to produce IS-LM graphics, or large scale models can be reduced to *maquettes* that show the same kinds of core features. It is my contention that in a statistical context, the IS-LM paradigm crowds out the monetarist paradigm for industrial countries.[12]

It is difficult to specify the corresponding paradigm for the centrally planned or developing economy, but it does appear that the two-gap model shows great promise. Chinese colleagues have succeeded in constructing small two-gap econometric models of China. This is interesting because China covers, simultaneously, the centrally planned and the developing country cases. Oddly enough, the two-gap model with a monetarist-type equation for price-level determination seems to fit the Chinese data very well and deal with the present tendency towards high inflation rates. This paradigm can be used to analyze Chinese economic policy by showing the effects of importing, exporting, and exploiting technical progress for growth. It could be extended to deal with such financial matters as foreign-exchange-reserve position, debt-service burden, and other problems related to financial capital flows. Model building was feasible in the Soviet Union and Eastern Europe before the onset of price reform. Technological relationships, choice subject to given prices, and foreign trade fit well into regular statistical patterns, but liberalization in Hungary opened the way for more extensive macroeconometric work. Under *perestroika* this development should be accelerated. Since 1978, China has been liberalizing, producing more meaningful data, and starting to construct usable models. Early versions were built by Lawrence Lau, but by now Chinese econometricians have acquired the capability, and macroeconometric models of China are likely to be as much in evidence as those of Poland, Hungary, and Yugoslavia. Since many of the China models will be built in state agencies, they will have the potential for use in macroeconomic policy formation.

Some Methodological Developments

There are two modern tendencies in macroeconometrics. One is the fascination of a younger generation with model-consistent expectations, that is, expected values are generated by the very models that are being estimated. This is being done on a large scale, but we have yet to see how such values stand up under the stern test of forecasting ability. They have no "track-record," and this must be established in order to gain credibility. Many U.K. models now have model-generated expectations and will eventually build up a track record.

A second tendency is to turn to time series analysis without much input from economics. I stressed earlier that econometrics is based on three disciplines, one of which is *economics.* When Tjalling Koopmans wrote his celebrated review of Burns and Mitchell's treatise on business cycle analysis, he meant by "theory" both economic and statistical theory.[13] The modern reliance on time series analysis leans heavily on statistical theory but is nearly empty in the field of economics.

Time series analysis can be interesting in searching for and describing relationships or hypotheses about the macroeconomy, but it is awkward to apply this methodology to scenario or policy analysis. The VAR versions assume that all variables are endogenous. How well will this kind of theoretical specification serve us when there is an oil embargo or similar supply-side shock? Will it survive an event like the breakdown of Bretton Woods? This is not to say that the mainstream structural model can foresee such momentous events, but once an event occurs, the structural model is readily capable of analyzing its effects.

Structural model builders in macroeconometrics should not ignore the powerful contributions of time series analysis, but they should not replace structural models by pure empirical analysis. The combination of high-frequency time series analysis with mainstream structural quarterly models, cited above, is but one way of combining time series with structural models. In fact, E. Philip Howrey of the Michigan Model group recommends forming a weighted average of a small monthly (VAR) time series model with a structural (Michigan) econometric model. There are other fruitful possibilities for drawing on time series analysis, such as the updating of parameter estimates as a sample evolves through time.

Concluding Remarks

Early in my professional career, I was impatient with the resistance to the introduction of Keynesian macroeconomics. I also enjoyed confrontation with established nonmathematical economists who resisted the introduction of mathematical methods and econometrics for the study of our subject. Eventually, the quantitative approach triumphed, and econometrics *cum* mathematical economics became common practice.

I feel uncomfortable now resisting some changes in macroeconometrics. Much of the new work is very good, although I find it hard to perceive a true breakthrough in the vast volume of research material that is being published. My problem is with the sterility of those aspects that have become very popular and enthrall young, fertile, productive minds without offering clear advancement of the science.

NOTES

1. L. R. Klein, "The Supply Side," *American Economic Review* 68 (March 1978), 1-7.

2. J. M. Keynes, *The General Theory of Employment, Interest and Money* (London: Macmillan, 1936).

3. G. L. S. Shackle, *Expectation in Economics* (Cambridge: Cambridge University Press, 1952), 2nd ed.

4. M. Hashem Pesaran, *The Limits to Rational Expectations* (Oxford: Blackwell, 1987).

5. Morten Jensen and Morten Jonassen, *The Formation of Household Expectations—A Test on Norwegian Cross-Sectional Survey Data*, The Bank of Norway, July 1986.

6. Alois Guger, "Einkommensverteilung and Verteilungspolitik in Osterreich," *Handbuck der Osterreichischen Wirtschaftspolitik*, ed. by H. Abele, E. Nowotny, S. Schleicher, G. Winckler (Wein: Manz, 1989), 1-17.

7. Wayne D. Angell, "A Commodity Guide to Monetary Aggregate Targeting" presented to the Lehrman Institute of New York, Federal Reserve Board, Washington, DC, December 10, 1987. At the annual meetings of the IMF and World Bank, Secretary Baker had recommended use of commodity price monitoring for exchange rate stabilization.

8. P. A. Samuelson, "Art and Science of Macromodels over 50 Years," *The Brookings Model: Perspective and Recent Developments*, ed. Gary Fromm and Lawrence Klein (Amsterdam: North-Holland, 1975), p. 7.

9. Stephen McNees, "The Accuracy Keeps Improving," *New York Times* (January 10, 1988), p. F2.

10. E. P. Howrey, "New Methods for Using Monthly Data to Improve Forecast Accuracy," to be published in a volume by Oxford University Press, 1989. L. R. Klein and E. Sojo, "Combinations of High and Low Frequency Data in Macroeconometric Models," to be published in a volume by Kluwer, 1989. Carol Corrado and Jane Haltmaier, "The Use of High-Frequency Data in Model-Based Forecasting at the Federal Reserve Board," paper presented at the AEA meetings, Chicago, December 29, 1987 (together with versions of the cited papers by Howrey and Klein-Sojo).

11. Donald W. Green and Christopher I. Higgins, *SOVMODI, A Macroeconometric Model of the Soviet Union* (New York: Academic Press, 1977). Haruki Niwa, "An Econometric Analysis and Forecast of Soviet Economic Growth," *The Prediction of Communist Economist Performance*, ed. P.J.D. Wiles (Cambridge: Cambridge University Press, 1971), 339-72.

12. Lawrence R. Klein, Edward Friedman, and Stephen Able, "Money in the *Wharton Quarterly* Model," *Journal of Money, Credit and Banking* 15 (May 1983), 237-59.

13. Tjalling C. Koopmans, "Measurement without Theory," *Review of Economics and Statistics* 29 (August 1947), 161-72.

During the past forty-five years **JAMES TOBIN** has focused on and made major contributions in the fields of macroeconomic theory, monetary theory and policy, portfolio theory, economic growth, and consumer behavior.

Professor Tobin holds A.B., M.A., and Ph.D. degrees from Harvard University. He has been awarded more than fifteen honorary degrees from U.S. and foreign universities. After spending two post-doctoral years at Harvard as a Junior Fellow and a year at Cambridge University, Dr. Tobin joined the faculty of Yale University in 1950. At Yale he served for seven years as Director of the Cowles Foundation for Research in Economics and for five years as Chairman of the Economics Department. On leave from Yale, Dr. Tobin was a Visiting Researcher at the Survey Research Center of the University of Michigan, a member of President John F. Kennedy's Council of Economic Advisers, Visiting Professor at the Institute for Development Studies of the University of Nairobi, Visiting Professor at the University of Minnesota, and Ford Visiting Research Professor at the University of California, Berkeley.

Dr. Tobin is a past president of the American Economic Association, the Econometric Society and the Eastern Economic Association. In 1955, he was awarded the John Bates Clark Medal by the American Economic Association. He is also a fellow of the American Academy of Arts and Sciences and the American Statistical Association and a member of the National Academy of Sciences and the American Philosophical Society. Dr. Tobin has been a consultant to many agencies including the Board of Governors of the Federal Reserve System, the U.S. Treasury Department, and the Congressional Budget Office. Dr. Tobin is presently a member of the Board of Trustees of the Joint Council on Economic Education and the Twentieth Century Fund and is a member of the National Academy of Sciences' Committee on the Status of Black Americans.

Professor Tobin has written several books and monographs as well as over 300 articles in journals and books. Titles of Dr. Tobin's books that are indicative of his work include: *National Income Policy, Financial Markets and Economic Activity, Essays in Economics: Macroeconomics, The New Economics One Decade Older, Essays in Economics: Consumption and Econometrics, Essays in Economics: Theory and Policy,* and *Macroeconomics Prices and Quantities.*

4

Dr. James Tobin
Sterling Professor of Economics
Yale University
Recipient of the 1981 Nobel Prize in Economics

I am pleased and honored to participate once again in this valuable series, and I congratulate the University and my hosts on its 25th birthday. The topic this year is very broad. I shall confine myself to the area of my greatest interest and experience, macroeconomic theory. It happens also to be the arena of the liveliest controversies over substance and methodology during the past 20 years. I suspect that the revolutions or counterrevolutions in macroeconomics may be the principal reasons that the organizers of this series invited the speakers to comment on the present state of economic science.

Even macroeconomic theory is too general for one essay. After a brief methodological introduction, I shall narrow my focus to a specific substantive issue, to what in my opinion is *the* fundamental issue of macroeconomics: the existence, reliability, strength, and speed of adjustments by which a market economy maintains or restores economywide equilibrium between the supplies of labor and of the products of labor and the demands for those services and goods. A half century ago, during the Great Depression, intense debate on this issue split the economics profession between John Maynard Keynes and the revolutionaries he inspired, on the one side, and the defenders of established orthodoxy, on the other. Today the same battle is rejoined, and the same ground is contested with more powerful ammunition. Those who, like me, were young rebels in the 1930s are now, as this essay will make all too clear, on the defensive. Whatever you may think about the merits of the controversy, you may find it interesting to relate the new debate to the old one on the same issue.

I begin with a general discussion of methodologies, old and new, in macroeconomics.

The "Micro Foundations" Methodology of Modern Macroeconomics

Macroeconomics has been a distinct field in economic theory only since Keynes's 1936 book, *The General Theory of Employment, Interest and Money*. The word "macroeconomics" itself, adopted to distinguish the study of economies as a whole from the study of households, businesses, markets, and sectors, "microeconomics," is of even more recent vintage. The central paradigm of economic theory begins with "micro" as the calculus of rational self-interested behavior by individual decisionmakers. They determine their supplies of and demands for multitudes of commodities by maximizing their incomes of their utilities subject to the constraints of their resources. Competitive markets coordinate their choices, balancing demands for all commodities with supplies. Prices adjust to clear all markets. Through the responses of rational economic agents to these price signals, markets transmute micro selfishness and myopia into optimal social allocations of resources—as if by an "invisible hand," to quote the famous metaphor of Adam Smith. The theory of general equilibrium, perhaps the most impressive intellectual structure in social science, gives rigorous content to Smith's intuitive conjecture. It purports to build the whole economy from the behaviors of individual agents. But it is a framework of analysis, rather than a source of specific conclusions about the signs and magnitudes of relationships among economic variables—e.g., price and income effects on demands and supplies or effects of taxes on prices and quantities.

