Book Reviews

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010 GENERAL ECONOMICS


This study is Volume II of the author’s planned Knowledge: Its Creation, Distribution, and Economic Significance, destined “to become a series of ten volumes.” In his Preface the author credits Jessica Kennedy, Mary Tyler Huber, and Peggy Ricardi with significant assistance.

Completion of the entire series is facilitated by the assistance of “more than sixty research assistants” and the support of nine agencies and foundations.

The introductory chapter of Volume II is devoted to the identification of the established disciplines which, according to the author’s present plans, will be involved in the ten-volume work. Six of the remaining nine chapters are devoted to “the branches of learning,” and the remaining three chapters to “the departments of erudition,” that is the “academies of science,” libraries, and universities.

Having discussed the “taxonomy of the branches of learning” and “classical and medieval synopsis of doctrines” in the initial two chapters of Part One, the author devotes one chapter to “the tree of knowledge,” touching mainly upon the work of Bacon, Hobbes, and Descartes, and three chapters to organized approaches to science and learning (e.g., encyclopedias, mapping of sciences).

The three chapters of Part Two are devoted to “departments of erudition,” that is, to academies in Europe and the United States, to libraries and their classification systems, to European and American Universities, Faculties, and Departments, and to their development of these institutions.

The present study is of great interest to students of the history of ideas, among them historians of economic thought. Chapter 3 is devoted to classical and medieval synopses of doctrines as represented mainly by Aristotle, neo-platonist Porphry, Saint Augustine, Robert Grossteete, Albertus Magnus and his disciple Thomas Aquinas, Roger Bacon, Ramon Lull, and al-Farabi and Ibn Khaldun, prominent representatives of medieval Moslem scholarship. Chapter 4 treats mainly of Francis Bacon’s “tree of knowledge” or classification of science and philosophy, based upon the intellectual faculty employed, and touches upon the contributions of Thomas Hobbes and Descartes.

Chapter 5, on “the circle of learning,” deals with the development of “encyclopedias in the sense of reference works—dictionaries, lexica,” beginning with Pliny’s 2,493 chapter, “Natural History,” a work that appeared in 43 editions by the sixteenth century. Many reference works including the first volume of Diderot’s Encyclopedia appeared before 1771 when Encyclopedia Britannica was completed, then in three volumes instead of eighteen volumes as in 1774, and much smaller than a 700 volume seventeenth-century Chinese reference work.

Chapter 6 is devoted to the works of eight philosophers of science who “engaged in mapping the sciences in what they thought to be the best of all orders,” among them: Kant, Hegel, Bentham, Andre M. Ampere, August Comte, Antoine Augustin Cournot (a critic of the synopsis of the encyclopedists), Herbert Spencer, Karl Pearson, and Charles Sanders Peirce (1839–1914), an American who viewed some humanistic disciplines as sciences.

Chapter 7 deals with two twentieth-century efforts “to renovate the classification of learning,” those of Rudolph Carnap and Otto Neurath, and with the fifteenth edition of the Ency-
The Encyclopaedia Britannica (1974), product of a board of editors chaired by Mortimer J. Adler. This edition includes a one-volume Propædia presenting a five-division classification of learning consisting of "logic," "mathematics," "science," "history and humanities," and "philosophy," a classification dismissing both the narrow concept of science adopted in British and American schools and universities and the wide concept other than in English-speaking countries.

Part Two of the book deals with "the departments of erudition," that is, with "academies" in Europe and the United States, with libraries and classification systems, and with "universities: faculties and departments." About one third of the chapter devoted to "universities," etc. deals with "early universities" in Europe and America and with developments in Europe. The remaining two-thirds deal with developments in American universities since 1876 and the impact of their changes upon the content and progress of learning.

The author closes Part II with inquiry into the possibility of conflict between "pragmatic usefulness and philosophic soundness" of changes under considerations and how to avoid such conflict. While he pointed out that "no grouping of learned disciplines can long remain valid" and that "to be conservative in classifying the expanding universe of learning would be quite unreasonable," he recently modified this view somewhat, given recent improvements in the classification of educational subject matter (pp. 167-71).

As suggested earlier, students of the history of social, educational, and economic thought will find the present well-indexed and well-annotated volume suggestive, stimulating, and quite useful.

Since the above was written, word has been received of Professor Machlup's death. His passing is not only a personal loss but a great loss to economics.

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020 GENERAL ECONOMIC THEORY


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There exists a growing consensus that the study of human behavior—whether consumption or any other aspect such as work or marriage—can benefit from an integration of the various relevant disciplines. Economists generally recognize that utility functions are based on biological and cultural factors, but they tend to leave the study of these factors to scholars from other disciplines such as psychology, biology or anthropology. Although such division of labor could in principle be efficient, in practice the study of any particular subject rarely combines insights from the various perspectives. In the case of buying behavior, Alhadeff is right in deploiring economists' abstract studies of consumption and their lack of controlled experimentation to probe the nature of tastes. Such experiments are the special province of behavioral psychology, so it should not come as a surprise that Alhadeff primarily draws on the work of this particular kind of psychologist. Although behavioral psychology and economics are in a period of rapprochement, as witnessed for instance by the collective efforts edited by Staddon (1980), Alhadeff deserves praise for contributing to interdisciplinary communication.