The shortcuts and simplifications of macroeconomics were and are the inevitable costs of getting interesting and testable propositions, of which full-blown general equilibrium theory is virtually empty. From

Keynes on, macro model builders relied on the standard paradigms of neoclassical theories of the behavior of individual agents in specifying their behavioral equations. But Keynes and his successors had to use information and hypotheses about behavior other than the implications of optimization theory. They could appeal to empirical observation, or to hunches about plausibility, to place restrictions on individual behaviors. Moreover, aggregate relationships are the results of diverse behaviors of multitudes of individual agents; a structural macro equation combines assumptions about individual behavior and assumptions about aggregation. Macro modelers also inject realism about the institutions and economic structures of the economies they are describing. Those economies did not, do not, conform to the assumptions of highbrow general equilibrium, for example, perfect and complete competitive markets.

Pure theorists naturally found macro models aesthetically unappealing and intellectually confusing. They criticized macro relationships as *ad hoc* because they were not explicitly derived from "first principles," i.e., from optimizations by individual decisionmakers. "Micro Foundations!" was the rallying cry of the methodological counterrevolution against Keynesian economics, really against all macroeconomics. Its protagonists complained of the absence of explicit derivations of macro behavioral equations from optimization; they proposed to build a new macro solidly and clearly based on individual rationality. Only relationships with those micro foundations, they said, could be expected to be stable over the range of applications—not just forecasts but also conditional estimates of the effects of policy interventions and other exogenous variations—to which macroeconomics aspires. This viewpoint has swept the profession.

After 15 or 20 years of methodological counterrevolution, where do we stand? "What you gain on the swings you lose on the roundabouts." Aggregation is a tough problem, so it is just finessed. It is easy to display explicit micro foundations when you assume the whole private economy can be represented as one agent, Robinson Crusoe, or as two who differ only in age and endowment, operating in competitive markets with flexible prices. But of course there are no transactions in these markets (except in the overlapping generations model once every two-period lifetime). The immense volumes of transactions we actually observe in markets for assets and commodities are simply not explained. No heed is paid to all the problems of coordination and communication which concerned Keynes and other macro theorists—the differences between savers and investors, lenders and borrowers, bulls and bears, risk-lovers and risk-averters, and so on.

Why the "representative agent"—Robinson Crusoe—is a less *ad hoc* and more defensible simplification than the dirty constructs of earlier macro modelers, and of today's macroeconometricians, is beyond me. I note some biases to which this methodology leads. The single-agent abstraction makes social welfare identical with the welfare of the individual agent. It excludes by definition any discrepancies between individual and social optima, in particular the deadweight losses due to involuntary unemployment, the market failure that motivated macroeconomics at its origins 50 years ago. The methodology treats government as an alien player in a two-person game with the anthropomorphic private sector, a game in which the government incomprehensibly tries to throw the private sector off its optimal solution while the private agent tries to outwit the evil or idiot policymaker. These biases work in a conservative and Panglossian direction.

I exaggerate. An increasing number of theoretical papers using the new methodology attempt to model setups in which things do not work out for the best and in which government may even play some beneficently corrective role. I note, however, that this role is seldom a Keynesian one, because the distortion the government can correct is seldom a failure of markets to clear. Moreover, because of the methodology those papers are, like the ones that glorify the invisible hand, logical exercises rather than models that seriously try to describe real-world economies.

Even the individual's optimization problem is simplified and specialized in the interests of analytic tractability. Utility and production functions take parametric forms. By convention, equations are linear or log linear or are so approximated. The whole point of "micro foundations" is to find stable relationships that survive policy variations, exogenous shocks, and the passage of time. But we have no basis for empirical confidence that an individual's utility function, for example, remains the same over her life, independently of her actual experience and environment. We certainly have no basis for assuming a utility function with a constant rate of relative risk aversion as a stable basis for both intertemporal choices and choices involving risk.

In journals, seminars, conferences, and classrooms, macroeconomic discussion has become a babble of parables. A macro theorist has become a story-teller who constructs a mythical economy in which institutions and environments conspire so that rational behavior in price-cleared markets comes up with observable outcomes that in one or two respects conform to stylized facts of the real world. In other respects, however, there is no resemblance of the mythical economy to the real world. Consequently there is a big gulf between academic macroeconomics and the macroeconomics oriented to contemporary events and policies.

Price Rigidity—The Essential Basis of Keynesian Macroeconomics?

Keynesian economics, at least old-fashioned Keynesian economics, is almost always described as dependent on *nominal price rigidity.* (The word "price" may be interpreted generically, to include nominal wage rates.) Whether the crucial rigidity characterizes labor markets or product markets or both is an interesting but secondary issue. In any case, nominal price rigidity is said to be necessary to enable monetary policies and other nominal macroeconomic shocks to affect real aggregate demand, in particular to cause real aggregate demand to deviate downward from real aggregate supply.

I could document the prevalence of this interpretation of Keynesian economics by quoting from textbooks, old and new, Keynesian and anti-Keynesian. I prefer to quote from a recent paper by three young stars of the American economics profession:

> In the early 1980s, the Keynesian view of business cycles was in trouble. The problem was not new empirical evidence against Keynesian theories, but weakness in the theories

> themselves. According to the Keynesian view, fluctuations in output arise largely from fluctuations in nominal aggregate demand. These changes in demand have real effects because nominal prices and wages are rigid. But in Keynesian models of the 1970s, the crucial nominal rigidities were assumed rather than explained—assumed directly, as in disequilibrium models, or introduced through theoretically arbitrary assumptions about labor contracts. Indeed it was clearly in the interests of agents to eliminate the rigidities they were assumed to create. . . . Thus the 1970s and early 1980s saw many economists turn away from Keynesian theories and toward new classical models with flexible wages and prices.[1]

I quote from this paper because the authors profess sympathy for Keynesian economics and propose to overcome its theoretical flaws by deriving rigidities from "micro foundations," that is, from rational optimizing behaviors of individuals. They style themselves "New Keynesians."

These writers, and many others of their generation, accept the methodology of the neoclassical counterrevolution, but they are impressed by the evidence that Keynesian macroeconomics fits empirical observations better than new classical business cycle theories. After all, Keynesian economics was originally inspired by the Great Depression, for which the orthodoxies of the day had no explanations and no remedies. I believe that the depth and duration of the two most recent recessions, 1974-75 and 1979-82 (longer in Europe), have similarly helped to discredit the revival of these classical orthodoxies a half century later.

Laudable though the New Keynesians' research program is, I shall argue that it is misguided. It is based on a misunderstanding of Keynes himself and of old Keynesian economics. The New Keynesians have accepted the terms of the debate formulated by the anti-Keynesian "new-classical" counterrevolutionaries. Both sides of the contemporary debate misrepresent and exaggerate the role of price rigidity and of nominal (as opposed to real) demand shocks in Keynesian macroeconomics.

Do Flexible Prices Fully Absorb Demand Shocks Instantaneously?

First, John Maynard Keynes, in his *General Theory,* did not postulate price rigidity, or even, money wage rigidity, in the ordinary common sense meaning of the word. It is true that some teachers and some writers of elementary textbooks drew backward L's in output/price space or employment/money-wage space. The wage or price is constant below full employment or full employment output. At those values aggregate labor and product supplies become perfectly inelastic, vertical in those diagrams. The *General Theory,* Book V, says that price will rise relative to money wage as output and employment increase, because the real wage follows marginal productivity down. (In postulating diminishing marginal productivity and countercyclicality of real wage rates, Keynes was leaning over backwards to be classical. The proposition was challenged on empirical grounds almost immediately. Keynes accepted the criticism and observed correctly that his general case was strengthened if expansion could occur without declines in real wages.) The same Book V anticipates that the money wage itself will rise as aggregate employment approaches full employment.

What is true is that Keynes and Keynesians did not expect the aggregate supply curve, plotting price against real output, to be vertical within the short run for which the Keynesian model applies. That short run they surely regarded as conditioned by the price and wage determined in previous periods.

Keynes and Keynesians used what Sir John Hicks has called the *fix-price method* as an expository device. The calculus of effective demand—spending propensities and multipliers—was a major innovative contribution of Keynesian economics, anticipating by 30 years the "disequilibrium economics" of Barro and Grossman and of the French school, Benassy, Grandmont, and Malinvaud. The variables in this calculus are real quantities, output flows and their components. It was convenient to keep effects on and of prices to one side during the exposition, and it was valid so long as prices were not completely and instantaneously clearing markets. This expository device, taken literally, doubtless contributed to the mistaken impression that absolute rigidity of prices was a necessary assumption.

The second point is more basic. The critics of Keynesian theory, friendly new Keynesians as well as hostile new classicals, take it for granted that if prices were flexible—that is, not rigid as they allege Keynes assumed—then there could be no departure at all from the real equilibrium, no departure even in the shortest run. Flexible prices would instantaneously and continuously clear all markets, for products, labor, and financial instruments. No involuntary unemployment could ever arise, no undesired excess capacity, no gap between actual and potential GNP.

The formal story is that the Walrasian Auctioneer receives all the multicommodity supply and demand schedules of the agents, including those of the monetary authority and other policymakers. These schedules refer to intertemporal as well as contemporaneous contracts and transactions. The Auctioneer, presumably using a super-computer yet to be designed and built, solves the equation system, generates the market-clearing price vector, and informs the participating agents of the transactions they have made at those prices. The next day, or the next hour, or really the next microsecond, the awesome feat is performed anew.

In this interpretation, flexibility of prices in response to shocks, and in response to news, occurs instantaneously. Prices *jump* to their new market-clearing values discontinuously, without the passage of clock time. A graph of a price time series would show discontinuities. This is certainly not what is meant by *flexibility* in common parlance. And it certainly does not take anything like *rigidity* in the common meaning of that word to believe that demand shocks will cause output to deviate, at least temporarily, from the "AS" schedule.

Anyway, if imperfect or monopolistic competition is assumed, rather than Walrasian pure competition, a Walrasian Auctioneer solution would not even exist.

Fifty years ago, no economists denied that demand shocks could at least temporarily affect output, in individual markets and in the economy at large. Keynes did not regard this possibility as problematic, and neither did his "classical" opponents. No one took the continuous competitive multimarket-clearing scenario as anything but an illustrative demonstration that in principle the system was self-consistent and solvable. As

Joseph Schumpeter, a great economist at whose feet I sat, put it, Walras's theory was the *magna charta* of economists, giving us the license to proceed in the knowledge that our quest for coherence was not a fruitless one. It was the beginning of the search, not the end. It was not then, as it seems to be now in theoretical circles, a point of reference from which any alleged departure bears the burden of proof. That is a Panglossian presumption, biasing our profession to the view that free markets are the best of all possible worlds.

Fifty years ago, and earlier, price theorists worried about *false trading.* Walras and Marshall envisaged temporary disequilibria in individual markets. Prevailing prices do not always clear the markets. They postulated dynamic rules of price adjustment (Walras) or quantity adjustment (Marshall) that would normally, but not invariably, bring supply and demand together. Stability of *general* multimarket equilibrium was especially problematic. "False trading" was recognized as a possible source of prolonged disequilibrium. Trades made at nonmarket-clearing prices change the endowments of the market participants, and thus alter their supply and demand schedules. A fashionable current term for effects of this kind is "hysteresis," a generic name for situations in which the nature of a system's equilibrium is not independent of the path the system takes when it is out of equilibrium. These problems have not been solved by later generations of theorists. They have simply been ignored, and replaced by firmer reliance on the great Auctioneer.

False trading and similar phenomena make it difficult for agents to learn the structure of the markets in which they are participating accurately enough to form rational expectations. The observations generated by disequilibria cannot teach the participants the equilibrium structure. Imagine a group of non-English-speaking foreigners from all over the world trying to learn English simply by conversing with each other.

Fifty years ago, the macroeconomic disagreement between Keynesians and classicals concerned this point. A shock occurs and takes the economy away from equilibrium. Unemployment arises, Keynesian involuntary unemployment. Would endogenous movements of prices and other macroeconomic variables return the economy to the equilibrium from which it was jarred? Does the capitalist-market economy have reliable and quick mechanisms of adjustment?

The classical economists thought there were effective stabilizers. Keynes thought there were not. Sometimes, on some pages, he argued that there were none at all. In Book I of his *General Theory,* he envisages a whole family of equilibria, not just the classical full-employment equilibrium, but many aggregate demand equilibria with involuntary unemployment, equilibria not escaped or escapable by adjustments of prices. This indeed is the meaning of ''general'' in his title. Although he modifies his opening statement of his theory in later chapters, particularly chapter 19, his overall theme stands: the natural endogenous adjustment mechanisms cannot be counted on. That is why Keynes regarded macroeconomic interventions by government as essential.

The question, as Keynes saw it, was whether reductions in wages and prices would increase aggregate demand, and thus take the economy to full-employment equilibrium. His answer contained two strands. First, nominal wages would not fall rapidly in response to excess supply of labor. This strand is the one that sticks in the memory of the profession, exaggerated into assumed wage or price rigidity. Second, even if wages, and with them prices, were flexible, deflation would not increase aggregate demand and eliminate unemployment and underutilization of capital. This is the stand the profession has forgotten or neglected.

The Origins of Wage Stickiness in Keynesian Theory

I will say something about the first strand, although it is not my central topic here. It is routinely and unquestioningly supposed that Keynes attributed ''money illusion'' to workers. Neoclassical theorists therefore dismiss Keynesian theory out of hand. Often Keynesians accede to the charge but defend it on grounds of realism. I have come to believe that Keynes's argument is free of the taint. And although it is not logically tight, it can be made so. Let me explain.

You will recall that Keynes's workers were willing to accept a cut in *real* wages achieved by an increase in the price of wage goods. Yet they were not willing to take a cut in money wages. Keynes's reason for this asymmetry is theoretically impeccable and at the same time realistic. Workers are concerned primarily with relative wages, with how their pay compares with those to whom they regard themselves at least equal in merit. Labor markets are disaggregated and desynchronized. To any single worker or local group, a nominal wage cut appears to be a loss in relative wages; there is no assurance that others will also take cuts. On the other hand, an increase in the cost of living is the same for everybody. Workers may be perfectly prepared to receive lower real wages with unchanged relative wages, but labor market institutions give them no way to communicate this willingness.

That real wages are too high is the time-worn orthodox explanation of unemployment. If labor unions or government regulations keep them too high, unemployment is classical, not amenable to remedy by demand expansion. There is an identification problem, because the same observable symptoms are consistent with different causes. Keynes agreed that it is likely that real wages are in depressions above their full employment values. But, he argued, that is not the same thing as saying they are rigid at their high depression values. Just try expanding demand, and you will see that profit margins can be expanded and real wages reduced as necessary to make higher employment profitable to employers.

As I observed above, recovery may not require lowering of real wages. But it is still true that the way to get higher employment is to raise aggregate demand, at the same time as money wages are stuck because of concerns for relative wage parity. Those concerns do not depend on money illusion. They are certainly not irrational. They are very human, and there is a great deal of empirical evidence of their importance.

The hole in the story in the *General Theory* is that it doesn't explain how the concerns of employed workers prevail when there are unemployed workers willing to work for less pay—real, nominal, or relative. The power of insiders vis-a-vis employers and outsiders evidently derives from the costs of turnover among members of an interdependent working team. Insider power is rightly the subject of considerable theoretical and empirical inquiry right now, for example by Assar Lindbeck and his colleagues in Stockholm. Labor economists have long observed that queues of jobseekers outside the factory gate have little effect on the wages paid to employees inside. Hard times do bring wage cuts, but generally through so damaging the financial and competitive positions of employers that they can credibly threaten layoffs of senior workers and even plant closings and bankruptcies.

Keynes did not squarely face the fact that the realistic descriptions of labor markets in his own argument were inconsistent with his assumption of pure competition in all markets. Wages are administered or negotiated prices. They are not set in impersonal auction markets. The same is true, of course, of product prices. Keynes did recognize that his theory applies to economies where the wages administered or negotiated are money wages. Things would be quite different with complete indexation.

The Weakness or Perversity of Price Effects on Aggregate Demand

The second strand in Keynes's basic argument was this: Even if money wages and prices were flexible, even if excess supplies of labor led to cuts in money wages, this flexibility would not prevent unemployment. Given a contractionary shock in aggregate demand, deflation of money wages and prices would not restore real demand to its full employment value. The classical market-clearing adjustment mechanism was, in Keynes's view, much too frail to bear the weight of macroeconomic stabilization. In fact, Keynes recommended stability rather than flexibility in money wages.

Two issues in this debate need to be distinguished. The first concerns the relation of real aggregate demand to the *price level.* The second concerns its relation to the expected *rate of change* of prices. In discussing them, I shall not distinguish between money wages and prices and their rates of change, but rather follow the assumption, conventional in this debate, that they move together. I remind you that the theoretical argument refers to a closed economy. You could think of the United States in years gone by, or of post-1992 Europe, or of the whole OECD area.

Keynes in Book I denied that real aggregate demand was related at all to the price and money wage level. In effect he turned the classical neutrality proposition against the classicals. If all money wages and prices are lowered in the same proportion, how can real quantities demanded be any different? Thus if a real shock makes real demand deficient, how can a purely nominal price adjustment undo the damage? Actually, Keynes himself provided an answer in chapter 19. If the nominal quantity of money remains the same, its real quantity increases, interest rates fall, and real demand increases. This scenario is often called the "Keynes effect." This mechanism would fail if demand for money became perfectly elastic with respect to interest rates—the famous liquidity trap—or if demand for goods for consumption and investment were perfectly inelastic.

Pigou and other authors provided another scenario, the "Pigou effect" or "real balance effect," which alleges a direct effect of increased wealth, in the case at hand taking the form of the increased real value of base money, on real consumption demand (possibly also on investment demand). This does not depend on reduction of interest rates.

The theoretical fraternity has taken the Pigou effect as a decisive refutation of Keynes's claim to have found underemployment equilibria. As long as involuntary unemployment and excess capacity push wages and prices down, there will be an equilibrium when and only when they reach so low a level that monetary wealth is so great that aggregate demand creates jobs for all willing workers.

The Pigou effect is of dubious strength, and even of uncertain sign. Most nominal assets in a modern economy are "inside" assets, that is, the debts of private agents to other private agents. They wash out in accounting aggregation, leaving only the government's nominal debt to the private sector as new wealth. Some, if not all, of that debt is internalized by taxpayers. The base of the real balance effect is therefore quite small relative to the economy—in the United States the monetary base is only 6 percent of GNP.

That inside assets and debts wash out in accounting aggregation does not mean that the effects of price changes on their real value wash out. Price declines make creditors better off and debtors poorer. Their marginal propensities to spend from wealth need not be the same. Common sense suggests that debtors would have the higher spending propensities; that is why they are in debt. Such a differential could easily swamp the Pigou effect. We're talking about gross amounts of 200 percent of GNP. I like to call this reverse Pigou effect a Fisher effect, because Irving Fisher emphasized the increased burden of debt resulting from (unanticipated) deflation as a major factor in depressions in general and in the Great Depression in particular. It is quite possible that this Fisher effect is stronger than the Pigou and Keynes effects combined, particularly when output and employment are low relative to capacity.[2]

The argument I have just made refers to *levels* of nominal wages and prices. An even more important argument refers to *rates of change.* The Keynes and Pigou effects compare high prices and low as if they were timeless alternatives, without worrying about the process of change from high to low in real time. Economists of the day argued in this way quite consciously, as required by the rules of the comparative statics game they were playing. The process of change works on aggregate demand in just the wrong direction. Greater expected deflation, or expected disinflation, is an increase in the real rate of interest, necessarily so when nominal interest rates are constrained by the zero floor of the interest on money. Here is another Fisher effect, another factor Fisher stressed in explanation of the Great Depression. Keynes stressed it too, as a pragmatic dynamic reinforcement of the lesson of his static general theory.

He was right to do so. In a 1975 article[3] I exhibited a simple macroeconomic system, classical in the sense that it has only one equilibrium, characterized by full employment, indeed by a "natural" rate of unemployment. Given the monetary base, the price level is stable in that equilibrium. The dynamic stability of the system depends on the relative strengths of the real balance effect and the real interest effect. If the real interest effect dominates, as it well may if the real balance effect is weak and certainly will if the Fisher debt burden effect prevails, then the equilibrium is unstable. Moreover, the system could be stable locally but unstable for large displacements.

I regarded my article as supporting Keynes's intuition that price and wage flexibility are bad for real stability. I wanted to shake the profession off its conventional interpretation of Keynesian economics, according to which unemployment arises only because of a dubious assertion of wage and price rigidity. I wanted to recall and reinforce the second strand of Keynes's argument, according to which unemployment is attributable to inadequate real demand, a deficiency that flexibility will not remedy. That is also what I am hoping to do here.

I am quite willing to subscribe to a meaning of *equilibrium* that excludes involuntary unemployment, and to characterize depressions as disequilibria. Either way, the Keynesian diagnosis and prescription are the same in practice.

Recently, at long last, the question whether price flexibility (in any sense short of the Walrasian Auctioneer fairy tale) is stabilizing has begun to receive serious attention. DeLong and Summers[4] have investigated it in the Taylor staggered-contract model, amended to allow price-level and price-change effects on demand. The Taylor model results in unemployment when there are new circumstances and information, because wages and prices cannot be immediately adjusted to them. It also allows Keynesian policies to work temporarily, because the authorities can react to new circumstances and information before existing contracts are renegotiated.

DeLong and Summers simulate increased flexibility by making the periods in the staggered-contract model shorter. They find that increased flexibility in this sense frequently does make real outcomes, employment and output, more volatile, not less. The reason is the same as in my model, the Fisher real interest rate effect of inflation and deflation. Their most interesting simulation has the intuitively desirable result that in the limit perfect price flexibility—instantaneous jumps of the Walrasian solution in response to shocks—does stabilize real variables perfectly. Close to this limit, greater price flexibility means greater real stability, but farther away from it, the reverse is true.