The book starts with a brief critique of neoclassical economics and an introduction to the operant approach central to the rest of the volume. Chapter 2 presents basic concepts of behaviorist psychology and applies them to buying behavior by classifying consumption goods according to criteria used in behavioral psychology. Terms such as positive primary reinforcer" and "nonfunctional pairing of stimuli" are introduced. In Chapter 3 the author outlines his conflict model of buying behavior, the basic idea being that buyers are simultaneously attracted to commodities and wanting to escape unpleasant payments. Chapters 4 to 6 develop that model in the context of evidence from animal experiments using different assumptions regarding the number of commodities and the equivalent of the degree of satiation (the reinforcer effectiveness limit). Chapter 7 shows how the equilibrium out-
comes are affected by changes in prices or incomes. Chapter 8 applies the model to the allocation of financial portfolios. The book concludes with a useful overview of the model.

Of most interest to this reviewer were the distinction between goods according to the biological or cultural basis of the needs they satisfy, the relative nature of needs with respect to previous history and timing (daily and over the life cycle), and the serious doubts Alhadeff raises regarding the assumption of non-satable needs often used in economic theory. There are also thought-provoking ideas regarding the "marginal utility of money" (my translation), and the elation or depression effects of price changes.

There is no doubt that controlled experiments—even if they are made with animals—can provide useful insights into buying behavior by overcoming some of the limitations of the uncontrolled experimentation economists use in most of their empirical studies. But this does not justify replacement of all economic theory, which seems to be one of Alhadeff's ambitions. He dispenses with the maximization paradigm in "an attempt to be economical about (especially implicit) assumptions" (p. 6) and, at a heavy cost to readers not fluent in behavioral psychology, introduces a long list of descriptive and analytical terms.

In comparison to the high time-cost of reading a new jargon, the pay-offs seem limited. Alhadeff's goal of replacing microeconomic theory with "an operational analysis entirely based on the well established findings of experimental psychology" (p. xi, my italics) is hard to support. As has been mentioned by Rachlin (1980), the maximization mechanism used in microeconomic theory and the reinforcement mechanism characteristic of behavioral psychology are indistinguishable. Many behavioral psychologists have models that are very similar to those used by economists, and they do not seem to view their experimental findings as a "foundation of lawful relations" (dust jacket). Even Alhadeff seems to move away from his anti-theoretical approach as he gives us an overview of his "conflict model" in the concluding chapter.

Given the scarcity of studies probing the nature of "tastes" as they explain buying behavior, this book is worth reading (especially by people familiar or desiring to familiarize themselves with the language of behavioral psychology). It is only through the proliferation of books such as this that a cross-fertilization of disciplines can occur. It will then be much easier for students of buying behavior (or other topics such as production or reproduction) to combine efforts, without paying much attention to categories such as "economics" or "psychology."

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REFERENCES


Baron Balogh has become almost a legend in his own time: bright, bad-tempered, Balliol, British Labour Party, and now Baron. Any label put on him would be as misleading as the GNP or a price index, but Belligerent Keynesian Left, or BKL, might be an approximation. This book is a brief summa of his life and work; one almost wishes he had written it in the form of an autobiography as an expansion of the fascinating and voluminous footnotes.

The major thesis has a certain majestic simplicity. Conventional Economics is neoclassical adulation of a mythical competitive market mechanism, with Marshallian-Walrasian equilibrium price theory, and Irving Fisher's quantity theory of money, pushed to laissez-faire political conclusions, crucified by Keynes, and then suffering a most deplorable resurrection in Milton Friedman monetarism, cum Reagan cum Thatcher. This resurrection suggests that the initial job was not done properly, due perhaps to some underlying streaks of neoclassical liberalism in Keynes himself, but also because of the neglect by his followers of the social consequences of full employment, and its inevitable slide into accelerating inflation, which produces the deplorable neoclassical reaction.
This reviewer, having lived through the 1930s and feeling a most uncomfortable sense of déjà vu, cannot help having a general sympathy with the Balogh thesis. The first two chapters develop an attack on the economists' assumptions of stable parameters and functions, like demand and supply curves, and denounce the quantification of the unstable and the unquantifiable. Chapter 3 takes us through the history of the past fifty years or so, with its disappointments as well as its very real successes. Chapter 4 is a sharp attack on welfare economics and on consumer sovereignty, which he dismisses as "not meaningful in any realistic sense" (p. 66), mainly because of the perversion of the consumer by advertising and corporate power; the theme is continued in the next chapter along rather Galbraithian lines of the underevaluation of collective needs. Chapter 6 attacks the conventional view that free markets will lead to efficiency in production, not only on the more familiar accusation of the inevitability of oligopoly and imperfect competition, but also on the more important thesis that the uncertainty involved in meaningless market fluctuations discourages innovation and investment and limits our getting richer. This is a point too much neglected by economists, as the remarkable history of American agriculture since price supports should have shown. Chapter 8 is a slashing attack on monetarism, with a knock-out punch Appendix on Professor Friedman and statistics. The last chapter, on the international aspect, is where Lord Balogh is most at home, and he denounces free trade as a producer and perpetuator of world inequality, and the inadequacy of simple models of comparative advantage, with a suitably righteous anger.

It is always fairly easy to show that something is bad; this, however, is an inadequate guide to action unless one can show that some alternative is better. Balogh has frequent pious references to economic planning, never very specific except in the case of his advocacy of incomes policy, which again is never spelled out much in terms of price and wage control. With his attack on conventional economics for brushing the problem of inequality under the rug it is hard not to have sympathy, but he never really discusses how to deal with the problem; there is no recognition of the problems of grants economics, for instance, and no recognition of the possible pathologies of grants. There is no recognition that planning implies somebody else being planned, and implies the substitution of threat for exchange. Consumer sovereignty which he dislikes so much may be indeed a very restricted and limited sovereignty, but if the alternative is the planned consumer, this may not be very agreeable, and indeed is best realized in jail.

The great problem with radical thought is that it tends to be based on what sociologists might call negative cathexis; that is, radicals know what they do not like and want to move away from it, but do not really know what they do like. This leads into all sorts of personal and political pathologies. With much of Balogh's criticism of conventional economics and the free market one is tempted to agree. But both of these could be very bad and still be the best of all possible worlds, if all the other possible worlds are worse. But if we are to know this we must explore possible worlds with more sophistication and enthusiasm than does this volume.

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Howard Margolis has written an interesting and original book on an important subject. The question he asks is: how can one explain group or socially motivated behavior (altruistic behavior) like voting, giving to voluntary organizations, or not availing oneself of a "free ride," in a rationality based model of human action? For instance, a central question that reappears throughout the analysis is: why do people take the time and effort to vote when the expected cost of voting is certainly positive while the expected benefits (in terms of actually changing the social outcome) are approximately zero? Selfish maximizing models cannot explain such behavior, yet empirically we see it all around us.

Margolis' answer is to posit the existence of two sub-beings within each of us which he calls the selfish being, S-self, and the group being,
G-self. The selfish being derives utility in much the way the conventional economic agent does by evaluating all actions in terms of its impact on his self-interested utility function, while the G-self cares only about the group’s well being. Hence, within each person lies a community of two sub-beings whose conflicting interests and preferences must be balanced and aggregated. This is done by positing the existing of a weight, \( W \), indicating the importance the individual gives to the S-self when it decides to allocate its resources between selfish activities and group activities. The central analytical construct in this allocation problem is the equilibrium condition \( W = \frac{G'}{S'} \) where \( W \) is as described above and \( G' \) is the marginal utility of spending another dollar on group oriented activities or goods (i.e., giving bread to the poor) and \( S' \) is the marginal utility of spending one more dollar on self interested activities or goods. Finally, \( W \) is assumed to be a function of \( (g/s) \) (the ratio of resources the person spends on group as opposed to himself) with \( W'(g/s) > 0 \) so that as one spends more on group oriented goods, the weight one gives to the S-self increases.

After giving us an overview of the problem in Chapter 1, Margolis presents some paradoxes in Chapter 2 (such as the paradox of rational voting stated above, what he calls the “public goods paradox,” etc.) which serve to motivate his analysis. Chapter 3 presents a Darwinian explanation of the evolutionary stability of his two-self model, while Chapter 4 discusses the equilibrium condition described above and calls it the “fair-share” allocation rule (Margolis from then on calls his model the f-s model). Chapter 5 deals with various details of the f-s model including the role it implies for satisficing and describes the internal allocation rule used to allocate goods within a person composed of two sub-beings (a Lindahl condition is used). In Chapter 6, Margolis attempts to translate across “paradigms” (Kuhn seems to be the author’s favorite) by comparing the f-s model with the demand-revealing analysis of the free rider problem (as portrayed by the work of Groves), the conventional model of narrow self-interested beings and the Lindahl-Samuelson model for public goods. Finally, Chapters 7, 8 and 9 present applications of the f-s model to voting theory (Chapter 7), the works of Downs and Olson (Chapter 8) and a variety of different possible and interesting applications ranging from the mathematics of social crises, to the evolution of social spending, ruling elites, anarchism, the underworld and more. A set of appendices offer some more formal details about the construction of utility functions which aggregate within-self community preferences.

In evaluating the book, let me first say that I liked it, found it well written and on a level of difficulty that facilitated communication while minimizing both useless verbiage and unnecessary formalities. Still, some critical comments may be in order. First, any book on altruism is going to have to face up to exactly what altruism is. There seems to be two approaches to this question. The first is that altruism is a utility based phenomenon which in some way alters people’s utility functions or preferences so that they act in what appears to be an altruistic manner. The second approach, seen in the work on reciprocal altruism, sees altruism as the equilibrium outcome of repeated games in which selfish maximizing agents act with restraint because more asocial behavior, if practiced, will be punished in the future (Morddecai Kurz, 1977). Margolis’ analysis is more of the first type since he creates altruistic behavior (i.e., behavior in which agents stop short of getting all they can for themselves in a given situation) by positing the existence of a G-self within all of us. Putting this in, not surprisingly, allows his agents to act in the group interest, but the real question is: why do people care about the group if they can get a free ride? Here is where Margolis’ analysis is most shaky. He claims that this dual-self attribute is an evolutionarily stable trait (à la John Maynard-Smith) in the Darwinian process of human evolution since groups of individuals exhibiting this behavior will have greater survivability than groups of purely selfish individuals. This assertion is never really proven or even argued for convincingly and in fact, at least from my understanding of the group selection debate, the evolution of altruism by group selection is very unlikely (David Barash, 1977, p. 75). Hence, the Darwinian argument of Chapter 3 is unconvincing and we must fall back on the fair-
share analysis of Chapter 4 where it is simply posited that people have within them a being who cares about the group. Having asserted this, it is not surprising that Margolis' individuals act altruistically, but I find this type of altruism of less behavioral interest than the equilibrium based reciprocal altruism.

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REFERENCES


This is an odd book; I am not sure what its audience will be. It presents the proceedings of a conference held in October 1980 by Washington University in St. Louis and the St. Louis Federal Reserve bank to consider supply-side economic policies. There are six papers, some extended comments by discussants and two speeches.

Arthur Laffer and two colleagues, Victor Canton and Douglas Jones, try to get some intellectual respectability for the Laffer curve. They use a Cobb-Douglas production function with constant elasticity of supply functions for capital and labor. They show that a Laffer curve exists, and that revenue does turn down for very high tax rates. They examine the Kennedy tax cuts, which they say offer a "natural experiment" for supply-side economics. Some rather feeble time-series evidence is used to show that "a significant expansion of economic activity and no significant loss of revenue occurred as a result of the Kennedy tax cuts."