Nominal and Real Demand Shocks

I began by calling your attention to the caricature of the Keynesian theory of business fluctuations all too generally accepted in the profession. According to that caricature, fluctuations in real output and employment arise from shocks to nominal aggregate demand, which become real shocks only because prices are rigid. Tides ebb and flow; they matter to boats only because they pass over rocks.

Keynesian theory of business fluctuations stresses shocks to real aggregate demand—investment, consumption, or government purchases. Some impulses may indeed come from the monetary side, but that does not make them purely nominal. A monetary policy action that lowers nominal interest rates also lowers real rates and affects investment demand. Likewise, a shift in production functions that raises the marginal productivity of capital stimulates investment and diminishes the demand for money at the same time. The world is not constructed in the dichotomous way assumed in the common classification of shocks as either nominal or real.

The great achievement of the *General Theory* is the theory of effective demand. Keynes's insight was that demand is constrained by amounts actually sold in markets, which may frequently be less than the amounts agents would like to sell at existing prices. This was a deeper insight than the assertion that nominal wages and prices are "rigid." I commend it to the New Keynesians as a more fruitful and important line of inquiry than the macroeconomic role of the real costs of changing nominal prices on menus, price lists, and catalogs.

NOTES

1. L. Ball, N. G. Mankiw, and D. Romer, "The New Keynesian Economics and the Output-Inflation Trade-off," *Brookings Papers on Economic Activity* 1988. 1, pp 1-2

2. I have examined the macroeconomic consequences of a dominant Fisher effect, in an IS-LM model that also has a Keynes effect, in my *Asset Accumulation and Economic Activity*, Oxford, Blackwell 1980, chapter 1.

3. J. Tobin, "Keynesian Models of Recession and Depression," *American Economic Review* 65 (May 1975), 195-202.

4. J. B DeLong and L. H Summers, "Is Increased Price Flexibility Stabilizing?" *American Economic Review* 76 (December 1986), 1031-44.

JAMES M. BUCHANAN is General Director of the Center for Study of Public Choice. Following the early analysis of Knut Wicksell, Dr. Buchanan is the modern developer of the theory of public choice. He has made major contributions to the development of the contractual and constitutional bases for the theory of political decisionmaking and public economics.

Professor Buchanan earned a B.A. degree from Middle Tennessee State, a M.S. degree from the University of Tennessee and a Ph.D. degree from the University of Chicago. He holds honorary degrees from the Universities of Giessen, Zurich, Valencia, Lisbon, and George Mason University. For twelve years he taught at the University of Virginia where he also directed the Thomas Jefferson Center for Studies in Political Economy and Social Philosophy. After a short interlude at the University of California, Los Angeles, he spent the next fourteen years at the Virginia Polytechnic Institute where together with Gordon Tullock, he founded and directed the Center for Study of Public Choice. He has been a Fulbright Research Scholar in Italy, a Fulbright Professor at Cambridge University, and a Visiting Professor at the London School of Economics.

Dr. Buchanan is a past president of the Mt. Pelerin Society, the Southern Economic Association, and the Western Economic Association. He is also a past vice president and a Distinguished Fellow of the American Economic Association. In 1984 he received the Frank Seidman Distinguished Award in Political Economy. Dr. Buchanan currently serves on the advisory boards of the Reason Foundation, the Carl Menger Institute, the Hoover Institution at Stanford University, and the Law and Economics Center at the University of Miami.

Professor Buchanan has publishd about 350 articles in scholarly journals and books. Collections of his articles have appeared in *What Should Economists Do?* and *Liberty, Market and State.* He has also written about twenty books. His best known work is *The Calculus of Consent* co-authored with Gordon Tullock. Earlier he had written *Public Principles of Public Debt* and *Fiscal Theory and Political Economy.* Subsequent works include: *Public Finance in Democratic Process, Demand and Supply of Public Goods, Cost and Choice, The Limits of Liberty, Democracy in Deficit, Freedom in Constitutional Contract: Perspectives of a Political Economist, The Power to Tax, Toward a Theory of the Rent-Seeking Society, The Reason of Rules,* and *Deficits.*

5

Dr. James M. Buchanan
Holbert L. Harris University
Professor of Economics
George Mason University
Recipient of the 1986 Nobel Prize in Economics

I. Introduction

In this essay, I was asked to assess the state of economic science, necessarily from my own personal perspective, which is perhaps less representative of median or mainstream evaluation than those perspectives that may be offered by my peers in this series.* I shall make no attempt to be comprehensive here, although the implications of my whole argument for the economist's stance as both a positive and normative scientist involve major shifts in attitudes toward the disciplinary subject matter. I shall concentrate discussion on my understanding of what an economy is, from which inferential criticisms of research programs, didactic instruction, and policy implementation emerge, more or less as a matter of course.

I may succeed in attracting your attention by stating two of these criticisms boldly at the outset. First, there is no place for macroeconomics, either as a part of our positive science or as a realm for policy action. Second, the appropriate mathematics is game theory

---

*I am indebted to my colleague, Viktor Vanberg, for helpful comments on an earlier draft

rather than maximization of objective functions subject to constraints. These apparently unrelated criticisms emerge from understanding and interpreting the economy nonteleologically, as an *order,* rather than understanding-interpreting the economy teleologically, as an institutional arrangement that is to be evaluated in terms of relative success or failure to achieve assigned system-defined objectives. Were I to have a subtitle for this essay, it would be "The Economy as a Constitutional Order." I would append the word "constitutional" to the word "order" so as to indicate that my perspective differs both from those evolutionists who do emphasize the economy as an order but who, at the same time, deny that such an order can be "constituted," and from those who fail to make the distinction between constitutional and post-constitutional levels of choice.

Before proceeding, let me also classify myself philosophically. I am a methodological and normative individualist, a radical subjectivist, a contractarian, and a constitutionalist. These descriptive attributes are familiar to those of you who may have been exposed variously to my published works over four decades. In a very real sense, these works are little more than my continuing and considered assessment of the state of economics or political economy. I have always been, and remain, an outsider, whose efforts have been devoted to changing the direction of the disciplinary research program. There is perhaps less reason for me to take a reflective look at where we are scientifically than there is for those of my peers who have remained inside the dominant research program that describes what economists do. You would scarcely expect me to take on some new colors at this stage, and I assure you that there has been no recent conversion to a new paradigm. No one has had, or will have, occasion to label me as a holder of the conventional wisdom.

I shall proceed as follows. Section II examines the relationships between scarcity, choice, and value maximization within the domain of economics as scientific inquiry. My aim in this section is to demonstrate how these concepts, by having been placed in too central a role, have generated intellectual confusion. Section III extends the perspective to examine the appropriateness of macroeconomics in the subject matter domain of our discipline. Section IV briefly treats the grand organizational alternatives and develops the notion that the conception of what the economy is does have normative implications. Section V compares and contrasts the two approaches in terms of the shift from individual to social choice. Finally, in Section VI, the argument is summarized.

II. Scarcity, Choice, and the Maximization of Value

I do not know what the 1989 instructors in economics tell their students about the content of the discipline. Perhaps they simply ignore definitional starting points. But I do recall that, in the 1940s, economic theory (price theory) courses commenced with something like Milton Friedman's statement (1962) to the effect that economics is the study of how a particular society solves its economic problem. And, at least in the 1940s, everyone knew that "the economic problem" was defined by Lionel Robbins (1932) as the allocation of scarce resources among alternative ends. Scarcity, the inability to meet all demands, implies that choices must be made, from which it seems to follow directly that a criterion for "better" and "worse" choices is required. This criterion emerges as some common denominator that allows the differing demands to be translated into a single dimension, which we then label as "utility" or "value." The "economic principle" offers the abstractly defined normative solution to the economic problem. Scarce resources are allocated among alternative uses so as to secure maximum value when a unit of each scarce resource yields equivalent value in each use to which it is put. Satisfying this norm maximizes value subject to the resource scarcity constraints. Economics, as a realm for scientific inquiry, does indeed seem to be reducible to applied maximization; the calculus seems surely to be its basic mathematics.

I want to suggest here that this economics, which is the economics that I learned both as a student and as a young professional, generates intellectual confusion and misunderstanding because it focuses attention inappropriately on scarcity, on choice, and on value maximization, while shifting attention away from the institutional structure of an economy, with the consequent failure to make elementary distinctions among alternative structures. Given the dominance of the Robbins formulation in the economic theory of mid-century, it is not surprising that market solutions were often modeled as analogous to planning solutions to the resource allocation problem. Economists proceeded as if "the market" embodies "social choices" among alternative allocations of resources, choices that may be compared with those that might emerge from the monolithic decisions of a single planner. Given the mind-set of mid-century, it is also not surprising that Arrow (1951) extended his impossibility theorem to the market as well as to political choice.

As early as 1963, in my presidential address to the Southern Economic Association (1964), I criticized the central role assigned to the maximizing paradigm in economics, and I called for a revival of "catallactics" (or "catallaxy") as the core of our discipline. My argument was that economics, as a social science, is or should be about trade, exchange, and the many and varied institutional forms that implement and facilitate trade, including all of the complexities of modern contracts as well as the whole realm of collective agreement on the constitutional rules of political society.

In a basic conceptual sense, the exchange process remains categorically different from the choosing process. In exchange, there is a necessary interaction between (among) separate actors (participants), no one of which can choose among "solutions." In exchange, each participant does, of course, make choices among alternative bids and offers (strategies). But these choices of any single participant are, at most, only a part of the interaction process. A solution to an exchange emerges only from the choices made, separately and independently, by all participants in the process. This solution, as such, is not explicitly chosen by any one of the participants, or by the set of participants organized as a collective entity. This solution is simply not within the choice set of either individual actors or the collectivity.

This elementary sketch of exchange provides the basis for my early assertion that game theory offers the appropriate mathematical framework that facilitates an abstract understanding of economics. In exchange, as in ordinary games, players or participants may be modeled as behaving so as to maximize their separately defined utilities, subject to the constraints separately faced, as defined by the rules, the endowments, and the predicted responses of other participants. The standard maximizing behavior embodied in rational choice models may, of course, be accepted for this analytical exercise. But, in exchange, again as in ordinary games, neither any single player-participant nor the set of players-participants, as a group, treats the outcome of the process as a maximand. The solution to the exchange process, simple or complex, is not the solution of a maximization problem, and to model it as such is the continuing source of major intellectual confusion in the whole discipline.

Equilibrium in any exchange interaction signals the exhaustion of the mutual gains, and this solution, as such, has behavioral properties that also describe positions of maxima for all choices. At equilibrium, no participant has an incentive to make further bids (offers) within the rules that define the structure of the interaction. In the equilibrium of the ideally competitive economy, there is no incentive, either for any single participant, or for any group of participants, including the all-inclusive group, to modify the results within the rules.[1] But what is maximized in this solution to the competitive "game"? That which is maximized, in any sense at all meaningful for behavior, is the value for *each* participant, as determined separately and subjectively, subject to the endowments initially possessed and to the expressed preferences of others in the nexus, as reflected in the bids (offers) made in markets. There is no "social" or "collective" value maximization, as such, in the exchange process, even in some idealized sense. Aggregative value, measured in some numeraire, is, of course, at a maximum in the solution, but this is a definitional consequence of the equilibrium. The relative prices of goods and services are themselves determined in the process of attaining the equilibrium, and it is only when these emergent prices are used that any maximum value, as an aggregate, can be defined.