I would not quarrel with either the existence of the Laffer curve in principle or with the findings concerning the Kennedy tax cuts; the question of course is the implications for policy. Alan Blinder takes a close look at the theory behind the Laffer curve and concludes that with plausible elasticities, the perverse position of the Laffer curve begins only at very high tax rates indeed, much higher than those currently in use. Blinder does practice critical overkill in saying Laffer should not get credit for his curve, because it follows from Rolle's theorem. I think Laffer should get the credit or blame for his curve. Do we credit the Slutsky equation to Leibnitz and Newton because they developed differential calculus?

Michael Evans presents some ideas about the productivity slowdown and the effects of taxation using his supply-side econometric model. This model is very badly flawed, a fact that is ably pointed out by Evans' discussants Stephen Braun and Albert Ando and also by Patric Hendershott's comparison of Evans' results with some of his own. A couple of examples will suffice to make the point. Taxes supposedly affect wage inflation. But Evans makes the rate of wage change depend on the level of taxes. His equation implies that a three percent cut in taxes reduces wages by six percent after 12 years. And the effect keeps getting bigger. In looking at productivity, Evans specifies that the level of secondary workers and the level of government expenditure affect the growth rate of productivity. Need I say more?

Lawrence Summers argues that conventional investment models do not adequately capture the effects of tax policy. The basic argument is a powerful one. Conventional equations reflect the fact that short-run variations in investment depend upon the short-run business cycle. But the efficacy of tax policy depends upon its long run impact on the capital stock. Summers then suggests that Tobin's q-theory of investment provides an appropriate basis for estimating the effect of tax changes on the capital stock. From theory, he derives the impact of taxation on the market's value of corporate capital. A tax-adjusted q is then constructed and used in an investment equation. The results are not shown in his conference paper, but in 1981 Summers presented a more extended version of this paper at the Brookings Panel meeting, so we know what the equations look like.

Summers simulates various tax policy changes based upon his equations. It is pretty strong stuff. A reduction in the corporate tax
rate from 0.48 to 0.40 raises the capital stock in the long run by 15 percent and the market value of capital is increased by 27 percent. Eliminating the capital gains tax adds 28 percent to the capital stock and 17 percent to market value.

There are some problems with Summers' approach. His model assumes that small tax policy changes have very large effects on the market value of capital. But nowhere is this idea really tested. He compares the performance of tax-adjusted $q$ against unadjusted $q$ in an investment equation and the tax-adjusted version does marginally better. But neither version explains much of the variance in investment. Summers has not explained why an investment model based on $q$ should overcome his original difficulty with tax analysis—namely that most of the variance in investment is cyclical.

The other problem with Summers' findings is that they imply very long lags indeed in the adjustment of the capital stock. For example, his simulations say that 15 years after a reduction in the corporate tax rate, less than half of the long-run adjustment of the capital stock is complete. After 50 years, 10 percent of the adjustment is still ahead. These lags are really wild!

In a paper written for a Brookings conference on taxation Jerry Hausman found some very high compensated wage elasticities of labor supply relative to the previous literature. These estimates imply that very large deadweight losses result from taxing labor income. In particular, the progressivity of the income tax makes it inefficient as a way of collecting revenue. The paper in this volume generalizes Hausman's original findings by simulating the national impact of a tax reduction modeled after the original Kemp-Roth. His findings emphatically refute the notion that existing tax rates put us on the diminishing revenue side of the Laffer curve. Nonetheless, taken at face value, Hausman's results imply that there is an agonizing tradeoff between equity and efficiency. In fact in his conclusion Hausman favors the flat tax over the current tax system, since the former yields a much lower deadweight loss.

One reason Hausman gets results that are different from those of previous authors is that he does the econometrics better. He and Gary Burtless (a former coauthor), have recognized that the marginal tax rate faced by an individual, given his gross wage and non-labor income, is determined by the choice of hours of work—the tax rate is endogenous, in other words. Previous estimates of parameters of the labor supply function may have been contaminated by the parameters of the tax function. The second reason for the difference in estimates is that Hausman constrains his results by specifying that no individual in the sample can have a positive income effect.

I do not know how much of his findings are due to the first of these two and how much to the second. If only the first is important, his results are of great significance.

In the final paper Daniel Hamermesh reviews the effect of unemployment insurance and other income maintenance programs on the NAIRU—the natural rate of unemployment. Hamermesh has done valuable work in the past in reviewing the impact of various social programs on labor supply and unemployment. But I sensed from this paper that a certain boredom had set in. Too many of his judgments are personal, evidence is presented and then discarded. The serious reader would do better to read the Hamermesh articles cited in the bibliography.

The volume concludes with two speeches. In the first Murray Wiedenbaum delivers a homily on excessive regulation. Senator Orrin Hatch then complains bitterly that the forecasts issued by the Congressional Budget Office are biased and inaccurate. He then goes on to give his own forecast. Speaking in October 1980, he predicts that the economic program proposed by (soon-to-be-President) Reagan would result in a $75 billion budget surplus, if enacted. It is good to know somebody can be unbiased and accurate.

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In this ambitious interdisciplinary work, Mancur Olson seeks, by adding an historical and comparative dimension to his earlier analy-
sis of the logic of collective action, to contribute to our understanding of phenomena as diverse as the differential growth rates of contemporary economies, the Indian caste system, and the macroeconomics of stagnation.

Three propositions are crucial to Olson’s argument: 1) Stable societies, undisturbed by revolution, invasion, or substantial boundary changes, tend gradually to accumulate rent-seeking collusive organizations; 2) these “distributional coalitions” on balance reduce efficiency and aggregate income and make politics more divisive; and 3) they make decisions more slowly than individuals or firms, thus retarding adjustment to change and reducing the society’s rate of economic growth.

Olson claims that his theory helps to account for the more rapid growth of France, Germany, and Japan, than of Britain and the United States, since World War II; that it does so parsimoniously, without resort to ad hoc assumptions; and that it is also consistent with the experience of smaller European countries. In addition, he presents statistical evidence supporting the theory’s prediction for the United States—that older areas of the Northeast and Midwest should grow more slowly than newer areas in the South and West.

Having tested his theory in the domain for which it was designed, Olson moves to other issues. Free trade, he argues, is valuable, less because of gains from specialization than from the fact that “free trade and factor movement evade and undercut distributional coalitions” (p. 142). Castes and racial discrimination reflect the operation of distributional coalitions, seeking rents that can be retained by well-defined groups over generations. Involuntary unemployment and stagnation in contemporary economies are explained, consistently with the rational behavior assumptions of microeconomics, as the result of cartelsitic actions of distributional coalitions, whether composed of workers or of firms.

Like Douglass C. North and other “new economic historians” Olson uses the powerful assumption of rationality to build a functionalist interpretation of history. Institutions develop because they respond to demands; an explanation of institutional change requires that one show why rational individuals would have altered their institutions in the observed direc-

tion. The assumption that what is rational will become real entails in Olson’s work an evolutionary approach that verges on Social Darwinism: “Every society, whatever its institutions and governing ideology, gives greater rewards to the fittest—the fittest for that society” (p. 72).

The assumption of rationality helps Olson go beyond ad hoc “explanations” of historical change. Yet the assumption that individuals are self-interested and rational itself is ambiguous, since “self-interest” is defined culturally rather than as an objective given. When Olson discusses particular societies—India, Britain, the United States—he implicitly has to take as given whole matrices of cultural beliefs and meanings. Why British distributional coalitions did not become as rigid as Indian castes, for example, is not explained by Olson’s theory, but can only be understood within a much richer cultural context. No economic theory of history can be fully endogenous: the evolutionary “survival of the fittest” can only be defined within a given cultural and ideological situation.

This is said less to criticize Olson than to indicate the limitations of rational-choice analysis as such. For Olson is no simple-minded economic imperialist. He explicitly disavows the intention of constructing a monocausal theory of history, he shows respect for the empirical works of historians and for the limitations of theory in understanding human history, and he criticizes economics for being “depressingly unhistorical, unevolutionary, comparative-static, and institution-free” (p. 215). Along with other recent works by economists, historians, and political scientists, this book shows that the explanatory framework of microeconomics can be enormously useful in interpreting political and social developments as the results of changes in incentives facing human beings, even though it is hardly a magic key unlocking all secrets of the past.

Specialists on particular countries are bound to take exception to Olson’s hurried discussions of how his theory helps account for rapid French economic development, the Swiss combination of prosperity with relatively slow recent growth, or the successful performance, until very recently, of such a highly organized society as Sweden. Even more hackles may be
raised by his brief discussions of such societies as India, Japan, South Africa, and South Korea. These empirical objections, however, may be less damaging to the theory than are certain of its own implications.

Olson’s rational individuals only gradually, and with difficulty, form distributional coalitions. Thus, as Albert Hirschman has pointed out, his theory should predict that sudden revolutions, or outbursts of mass participation such as those of the 1960s, will not happen. But they periodically do. Olson criticizes monetarist-equilibrium theories for denying the reality of involuntary unemployment, but his own theory denies the reality of ideologically-rooted mass politics.

Secondly, Olson’s claim that the organization of interests is inimical to economic growth depends on the assumption that interest groups are narrow, so that groups need not take into account the effect of their rent-seeking activities on the economy as a whole. Inclusive or “encompassing” organizations may have an incentive to promote growth; in several countries characterized by such organizations, such as Austria, Norway and Sweden, growth has been quite rapid. It might be expected on the basis of Olson’s theory, therefore, that older, more stable countries would have more encompassing organizations, since these large groups would have had more time to emerge; and that their economies should therefore grow more rapidly than those of countries with shorter histories and narrower groups. Olson does not draw this implication—if he did, his theory would predict that Britain would have encompassing groups and would be growing rapidly—yet he provides no convincing argument to explain under what conditions such groups will develop, and under what conditions narrowly based distributional coalitions will continue to predominate. Although he promises to deal with this question in a subsequent publication, the absence of a satisfactory answer to it in Rise and Decline obscures both the theoretical and the policy implications of Olson’s theory. If stability alone does not retard economic growth, what other political factors are involved? Is the cure for stagnation perhaps more thorough organization of interests, rather than (as Olson implies) their greater fragmentation?

A third limitation of Olson’s theory is that it ignores international effects on the rise and decline of nations. Yet much recent work in international political economy, and the development of the state system, has stressed both the differential impact of technological change on differently situated economies, and the impact of strategic interactions and war among states. Economic factors must be seen in an international context: as Robert Gilpin has recently argued, hegemonic states in the world political economy may decline as a result of diffusion of technological advantages, military overextension, or overconsumption and underinvestment at home.

The incompleteness of Olson’s theory can hardly be surprising: no comprehensive theory of “the rise and decline of nations” seems possible. It is the power of Olson’s work, rather than its limitations, that is most remarkable. The fundamental ideas are simple, yet they provide insight into a wide array of social and historical issues. He has read widely in history, political science, and sociology, as well as in economics. He has also benefited greatly from being willing to circulate drafts of his work and from listening to the criticisms of colleagues in the other social sciences. His evidence is not rigorous by the standards of contemporary economists, but this is less a fault of his work than a function of his comparative, macro-historical task. We may not be willing to take Olson’s conclusions as our own, but we certainly must take his hypotheses seriously. The Rise and Decline of Nations promises to be a subject of productive interdisciplinary argument for years to come.