Since an abstractly defined maximum for aggregative value cannot exist independently of the market process through which it is achieved, it is meaningless to refer to a shortfall in aggregative value, as such, except as some indirect identification of failure to exhaust gains from trade among participants somewhere in the nexus. Since participants are presumed able to make their own within-exchange choices, the political economist's hypotheses that value is not being maximized must be derived from observations that there exist impediments to the trading process (Buchanan 1959, 1988), whether at the simple level of buyer-seller exchange or at the level of all-inclusive complex "exchanges" in public goods. The observing political economist is unable, even conceptually, to construct a "social welfare function" that will allow him to carry out a maximization exercise analogous to that which the planner for a centralized economy must undertake. For such a planner, his choices are analogous, even if at a different dimension of complexity, to those faced by any single participant in the exchange nexus.

III. Macroeconomics and Constitutional Political Economy

The basic and elementary distinction between the maximizing and the exchange paradigms supports the proposition advanced earlier concerning the suggested exclusion of macroeconomics from the domain of our disciplinary subject matter, at least macroeconomics as normally defined. That which is generated in the economic interaction process, whether or not represented as a formalized, abstractly defined equilibrium or solution, emerges from the separate and interdependent choices made by many participants, choices that are coordinated, whether efficaciously or not, through the institutional arrangements that define the economic structure. The economywide aggregated variables, such as national income or product, rates of employment, capacity utilization, or growth, are not variables subject to choice, either directly or indirectly, by individual participants in the economy or by political agents who may presume to act on behalf of all participants as a collectivity, or any subset thereof.

It is intellectually confusing even to model "the economy" as if its normative purpose is one of maximizing income and/or employment, or, indeed, as if 'the economy' has normative purpose at all. As noted earlier, any failure of the interaction process to generate maximum value must reflect failure to exploit gains from trade, whether simple or complex. This putative diagnosis calls attention to the structure itself which may contain constraints that prevent the consummation of mutually advantageous trades.

Alternative structures are, of course, to be evaluated indirectly by observations of the patterns of results generated, and these results may be represented in terms of the familiar macroaggregated variables such as the level and growth of national product or employment. An economy that persistently generates wide swings in levels of income and employment would, appropriately, be deemed to be a *structural* failure, and such a pattern of results should offer incentives to investigate, locate, and identify the structural sources of the problem, leading ultimately to structural-institutional reform.

The tragic flaw in Keynesian-inspired macroeconomics lies in its acceptance, and, hence, neglect, of structure while concentrating almost exclusive attention on the prospects and potential for "guiding" the economy toward more satisfactory target levels of the aggregative variables. It is not at all surprising, when viewed in retrospect, that this monumental misdirection of scientific effort should have occurred, given the dominance of the maximizing paradigm during the critical years of mid-century. There was a general failure to recognize that the whole intellectual construction is inconsistent with a structure that allows for the independent choice behavior of many participants in the economic nexus. As Keynes himself recognized in his preface to the German translation of his book, the whole reinterpretation of the economic process in a normatively directed teleological model was more applicable to an authoritarian regime than to a democratic one.

I do not want to suggest, however, that the classical economists, at least those who were the targets of Keynes's direct criticism, were free of their own peculiar sort of blindness that led them, also, to neglect structural elements. In their implied presumption that results embodying satisfactory levels of the aggregative variables would emerge, independently of possible structural failures, these economists were ill-prepared to defend the discipline against the emotionally driven zealots for macroeconomic management.

The intellectual, scientific and policy scenario should have been, and could have been, so different in those critical decades before mid-century.

Little was really needed beyond an elementary recognition that the economic process functions well only within a legal-constitutional structure that embodies predictability in the value of the monetary unit, accompanied by a regime reform that would have been designed to guarantee such predictability. (In this respect alone, a unique window of opportunity was missed in the 1930s.) Macroeconomic theory, in both its lower and its higher reaches, need not have been born at all, along with the whole industry that designs, constructs and operates the large macroeconomic models.[2]

IV. Socialism, Laissez Faire, Interventionism, and the Structure of an Economy

It is now widely acknowledged, both in theory and in practice, that socialism was (is) a failure. The socialist god is dead; the promise that was once associated with socialism, as an overarching principle for social organization, no longer exists. The romantic image of the state as an omniscient and benevolent entity, an image that had been around since Hegel, was shattered by the simple observation that those who act on behalf of the state are also ordinary humans, like the rest of us, who respond to standard incentives within the limited informational setting they confront. Centralized economic planning, with state ownership and control over means of production, has entered history as intellectual folly, despite the record of its having attracted the attention of so many brilliant minds in the first half of this century, and also despite the awful realization that efforts to implement this folly involved the needless sacrifice of millions of lives.

At the opposing end to socialism on the imagined ideological spectrum stands the equally romantic ideal of laissez faire, the fictional image of the anarcho-capitalists, in which there is no role for the state at all. In this model, freely choosing individuals, who have somehow costlessly escaped from the Hobbesian jungle, will create and maintain markets in all goods and services, including the market for protection of person and possessions. It is as difficult to think systematically about this society as it is to think of that society peopled by the "new men" of idealized communism. Robert Nozick's derivation (1974) of the minimally coercive state was surely convincing even to those stubborn minds who held onto the laissez-faire dream.

Any plausibly realistic analysis of social order, whether positive or normative, must be bounded by the limits set by these ideological extremes. The state is neither omniscient nor benevolent, but a political-legal framework is an essential element in any functioning order of human interaction. The analysis, discussion, and debate then centers on the degree or extent of political control over and intervention into the interaction process. The extended interventionist state remains a viable alternative in the ongoing political argument and proponents for such a state are found among scientists and citizens alike, and despite the general loss of faith in the socialist ideal. Opposed to the extended interventionist polity lies the minimal or protective state, tempered variously by acknowledgment of the appropriateness of both productive and transfer state elements.[3]

Questions may be raised at this point concerning how these issues relate to my evaluation of the state of economic science, which was, after all, my assigned task for this essay. I return to my central theme. My hypothesis is that the basic conceptualization of what "an economy" or "the economy" is, the paradigmatic vision of what it is that we are inquiring into and about, does, indeed, carry direct normative implications. In a real sense, my hypothesis suggests that divergent normative stances may reflect divergent *understandings* rather than differing ultimate values. If this hypothesis is descriptively accurate, genuine scientific progress may be made at the level of fundamental understanding (methodology) as well as at the apparent cutting edges of some presumed invariant empirical reality.[4]

Applied somewhat more narrowly, my hypothesis is that the normatively preferred scope for state or collective intervention will depend directly upon the conceptualization of what the economy is, as the subject for scientific inquiry. That is to say, the normative debate on the turf bounded between the socialist and the *laissez-faire* extremes will reflect the divergent models of the observed reality. In a certain sense, *the ought is derived from the presumed is.*

Let me try to be more specific. I suggest that an accepted understanding of the economy as an order of interaction constrained within a set of rules or constraints, leads more or less directly to a normatively preferred minimal intervention with the results of such interaction. By comparison and by contrast, an accepted understanding of the economy as an engine, mechanism, or means, organized for the achievement of specifically defined purposes, leads more or less directly to a normatively preferred stance of expediency in evaluating possible state or collective intervention with the interaction process.

Many textbooks commence with a discussion of the functions of an economy, as introduced by Frank H. Knight (1934). I have suggested (1988) that even so much as a listing of ''functions'' for an economy may generate confusion and misunderstanding. If the economy, as such, is without purpose, how can we attribute functions to its operation? The economy-as-order conceptualization forces us to restrict evaluation to the relative success of the structure in facilitating the accomplishment of whatever it is that the separately interacting participants may seek. (Again, the basic game analogy is useful. We evaluate the rules that describe a game by assessing how successful these rules are in allowing players to achieve those objectives they seek in playing.)

The point here may be made emphatically in the simple example of two-person, two-good exchange. Two traders are presumed to hold endowments in two goods, and these endowments are assumed to be mutually acknowledged to be owned by the initial holders. The traders are observed to engage in exchange, and a post-trade distribution of endowments different from the pre-trade distribution emerges. How do observing economists evaluate this simple exchange process? The two interpretations or understandings involve quite different exercises. The mechanistic, functionalist, teleological understanding introduces a presumed prior knowledge of individual utility or preference orderings, and the post-trade positions are compared with the pre-trade positions, for each trader. If the comparisons indicate that each trader has moved to a higher level of utility, the exchange is judged to have been mutually utility-enhancing.

The economy-as-order understanding proceeds quite differently. The economist does *not* call upon some presumed prior knowledge of the utility or preference functions of the two traders to be able to conclude that the exchange has been utility-enhancing for each trader. He does not evaluate the results of exchange teleologically against some previously defined and known scalar. Instead, he adjudges the exchange to have been utility-enhancing for each trader to the extent that the *process* itself has embodied attributes of fairness and propriety. If there has been neither force nor fraud, and if the exchange has been voluntary on the part of both traders, it is classified to have been mutually beneficial. When the economist analyzes the behavior of the traders in entering into and agreeing on terms of exchange, he may, if desired, use the language of utility maximization, provided that the exclusive emphasis is placed on individuals' behavior in maximizing their separately identified utilities, which are not observable independently.

Important implications for potential intervention in voluntary exchanges stem from the contrasting interpretations here. If the economist bases his evaluation on the relative success of the exchange in moving the traders higher on an independently existing utility scalar, he may be led to recommend intervention even in the absence of observation of force, fraud, or coercion in the exchange process itself. This approach provides the basis for paternalistic, merit-goods arguments for collective interferences with voluntary market exchanges. The individual may not act so as to maximize his own utility. On the other hand, if the observing economist bases his evaluation exclusively on the process of the exchange itself, recommendations for collective intervention must be limited to proposals for removing barriers to trade inclusively defined.

We can remain with the simple exchange example to discuss the role of agreement in the two interpretations-understandings of economic interaction, along with the place of the Pareto criterion in any evaluative exercise. Exchange involves agreement on the part of traders, both upon entry into trade and upon terms of trade. The emergence of a post-exchange distribution of goods signals an equilibrium of sorts. The teleological interpretation of exchange does not call upon agreement for any critical purpose. The dual criteria are the separate utility scalars of our two traders, presumed known to the assessor prior to trade. If exchange moves each trader higher on the scalar assigned to him, the change is defined to have been Pareto superior. The welfare assessment can be positive without any necessary resort to interpersonal utility comparisons.

By contrast, the economy-as-order interpretation depends critically upon agreement as the criterion for assessment. Since there are no independently existent scalars, the only indication that traders have improved their position lies in their observed agreement. A positive welfare assessment becomes possible because the agreement has signaled mutually preferred change. Agreement is the means of defining Pareto (Wicksell) superiority, and it is the only means that exists.