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REFERENCES


Liberalism against populism: A confrontation between the theory of democracy and the

The virtue of this book, fortunately, is not in its title, and its bottom line is rather disappointing. "It seems clear to me," Riker writes, "that democracy cannot be preserved simply with the liberal interpretation of voting" (p. 249). Who, one wonders, ever assumed or expected this? So, Riker concludes, democracy's survival depends on all those constitutional limitations for which James Madison persuasively argued in The Federalist. This is hardly a tantalizing conclusion, were it not for the fact that Riker previously devoted the bulk of the book to an exegesis of alternative voting systems and their implications, from the point of view of social choice theory, for the populist and liberal interpretations of democracy, at least as he construes them. Surely, analysts other than those committed to the "discoveries" or "revelations"—Riker's terms—of social choice theory have been there before. And they will hardly be impressed by the afterthought that "a wide dissemination of the discoveries of social choice theory is a desirable additional defense" of liberal democracy. Alas, Riker informs, "the dissemination of a rather arcane theory is a task for generations." As he candidly confesses, "It took me a score of years of reflection on Black's and Arrow's discoveries to reject the populism I had initially espoused." And as the prospect of social choice theory becoming the new opiate of the people is rather dim, Riker returns to the old-time religion: "...the fundamental method to preserve liberty is to preserve ardently our traditional constitutional restraints—decentralized parties and multicameral government" (p. 252).

This, then, is a curiously bamboozling book whose merit can only be appraised by dividing it into three parts. Its strongest, longest and central part (Chapters 2–7, some 175 pages) is a theoretical and technical elucidation of alternative voting methods, their meanings in the perspective of social choice theory, and those real-world disturbances such as strategic voting or agenda control that interfere with the intentions of the designers of electoral systems. Taking the reader by the hand, Riker leads him/her in didactic and patient manner through the intricacies of vote counting procedures, demonstrating how, on the road to amalgamating individual choices accurately and fairly, different results can be achieved. This expedition is indeed enlightened by the logic of social choice theory and can be of enormous use to the student of electoral systems and voting behavior who seeks understanding of the complexities involved in the evidently simple act of voting.

In what I consider the book's second and genuinely creative part (Chapters 8–9, some 35 pages), Riker develops a model of disequilibrium in voting systems that seeks to explain the emergence of new issues on the agenda of politics in terms of losers' dissatisfaction with current outcomes of the electoral process. It is a persuasive theory of political change that one rarely encounters in the study of politics, though it is somewhat marred by what I think is a rather unnecessary analogy to "natural selection" in biological evolution. The model is then applied in a fascinating, strictly political interpretation of the development of the slavery issue as a prelude to the Civil War.

The book's third part (Chapters 1 and 10, some 40 pages) deals with what is presumably its major theme—the question of which interpretation of voting in a democracy, the populist or the liberal, is the more viable, given the uncertainties of majority rule due to vote and agenda manipulation, and the unsolved problematics of accurately and fairly amalgamating individual preferences. This, it seems to me, is by all odds the most unsatisfactory part of the book because its analytic logic is accompanied by polemical language, obiter dicta and, in places, sermonizing. Only an extended essay could come to grips with the book's major theme which, after all, has been around for a long time and has occupied generations of political philosophers and social scientists alike. It is quite clear that Riker, long a distinguished student of democratic institutions and an advocate of positive theory in the study of politics, is deeply troubled by the implications of the paradoxes of voting for democratic governance. For, insofar as social choice theory unmasksf the liability of voting systems and especially of majority rule, it leaves the theorist uncomfortable in a "democratic" envi-
environment that, after all, exists despite the theory’s demonstration that its survival is predicated on untenable assumptions about the meaning of collective decisions. The task, then, is to make these assumptions viable; and in order to do so, Riker seeks to show that while they remain untenable if one accepts the populist interpretation that “the opinions of the majority must be right and must be respected because the will of the people is the liberty of the people” (p. 14), they are tenable if one accepts the liberal interpretation which “simply requires regular elections that sometimes lead to the rejection of rulers” (p. 248).

Contrary to what one might expect, Riker makes the case against populism in favor of liberalism, both partially presented and highly stylized, in terms of literary arguments which are cogent, even though, it seems to me, they derive more from traditional concerns and actual experiences—such as the ultimately coercive quality of populism or the inherent tolerance of liberalism—than from social choice theory. What I find flawed, then, is not the argument itself but the method of argument. First, by positing a strict dichotomy between populism and liberalism and reducing the latter to “a negative ideal” (p. 242), his defense of liberalism in terms of social choice theory will drive every thoughtful person out of the temple of liberalism. One need not be a “populist”—almost a swear-word in Riker’s lexicon—to hold that an election can and sometimes does mean more than retaining or rejecting an official or party. Indeed, in his discussion of the emergence of new issues Riker concedes just that.

Second, by concentrating on voting as evidently the only way to hold officials or parties responsible (and it is not at all clear for what, other than that losers are dissatisfied), Riker altogether neglects the representational aspects of liberal democracy. Representation itself, of course, makes for a theoretical and practical can of worms, but it is surely not alien to liberalism as it has come down from the seventeenth century, and the struggle for democracy in the nineteenth century was eminently a struggle for representation of as much concern to liberals as to populists. I cannot see, therefore, how a liberal interpretation of elections, even though it rejects the naive-populist version of a “popular will” being represented, can do without factoring the issue of representation into its calculus. That rulers in a democracy are, can, will or should be responsive to “people” (interests, classes, support groups, influentials, or what not) is a notion not beyond the bounds of liberal democracy. In other words, one need not be a populist “mandate theorist” to interpret elections as something more than merely trading one set of rulers for another. In short, whatever contribution social choice theory may make to an understanding of elections in liberal democracy, it is a partial contribution that does not preempt alternative or complementary meanings that can be given to electoral outcomes. Insofar as social choice theory in its wisdom dampens undue enthusiasms of such alternative or complementary interpretations, it performs an important theoretical function.