V. From Individual to Social Choice: Utilitarian Versus Contractarian Foundations

The economist who conceptualizes the economy as a potential welfare-generating mechanism or instrument may be unwilling to limit criteria of evaluation to separately imputed, individually identified scalars. Almost by necessity, and despite the acknowledged insupportability of a simplistic utilitarianism, some attempt will be made to derive meaningful measures for "social" or "collective" utility. This is the essential thrust behind the invention-elaboration-use of the social welfare function constructions in mid-century theoretical welfare economics, constructions that embodied both explicit introduction of ethical judgments and the relevance of the Pareto escape from direct interpersonal utility comparability. This whole exercise involved a search for a post-Robbins scalar against which the potential performance of the economy might be measured, a scalar that could be set up to exist independently of the performance itself. Success or failure of that which is evaluated, the economy or the market, is then determined from some comparison of observed results with those that might have been achieved. Modern economists who resorted to the social welfare function constructions, and despite all their methodological and philosophical sophistication, have really not succeeded in escaping from the utilitarian foundations from which the whole maximizing-allocationist paradigm emerged late in the nineteenth century.

If we shuck off the utilitarian trappings and simply abandon efforts to construct a scalar that will allow evaluation of performance for the economy or the market, as such, we are then forced into an acceptance of the alternative conceptualization advanced here, that of the economy as an order, or structure, or set of rules, the performance of which is not to be evaluated in terms of results that are conceptually divorced from the behavior of acting individuals within the order itself. Within the order or structure, individuals engage in trade. If we then generalize the trading interaction and extend its application over large numbers of actors, we may begin to explain, derive, and analyze social or political interdependence as complex exchange, as a relationship that embodies political voluntary agreement as an appropriate criterion of legitimation.

The contractarian tradition in political philosophy offers the intellectual avenue that facilitates the shift of inquiry from simple market exchange engaged in by two traders to the intricacies of politics. Many critics balk at this extension. They may accept the centrality of voluntary exchange in economic process but remain unwilling to model politics in the exchange paradigm. By simple observation, so say such critics, politics is about conflict and coercion. How can we even begin to explain political reality by an exchange model?

The contractarian response requires a recognition of the distinction between the constitutional and the in-constitutional or post-constitutional levels of political interaction, a distinction without which any normative justification for political coercion could not exist, at least for the normative individualist. Conflict, coercion, zero-sum or negative sum relationships among persons—these interactions do indeed characterize political institutions, as they may be observed to operate *within a set of constitutional rules,* that is, within a given constitutional order. The complex exchange model which embodies agreement among the many participants in the political "game" is clearly inapplicable here. But if analysis and attention are shifted to the level of rules, among which choices are possible, we can use potential and actual agreement among persons on these rules as the criterion for normative legitimacy. And such agreement way well produce rules, or sets of rules, that will operate so that, in particularized sequences of ordinary politics (single plays of the game) there may be negatively valued results for some of the participants (Buchanan and Tullock 1962).

Note that there is a more or less natural extension from the simple model of market exchange to the complex model of constitutional politics. There is no categorical distinction between the economic and the political process; inquiry in each case centers on the choice behavior of individuals who act, one with another, to choose rules that will, in turn, constrain their within-rule choices that will, in their turn, generate patterns of results. Note also, however, that this politics-as-complex-exchange derivation is not readily available to the economist who remains trapped in the maximizing straightjacket.

VI. The Political Economy as a Constitutional Order

I fudged a bit earlier in this essay when I indicated that my subtitle for it would have been "The Economy as a Constitutional Order." It should now be clear from my discussion that I define the institutions of both the economy and the polity as belonging to an inclusive constitutional order that we may designate as "the political economy." The political economy is described by the whole set of constraints, or structure, within which individuals act in furtherance of their own objectives.

Defined exclusively, these constraints include physical and technological limits, including those embodied in human capacities, that can be taken as invariant. These "absolutes" are beyond my range of interest, except to note that much of the folly of the socialist idea stemmed from a failure to recognize the relative immalleability of human beings. My concern here, however, is with the set of constraints that are subject to deliberative change, and, hence, to choice.[5] Because these constraints are general and extend over all participants in the political economy, any choice must be, by definition, public, in the classic public good sense of this term. A shift in constraints for any one actor must apply for all actors.

Let me now return to the distinction made earlier between the constitutional and the in-constitutional levels of choice. Given any set of constraints, individuals will, separately and jointly, act in pursuit of their own interests and objectives. For some purposes, it is useful to take the existing constraints as a set of relatively absolute absolutes and to direct inquiry to predictions about the emergence of patterns of results. This domain of positive economics is productive, but it should not lead to the inference that these patterns of results can be modified to meet predetermined objectives, independently of any shift in the constraints themselves. Such effort must be paralleled by analyses aimed at predicting results that will emerge under alternative constraints, other rules of the game, other constitutional structures. As I noted earlier, the tragedy of the Keynesian enterprise lay in its failed effort to modify aggregative results directly, due to its oversight of any prospects for institutional-constitutional change.

If the political economy is conceived as being described, in part, by constraints that can be subject to explicit collective choice, attention is immediately drawn to prospects for constitutional-institutional change. Once again the game analogy is helpful; we change a game by changing the rules, which will, in turn, modify the predicted pattern of outcomes. If we diagnose the patterns of results observed to be less desired than alternative patterns deemed to be possible, it is incumbent on us, as political economists, to examine predicted results under alternative constraint structures. It is not legitimate to criticize, for example, an existing distribution of income of allocation of resources as being unjust, inequitable, or inefficient, without being able, at the same time, to demonstrate some proposed alternative regime that can be expected to generate distributions or allocations that will do better by the same standards (Vining 1984; Usher 1981; Brennan and Buchanan 1985).

No one will, of course, be surprised that I have used the occasion of this essay to present a varied reiteration of the case for "constitutional political economy" as the research program that should command the current attention of economists. As such, this research program involves both positive and normative elements. Some critics have often accused me of skirting dangerously close to, if not actually committing, the naturalistic fallacy, that of deriving the "ought" from the "is." I have never been concerned with such criticisms directly because, as noted earlier, in a certain sense we do derive 'oughts' from our conceptions of what "is." The "is" that we take to be the economy does, indeed, have direct implications for how we ought to behave in our capacities as citizens who indirectly make collective choices among sets of rules. And let us be sure to understand that there is no "is" that is "out there" to the observing eye, ear, or skin. We create our understanding of the "is" by imposing an abstract structure on observed events. And it is this understanding that defines for us the effective limits of the feasible. It is dangerous nonsense to think that we do or can do otherwise.

NOTES

1. In slightly more formal terms, the competitive equilibrium is in the core of the game. This conclusion holds only if the rules of the game are strictly defined and enforced, and especially in relation to the incentives offered to potential monopolizing coalitions

2. Because of the near-universal failure of economists to look at structure, then and now, we face, in the 1990s, even more potential unpredictability in the value of the monetary units than we did in the 1920s. Given the inherent structural defect in our monetary regime, macroeconomic theorizing and the macro models may be useful, if for no other reason than that our discretionary monopolists of fiat issue may use such models for their own purposes The macro money game that we all must play is cumbersome, complex, and confusing It is sheer intellectual folly, joined with some jealousy for pseudo-scientific inquiry, to pretend that a regime shift could not produce dramatic increase in well-being for almost everyone

With predictability in the value of the monetary unit established (with any one of the several alternative regimes that might be the replacement for the discretionary authority in existence), economists could then get on with their appropriate social roles of analyzing the exchange process in detail, with identifying barriers to the implementation of value-enhancing voluntary exchanges, with advancing hypotheses concerning changes in constraints that allow individuals to exploit more fully all potential for mutual gains

3. A cynical observer might suggest that little, if any, scientific progress has been made since 1776, when Adam Smith first presented the antimercantilist argument from which modern economics emerged Mercantilism (protectionism, interventionism) seems to have reemerged in the decades of the 1970s and 1980s in partial replacement for the acknowledged demise of socialism.

4. As my great professor, Frank H. Knight, once remarked at the end of an impressively presented empirical survey, "proving that water runs downhill," which expresses my own verdict on much of what I see in the now-dominant empirical emphasis of modern economic research I doubt if many economists are convinced by empirical evidence alone, although I acknowledge that the linkage between evidence and understanding remains mysterious

5. I do not accept the implications of the analyses of some cultural evolutionists, who suggest that the basic institutions of social order evolve without conscious design and, by inference, suggest also that deliberate improvement in these institutions may be impossible, and, further, that attempts at improvement are harmful

References

Arrow, Kenneth, *Social Choice and Individual Values* (New York: Wiley, 1951).

Brennan, Geoffrey and James Buchanan, *The Reason of Rules* (Cambridge: Cambridge University Press, 1985).

Buchanan, James M., "Positive Economics, Welfare Economics, and Political Economy," *Journal of Law and Economics* II (October 1959), 124-138.

____________, "What Should Economists Do?" *Southern Economic Journal* XXX (January 1964), 213-222.

____________, *What Should Economists Do?* (Indianapolis: Liberty Press, 1979).

____________, "Economists and Gains-From-Trade," *Managerial and Decision Economics,* special issue in honor of W. H. Hutt (Winter 1988), 5-12.

____________, "On the Structure of the Economy," *Business Economics* XXIV (January 1989), 6-12.

Buchanan, James M. and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

Friedman, Milton, *Price Theory* (Chicago: Aldine, 1962).

Hayek, F. A., *Law, Legislation and Liberty,* Vol. III, *The Political Order of a Free People* (Chicago: University of Chicago Press, 1979).

Knight, Frank H., *The Economic Organization* (Mimeographed, University of Chicago, 1934).

Nozick, Robert, *Anarchy, State, and Utopia* (New York: Basic Books, 1974).

Robbins, L., *The Nature and Significance of Economic Science* (London: Macmillan, 1932).

Usher, Dan, *The Economic Prerequisites to Democracy* (New York: Columbia University Press, 1981).

Vining, Rutledge, *On Appraising the Performance of an Economic System* (Cambridge: Cambridge University Press, 1984).

During the past thirty years **HERBERT A. SIMON** has focused on decisionmaking and problemsolving processes, using computers to simulate human thinking.

Professor Simon holds A.B. and Ph.D. degrees from the University of Chicago. He has been awarded more than a dozen honorary degrees from U.S. and foreign institutions including the University of Chicago, Yale University, Columbia University, the University of Michigan, the University of Pittsburgh, McGill University in Canada, Lund University in Sweden, and Eramus University in The Netherlands. Before joining Carnegie Institute of Technology in 1949, Professor Simon served as Director of Administrative Measurement Studies at the University of California, Berkeley for three years and as a professor of political science at Illinois Institute of Technology for seven years. At Carnegie-Mellon he has served as Professor of Administration, head of the Department of Industrial Management, Professor of Administration and Psychology, and Associate Dean of the Graduate School in Industrial Administration.

Dr. Simon is a fellow of the American Academy of Arts and Sciences, the American Association for the Advancement of Science, the American Psychological Association, the American Economic Association, the American Sociological Society, the Econometric Society, and the International Academy of Management. He has received awards from several of these associations and also from the Association for Computing Machinery, the American Political Science Association, and the Institute of Electrical and Electronic Engineers. He is a member of the National Academy of Sciences. In 1986 he was the recipient of the National Medal of Science. Dr. Simon has been Chairman of the Board of Directors of the Social Science Research Council and of the Behavioral Science Division of the National Research Council, and was a member of the President's Science Advisory Committee.

Professor Simon is the author of about 25 books and monographs and over 600 articles in scholarly journals and books. His books include: *Administrative Behavior, Human Problem Solving, The New Science of Management Decision, The Sciences of the Artificial, Models of Thought, Models of Bounded Rationality, Reason in Human Affairs, Protocol Analysis*, and *Scientific Discovery: Computational Exploration of the Creative Process.*

6

Dr. Herbert A. Simon
Richard King Mellon University Professor of Computer Science and Psychology
Carnegie-Mellon University
Recipient of the 1978 Nobel Prize in Economics

The title of this volume of essays is "The State of Economic Science," but I should not like to limit myself to just describing where economics is now: I would like also to say something, however conjectural, about the future. Since, in contrast to Larry Klein, forecasting is not my professional metier, perhaps I can avoid some of the dangers of amateurism by putting more emphasis on the prescriptive than on the predictive; by taking high moral ground and telling economics what it should be doing that it is not now doing.

Consensus and Dissention Among Economists

With this agenda, my emphasis will be on methodological issues: on what kinds of research economists should be doing and what tools they will need for doing it. But my methodological prescriptions need to derive from a view of the substance of economics and its problems, and should rest on an assessment of where economic science stands today. Let me begin with that.

Public Disagreement Among Economists

I must first explain why economists are seen by the general public to be constantly disagreeing with one another. Of course that is not wholly true with respect to my distinguished colleagues in this volume. I have observed that Ken Arrow, Larry Klein, Jim Tobin, and Bob Solow often agree even on public policy issues—they may more often disagree with Jim Buchanan. But if Milton Friedman, or George Stigler, or Ted Schultz—to mention just a few equally notable luminaries—had happened to be present in this year's sample, the disagreement/agreement ratio would have increased quite sharply. And we could perhaps liven up the discussion even further by adding Bob Lucas or Sam Bowles. I have not even begun to exhaust the spectrum of policy views we can find within the economics profession.

Although you may have seen relatively little disagreement among the authors in this volume, the public is not mistaken in its perception that economists disagree frequently and vociferously. The fact of the matter is that, no matter what our views on matters of public policy, we can almost certainly find a relatively prominent economist to defend them; and this democratic indiscipline of the profession is well known to all readers of the daily press. We have Keynesian economics, monetarism, supply-side economics, rational expectations, and budget balancing, not to mention free trade, protection for infant industries, and proponents of the income tax, the single tax, the sales tax, and almost no taxes at all.

When national economic problems are under discussion, economists disagree as to whether to give priority to inflation, the public debt, employment, taxes, the trade balance, or the rate of interest. Clearly, these priorities have much to say about what policies economists will advocate. During the late seventies, there was a massive shift in primary concern from employment to inflation, but the shift was not unanimous and left the profession in as much disarray as before—or more. Likewise, it is easy to start a lively argument about whether taxes should be raised in order to stem the rise in the national debt, or whether the burden of the national debt on the federal budget should be relieved by lowering interest rates.

And finally, economists disagree about their forecasts for the economy over the coming months or years. We can almost always find optimists, pessimists, and flat-planers, although their relative numbers vary from time to time. Nor do these proportions much correlate with what the economy actually does. Economic forecasting is in low repute, often also among economists, who can even provide theoretical reasons why it should be impossible to predict the course of the business cycle or the stock market. The last recession saw a high casualty rate among economic forecasting units attached to business firms—at a time of budget crunch, they were seen as dispensable.

With this cacophony of economic voices, it is not surprising that the status of economics as a science is sometimes challenged by a bemused public. Isn't a science supposed to provide accurate predictions about its phenomena? Is economics therefore not a science? We had best reserve our judgment a bit until I get on with my story. We are aware that meteorologists also disagree widely in their forecasts, but we would not doubt that meteorology is a science. For meteorologists have built a model of the atmospheric processes that is consistent with the basic, well-supported, laws of physics. Possessing such a model does not guarantee an ability to predict the future of the weather with any great accuracy.

In recent years, there has even been a very exciting development in the field of mathematics that suggests that certain complex systems (systems of nonlinear equations) may behave in a way that makes them inherently unpredictable. Such systems are called "chaotic," and their central characteristic is that a tiny displacement of their initial conditions sometimes causes them to go off on completely divergent paths. We don't know whether the atmosphere is actually a chaotic system, but if it is, the wingbeats of a butterfly in Singapore may cause a tornado in Chicago.

We also don't know whether the economy is a chaotic system. There are some statistical tests that can be applied to time series—the money supply, or stock prices, or other economic series—to distinguish between simple randomness and the kind of chaos and unpredictability I am describing here. Some attempts have been made recently by economists to test whether various economic time series are chaotic, but the results so far are inconclusive. (The September 1987 issue of the *Journal of Economic Behavior and Organization,* Volume 8, Number

3, which is devoted to the subject of chaos in dynamic economic systems, will provide the reader with a good introduction to the topic.) I mention chaos to indicate that we should be wary of using prediction as a test of science, and especially of whether economics is a science, for an understanding of mechanisms does not guarantee predictability.

Private Agreement Among Economists

If it is well known that economists disagree, it is less well known to the general public, although perhaps not among economists, that there is a very high level of *agreement* among them. I will try to explain the apparent paradox and show why this is so. In spite of the disagreements I have enumerated, economists subscribe, nearly to a man and woman, to a common central core of theory, and above all, to a way of reasoning about economic questions. Before I state what the common core is, let me give an example of the kind of agreement that this communality produces.

> Suppose that during a period of gasoline shortage, the government issues ration tickets to the poor to guarantee them an adequate supply of gasoline at a moderate price. Should the law be written so that the poor may sell their ration tickets to others if they would prefer the money to the gasoline, or should such exchanges be prohibited by law?

When this question was presented to a national sample of lay persons, about 80 percent replied that the exchange should be prohibited—that the poor should be able to use the tickets only to purchase gasoline. When the same question was presented to a national sample of economists, virtually all responded that the exchange should be allowed.[1]

The reason economists are able to agree on this conclusion is that they apply to the problem a common set of assumptions and inference procedures. All (or nearly all) economists assume that every person is a utility maximizer: (1) possesses a consistent, comprehensive utility function that orders all the possible alternatives of choice, and (2) selects among alternatives so as to maximize subjective expected utility. "Subjective expected" means that the decisionmaker assigns (subjectively) probabilities to all possible outcomes of each available alternative, and averages the utilities of these outcomes, using the subjective probabilities as weights. Economists (nearly all economists) claim that people do behave so as to maximize utility, and that they *ought* to behave in this way, which they regard as uniquely rational.

Now in the rationing case, if the potential seller and buyer both agree on a price for the tickets, this must mean—using the standard economic reasoning—that each one thinks that his or her utility will be increased by the exchange. Hence, they will make the exchange if they are permitted to, and they should be permitted to because then everyone will be better off. When certain other conditions are also satisfied, the exchange is said to produce a *Pareto optimum,* a situation that could not be altered in any way to produce an increase in utility for *both* participants. Economists also refer to solutions of decision problems that are Pareto optimal as producing an *efficient allocation of resources.*

My account of the core of standard economic theory is, of course, something of an oversimplification. Many of the conclusions, particularly the normative ones, depend on the economic system being in a stable state of competitive equilibrium, and only under these conditions can a Pareto optimum guarantee an efficient allocation of resources. My simplified version, however, gives a clear enough picture of the nature of contemporary economic reasoning that is subscribed to by almost every school of thought.

Even Marx, in *Das Kapital* subscribes to this theory and this way of reasoning. So does Keynes; for the *General Theory* uses, on every page, standard economic reasoning. So do the rational expectationists, like Lucas and Sargent, and the monetarists, like Friedman. So do my predecessors in this volume. The allegiance of economists of all sects and descriptions to subjective expected utility maximization is hardly less unanimous than the allegiance of physicists to Newton's Three Laws of Motion in their relativised form.

Disagreement From Consensus

Now I appear to have hooked myself on a serious dilemma. If economists agree in the way I have just outlined, how can they disagree in the ways I described earlier? They can disagree because, in order to apply the standard core theory to real-world problems, that theory has to be augmented by additional assumptions, especially assumptions about what information people have, and how they deal with uncertainty and with their own limited capacities for computation. Economists disagree about these auxiliary assumptions; and as a result of the disagreement, reach very different conclusions even though applying identical modes of reasoning. That is not surprising: conclusions depend on premises. Using identical laws of logic, we will reach quite different conclusions about the visibility of swans in dim light from the premise "All swans are white" than from "All swans are black."

Let me provide an example of the sources of disagreement in economics from a central topic of macroeconomics: business cycle theory. The theory of static equilibrium under perfect competition developed by Arrow, Debreu, and others proves that, under equilibrium conditions, all resources will be fully employed: labor, capital, and so on. However, most (not all) economists concede that the real world is plagued by a fluctuating level of employment of both labor and production facilities, with periods, from time to time, of substantial unemployment. How can we, within the framework of the accepted theory and method of reasoning, reconcile general equilibrium theory with the fact of unemployment and underemployment of resources. We can do it by making one small change in the assumptions of rationality—by introducing one small grain of irrationality or nonrationality into behavior, and allowing that grain to grow into the pearl of unemployment of resources.

Keynes uses several mechanisms to produce this result; I will refer to just one of them. Starting with a standard classical model, he assumes that labor is not always quite rational, specifically, that in bargaining for wages, labor sometimes confuses the money wage, in current prices, with the real wage, adjusted for price changes. Labor is just a little stupid, and this stupidity draws the system away from full-employment equilibrium into a situation in which unemployment can appear and persist.

Lucas, a rational expectationist usually regarded as standing at the opposite end of the policy spectrum from Keynes, also admits that there is sometimes unemployment in the real world. He too starts out with a model of perfect rationality, which of course leads to full-employment equilibrium. He also introduces a grain of sand to displace that equilibrium and produce unemployment. His grain of sand consists in assuming that businessmen are not always quite rational, specifically, that they sometimes stupidly mistake a shift in the general price level for a change in relative prices in their own industry. This mistake (from lack of knowledge or ignorance in calculation) draws the system away from full-employment equilibrium into a state where there is unemployment.

So both Keynes and Lucas depart from the assumptions of perfect rationality in order to account for a fact of the real world, the fact of periodic unemployment. Both introduce a grain of irrationality in the form of a confusion between monetary and real prices—what economists usually call "money illusion." In the case of Keynes, the liberal, the money illusion is suffered by labor; in the case of Lucas, the conservative, it is suffered by businessmen. Presumably we know our friends better than we know strangers! Not only do they introduce irrationality in different ways, but they draw different conclusions, as a consequence, as to what policies should be followed to deal with the resulting unemployment.