But if one allows for representation in the electoral calculus, the dichotomy posited by Riker between the populist and liberal interpretations of voting appears as a false dualism. It seems preferable to conceive of a continuum that stretches from a hypothetical populist-positive, even if unacceptable, pole to a hypothetical liberal-negative and equally unacceptable pole in the interpretation of voting in a democracy. Most real-world electoral events and outcomes are likely to be somewhere in-between. How responsive a particular voting system makes representatives to the represented is surely as significant a question in a democracy as the social choice question: how accurate and fair is the voting system.

Stanford University


This is an interesting, original, and elegant work in theoretical industrial economics. It is described in the Preface as “a progress report,” not “a book in the traditional sense of the word” (p. iii), and that description is accurate. A number of ideas and models are treated in an illuminating fashion, but the treatment is
not fully integrated. It must also be noted that some of the more important ideas and models have been published (in somewhat condensed form) in the *Bell Journal of Economics* and are thus widely available (von Weizsäcker, 1980). Despite these problems, and others I note below, this book deserves to be read by theoretically-inclined specialists, even if they have already read the *Bell Journal* article.

The central theme is von Weizsäcker's contention that the definition of "barriers to entry" in industrial economics should be changed. The traditional definitions of Bain and Stigler have a positive, descriptive focus: they are concerned with identifying factors that protect incumbent firms from the competition of potential new entrants. Von Weizsäcker argues that we should adopt instead a definition with a normative, prescriptive focus; he would define barriers to be "socially undesirable limitations to entry of resources which are due to protection of resource owners already in the market" (p. 13). He analyzes a number of different models of imperfect markets in light of this definition by comparing market equilibria with first-best optima, finding barriers only if the equilibria involve insufficient entry relative to the optima. (In modeling equilibria, he excludes strategic behavior aimed at deterring entry. While this is surely appropriate in many situations, I find this general defense of this approach unconvincing, pp. 13–15.) He goes on to argue that the presence of entry barriers in his sense is a necessary but not sufficient condition for the desirability of government intervention to encourage entry, (pp. 18–19). It follows that the absence of such barriers rules out the desirability of such policies.

I think von Weizsäcker performs a valuable service by making it quite clear that the presence of entry barriers in the sense of Stigler or Bain does *not* constitute a sufficient case for government intervention to promote entry. But the traditional concept of entry barriers is quite useful in applied positive analysis, and I see nothing to be gained by abandoning it. We might thus consider retaining both concepts, perhaps labeling the traditional one "private barriers to entry" and calling von Weizsäcker's "social barriers to entry." It would make sense to add this new terminology only if the concept of "social barriers to entry" were likely to be useful in the sort of policy contexts for which it seems to be designed, however, and I do not think it would be.

The problem lies in the use of the first-best standard in von Weizsäcker's definition. To take the simplest example, in his analysis of scale economies (Chapter 4, summarized in von Weizsäcker, 1980) von Weizsäcker shows that Cournot equilibria with U-shaped average cost curves may involve more firms than the first-best optimum. But if the first-best point is not feasible, it does *not* follow that the best feasible point will also involve more firms than the Cournot equilibrium. Thus it does *not* follow from the absence of "social barriers to entry," in this case or in general, that no intervention that encourages entry (and perhaps makes additional structural or behavioral changes) would be socially beneficial. In order to answer questions of that sort, one must evaluate the social gains from alternative feasible government actions. Given that such evaluations must be carried out in any case, it is not clear that one learns anything of additional value by an analysis of "social barriers to entry."

At this point one might ask why I recommend the book, since I disagree with its central theme. The answer is simple. There is a great deal of modeling and analysis here that is of considerable interest in its own right, even though it does not seem to me to make the case for the importance, let alone the primacy, of the concept of "social barriers to entry." The treatments here of scale economies (Chapters 3 and 4) and uncertainty about product quality (Chapters 5 and 6) are well worth reading, even if one has already read the summaries of those treatments in von Weizsäcker (1980). The analysis of innovation (Chapters 8 and 9) is similarly interesting and original, and as far as I know it is not available anywhere else even in summary form. The discussions of absolute cost advantages (Chapter 2) and of the interaction between risk and capital requirements (Chapter 7) contain a number of valuable insights and observations. In this book one encounters a clear, rigorous, and original thinker grappling with some of the central issues in industrial economics. Though I wish he had been able to find time to produce "a book in the traditional sense of the word," those inter-
ested in theoretical industrial economics should be grateful, as I am, that this "progress report" is available.

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REFERENCE

030 History of Economic Thought; Methodology


JEL 82-1035

J. C. Gilbert's book, Keynes' Impact on Monetary Economics, is enjoyable and worthwhile. The title, however, is a bit misleading; one which better reflects its content might be "Reflections on the Keynesian Revolution by an Eclectic Keynesian" because, rather than presenting a tightly knit discussion of Keynes' impact on monetary theory, Gilbert surveys a variety of developments in macroeconomic thought, giving the reader his assessment of these developments.

Gilbert's style is to state an issue, summarize two or three economists' views on that issue, and then give his assessment, both of the issue and of other economists' viewpoints. For example, in Chapter 13 Gilbert first reviews Keynes' different views on the stagnation thesis. He then considers arguments by D. H. Robertson, T. Wilson, J. Williams, A. Hansen, A. Sweeney, W. Fellner and H. Johnson (among others), comparing and contrasting their arguments with Keynes' views.