In understanding the disagreement between Lucas and Keynes, it is important to remember where it comes from. It does not come from the central theory of rational economic behavior, on which they both agree, but from the auxiliary assumptions—the grains of sand—on which they disagree. That might lead us to ask where these auxiliary assumptions come from. They are empirical assumptions, assumptions about how particular groups of human beings behave in the real world, about what they know and don't know and how they use that knowledge or are affected by that ignorance. But you will search the pages of Keynes and Lucas vainly if you want to know the evidence from which these crucial premises are drawn. They are presented simply as "obvious" facts, as something "everyone knows." A dangerous procedure for a science that purports to give an account of the real world!

It is not only in business cycle theory that the auxiliary assumptions do all the work of deriving the conclusions. Here is an example drawn from microeconomics. In recent years, we have seen a kind of economic imperialism in which microeconomists seek to demonstrate that they can use the standard theory to explain not only strictly economic phenomena but the phenomena of politics, the family, and indeed of all of our social institutions as well. A vigorous and well-known leader of this intellectual imperialism is Gary Becker, of the University of Chicago; and James Buchanan also belongs to this school of thought, especially as it applies to political institutions.

Let us see how Becker, in his book on the family, deals with the observed fact that the employment of women has increased rapidly in recent years. Becker explains that this is due to the fact that the demand curve for women employees has shifted upward during this same time—more women will be employed at any wage, and hence wages will rise as more women are attracted from the home to the labor market. Becker cites no particular evidence for the shift in the demand curve, and indeed, it is not clear that during the period he is concerned with, the wages of women increased much more than the wages of men.

Moreover, there are alternative explanations we could consider. Perhaps the utility function of women has shifted, for example, as a consequence of consciousness raising, so that they prefer outside work to homework more than they did before. Or perhaps the productivity of homework has increased with the availability of household appliances, while the demand for it is relatively inelastic. All of these shifts would cause an increase in the number of women employed outside the home, and we are far from exhausting the list of possible explanations.

Again we see that the conclusion (which matches an observed fact) is not a consequence of the theory of rationality. The real work is done by the auxiliary assumptions, and without additional empirical evidence (which Becker does not bother to provide), we do not know which of the possible auxiliary assumptions is actually satisfied, and hence is causing the phenomenon. Without taking seriously the task of verifying the auxiliary assumptions empirically, we can find an "explanation" for any fact; our theory becomes tautological and vacuous. As Poincare once remarked about another theory: "An explanation was necessary, and was forthcoming; they always are; hypotheses are what we lack the least."

In economics, hypotheses are certainly what we lack the least. Auxiliary hypotheses, frequently manufactured in the armchair, are in good supply. What is lacking is empirical evidence to test them. Why don't economists devote themselves more vigorously to testing their auxiliary hypotheses? I can only conjecture (the question is one that should also be settled by facts, historical and sociological facts about the economics profession).

First, there is a tradition of a priorism and deductivism in economics, which goes back at least to Adam Smith (although Smith was not reluctant to take facts where he could find them). It is more fun, and easier, to play with theory, especially mathematical theory, than to grub for facts. As long as the tradition persists in the profession, editors will accept analyses based on postulated facts or facts tested by casual empiricism.

Second, a few philosophers of economics, for example Boland, think that the standard theory is true by definition, and not empirically refutable. Friedman holds a related position, arguing that economic theory can only be tested by assessing the empirical correctness of its "predictions," and not by testing the correctness of its "assumptions." I have not the time here to argue the fallacies in these positions, which have been pointed out by numerous economists on other occasions. I mention these erroneous beliefs simply to account for the relative indifference of economists to empirical evidence.

Third, economists of course do carry out a very considerable amount of empirical work to test their theories, but they employ only a narrow range of methodologies in doing so. As a result, they spend much of their time viewing the world through the wrong end of a telescope, with correspondingly foggy results. By "the wrong end of a telescope" I mean that economists usually test their theories with the help of rather aggregated data about the economy which have mainly been gathered for practical purposes unconnected with economic theory and whose concepts and units bear only a faint resemblance to the concepts to which they are matched in the theory.

Robert Eisner has described these difficulties eloquently in his recent presidential address to the American Economic Association, which appears as the lead article in the March 1989 *American Economic Review,* 79:1-14. His summary (p. 11) displays the seriousness of his concerns:

> I have . . . noted some pitfalls in using current and past variables in analyses that depend critically upon expectations of the future. I have warned of confusion in employing narrowly defined measures of income and product in evaluating flows and trends in comprehensive earnings and output. I have argued that particularly large dangers abound in basing policy on conventional measures of private and public saving, investment and capital. I have suggested that usual estimates of some of the critical behavioral relations of macroeconomics may be suspect because of a failure to match theoretical constructs with appropriate empirical counterparts.
>
> Very generally, I conclude, it is important in economics—as elsewhere—to know what we are talking about.

A very sophisticated body of econometric theory and methodology has been created since World War II to deal with the problem of carrying out economic analyses with data that are too aggregated, measure the wrong concepts, are too infrequently collected, and are exceedingly noisy. The experience of the past 50 years has shown that these sophisticated econometric tools do not compensate for the poor quality of the data to which they are applied.

Other kinds of data can of course be obtained; data that tell us something about the expectations, preferences, and choices of economic actors at micro levels. Opinion and attribute studies can be conducted (and of course have been). Actual decisionmaking processes can be observed in business organizations (and of course have been). But polling data and "case studies" of individual behavior are mistrusted in orthodox economic methodology. The use of these kinds of data to discover the realities of economic life and to provide an empirical basis for the auxiliary assumptions we need will require a revolution in methodological beliefs in economics, and a revolution in the training of economists, who are not now trained to use these techniques.

Validity of the Standard Theory

If casual empiricism and the neglect of available sources of empirical evidence are a major scandal in contemporary economics, equally scandalous is the retention of beliefs, central to the standard theory, that are empirically false. For when investigators have begun to examine, in field and laboratory, the central assumptions of the standard theory—not just the auxiliary assumptions this time, but the very core of the theory—it has turned out that these assumptions do not fit the facts.

For example, research in the 1950s, reported in *The Behavioral Theory of the Firm* and elsewhere, shows that business firms are not the simple profit-maximizing entities that the standard theory assumes them to be. They sometimes settle for "satisfactory" profits. They operate with slack that is only reduced under the pressure of hard times. They suffer from internal power struggles. They simply do not behave as required to meet the conditions of competitive equilibrium with efficient use of resources.

Second, an increasing number of studies of experimental markets, operated under the controlled conditions of the laboratory, have shown that it is easy to construct market situations where behavior will depart widely from the predictions of the standard theory. Vernon Smith, Charles Plott, and others have shown that behavior tends to settle toward the predicted market equilibrium in simple markets, but that it is not at all difficult to produce booms and busts even under conditions where participants could easily compute the correct, rational behavior that would produce stability and equilibrium.

Third, a long series of studies by Allais, Kahneman, Tversky, and others have shown serious departures from utility-maximizing rationality when choices are made under conditions of uncertainty. Respondents, for example, opt much less frequently for a surgical operation where there is a 20 percent chance of dying than for an otherwise identically described operation where there is an 80 percent chance of living.

The sum and substance of these findings is that people just do not maximize utility. They do not have consistent utility functions (Allais). They do not reason correctly and consistently about probabilities and risks (Kahneman and Tversky). They use rules of thumb (heuristics) to simplify choice (Cyert and March). They look for satisfactory courses of action, they satisfice, instead of optimizing (Simon).

Nature is supposed to abhor vacuums, and the demonstration that a theory is empirically false, as the standard theory of economic choice most certainly is, appears to leave a large vacuum. Is there anything to fill it? Indeed there is. Over the past 30 years, cognitive psychology has developed a large body of empirically tested theory of decisionmaking and problemsolving. So far, economists have mostly ignored it.

The theory of decisionmaking to be found in psychology today substitutes satisficing for optimizing. It gives central position to the limits of human ability to deal with real-world complexity, and to the methods of radical approximation that are used to remove this mismatch of adaptive power to problem difficulty. Enlarging the scope of economic decision theory, it does not assume that the alternatives of choice are given *a priori,* but shows how they can be generated by selective (heuristic) search in problem spaces. It is beginning to investigate the processes that focus human attention on certain problems so that others are ignored, and the processes of representing or framing problems in terms of their most important dimensions, omitting those that are less important.

The research that has produced the new theory has also produced new methodologies that are relevant for economic research. Methods have been devised to use computers to simulate complex human thought processes and thereby to model problemsolving and decisionmaking situations of real-world complexity. The theory and practice of taking thinking-aloud protocols from human subjects while they are thinking and solving problems has been developed and explored.

Of course these developments do not mean that the problems of economics have been solved and that economists need merely to replace their standard theory with the new theory and apparatus. Certainly few of the economic implications of the new theory have been worked out. But a vacuum does not exist, and would not exist if the standard theory, or its invalid components, were simply abandoned. There is a firm foundation on which economic research can proceed.

What is to be Done?

I have described what I believe to be the serious problems facing economics and a path toward their solution. Both the auxiliary assumptions and the central postulates of classical economics must be revised on the basis of sound empirical evidence. To get the proper evidence, economists must receive new kinds of research training, much of it borrowed from cognitive psychology and organization theory. Doctoral candidates must learn how to obtain data about beliefs, attitudes, and expectations. They must learn how to study decisionmaking processes inside and outside business organizations. They must learn how to analyze and interpret the verbal reports of human subjects in both laboratory and field situations. They must learn how to conduct laboratory experiments.

Increasing numbers of mainline economists are beginning to perceive and accept the need for these reforms. I have already mentioned some of them, and I could add many names to the list. But there is a long way to go.

The inability of economics today to play the policy role to which it aspires is a major source of pressure toward reform. The hubris of the 1950s, when many economists thought we knew how to fine tune the economy, has disappeared in the face of stagflation and the ideological struggles between Keynesians and rational expectationists. Nevertheless, the reform will not be rapid.

As it progresses, however, the alteration of economics toward a responsible empiricism will gradually remove the present scandals in economic science: not just the public scandal of disarray in the policy arena, but the private scandal, known best to economists themselves, of a science that nearly renounced the practice of looking hard at the empirical evidence that was supposed to provide its foundations.

The late Tjalling Koopmans, a distinguished econometrician and a Nobel Laureate in economics, warned, in a celebrated book review against "measurement without theory." The remarks of Robert Eisner, quoted earlier, echo the same concern. The concern for economics that I have emphasized in my remarks is the complement of that one—theory without measurement. What distinguishes science from every other form of human intellectual activity is that it disciplines speculation with facts. Theory and data are the two blades of the scissors. But the metaphor is not quite right, for the blades are not symmetric. When theories and facts are in conflict, the theories must yield. Economics has strayed from that simple principle, and it must return to it.

NOTE

1. I have only a newspaper reference to the original survey, which was conducted in Canada. I have repeated the finding by polling several audiences at my talks, but I must confess that when I tried to replicate the result during my talk in Kalamazoo, a majority of the noneconomists present voted like economists Perhaps most of my audience had been taking economics courses from a very persuasive faculty

INDEX

0–88099–083–X