Because the book covers so much ground, and is based on so many textual references, it reads much like an annotated bibliography, connected by margin-comments. (There are roughly nine footnotes per page). While Keynes' ideas are the connecting thread, often Keynes' impact on various schools of thought is only tangentially discussed.

The book is organized into four parts. The Introduction discusses "Keynes the Man" and surveys the literature on the Keynesian revolution. The second part discusses critics of Keynes, both his contemporaries and the monetarist revisionists. It includes two long chapters on the work of A. C. Pigou and D. H. Robertson, a shorter chapter devoted to the work of H. G. Johnson, F. A. von Hayek and W. H. Hutt and two chapters on Milton Friedman and monetarism. Part Three discusses Keynes' "disciples," under which Gilbert includes economists such as Joan Robinson, James Tobin, G. S. Shackle, Robert Clower and Axel Leijonhufvud. The final part considers the policy implications following from Keynes' work, specifically focusing on the stagnation thesis and monetary and fiscal policy.

As can be seen by this four-part division, the book covers a variety of topics. In discussing these topics, Gilbert does a good job; he has no specific ax to grind and sees some good in almost all approaches. For example, his conclusion on the stagnation thesis is the following:

Whether advanced capitalist economies have experienced secular stagnation in the past and what is the likelihood that they will be faced with the problem in the future are questions that remain unanswered. We cannot say of the economist, with Omar, 'He knows about it all; he knows, he knows.' [p. 230].

His reasonableness is that of a Keynesian; Gilbert has a definite soft spot for "Keynesian policies," although as he rightly points out, precisely what these policies are is subject to dispute.

Gilbert's discussion leaves certain gaps in the development of theory, especially in regard to recent macroeconomic developments such as rational expectations, implicit contracts, or Jurg Niehans' theoretical work on monetary theory. Additionally, Gilbert focuses on issues that seem more relevant to the history of economic thought than to economic policy today. For example, much of his discussion on Friedman's thought is based on papers given at a 1970 Sheffield Money Seminar, with little updating. I suspect part of the problem is the time lag from writing to publication. The Preface is dated 1980, and much of the book was probably written in the 1970s; it was not published until 1982. Such a lag significantly reduces the book's currency.

Despite these qualifications, I must reiterate that I found the analysis both enjoyable and worthwhile. (I am not sure, however, that it
is $50.00 worth of enjoyment, but outrageous prices are a problem of all books published in England.) The most enjoyable, and I suspect most useful, parts of the book are those focusing on the history of economic thought. Gilbert's viewpoints are especially interesting here since he was a student at the London School of Economics in the 1920s and is friends with many of the economists whose contributions he considers. When he is discussing these issues, Gilbert's style becomes lively and exciting.

One theme which runs throughout his history of thought section is that the classics had a far more sophisticated view of the problems of monetary theory than is generally thought. They (especially D. H. Robertson) were working on problems of monetary dynamics, and the comparative static models that developed in the post-Keynesian literature do not do them justice. Thus Gilbert's book adds another step in the rehabilitation of Robertson.

In conclusion, I would say that Gilbert is a reasonable Keynesian economist, and his book provides a definite contribution to the history of macroeconomic thought. It is especially useful reading for graduate students who generally receive far too little training in the history of thought. For specialists, however, it is less useful because it breaks no new ground and offers little in the way of integrative thoughts. But then, few books do, and Gilbert's has the advantage of not claiming to do so.

DAVID C. COLANDER

University of Miami

100 Economic Growth; Development; Planning; Fluctuations

110 ECONOMIC GROWTH; DEVELOPMENT; PLANNING; FLUCTUATIONS


This is a welcome companion to I. Adelman's and S. Robinson's 1978 contribution; it provides the first complete account of what have come to be called "computable general equilibrium" (CGE) models. The presentation is clear, accurate and gives an excellent overview of one specific type of modeling undertaken mainly at the World Bank (with which the three authors are associated). Because of this specificity, the volume unfortunately cannot really be used as an advanced textbook in economic development and planning; but clearly, it should strongly be recommended to graduate students in the field, and to all researchers in economic modeling, whether from developing or from developed countries.

The book can be subdivided into three parts: Part I deals with good old linear models, with emphasis on input-output analysis, and linear programming (Chapters 1–3). The core of the volume is devoted to methodological aspects of CGE model building for closed and open economies (Chapters 5–7), and includes the representation of income distribution mechanisms (Chapter 12), which was actually the starting point of this CGE story (in Adelman and Robinson, 1978). The third part illustrates what can be done with CGE models; the applications are centered on the Turkish economy (Chapters 8–11); one application, probably the most unusual and interesting, explores reactions of three "typical" (archetype) developing countries to similar external shocks (Chapter 13).

The Chapters on linear models cover the theory of static and (to me, useless) dynamic input-output models, and linear programming "planning" models of the type constructed during the sixties and early seventies. Even if this kind of modeling is not very fashionable any more, I do think, in opposition to the authors, that whether one represents an economy as a mathematical (linear or non-linear) program, or as a CGE model is just a matter of taste. Both approaches can be made theoretically correct, to represent competitive economies (and even to a certain extent, market imperfections, like tariffs and taxes and macro-economic features); and both can be made theoretically wrong. The discussion on "shadow prices and market prices" (p. 78–80) which gives the main arguments to dismiss programming models, is perhaps too hasty. It is true of course that usually, "planning" models rep-
resent only "one consumer" competitive equilibria; and it is true also that it is much easier to solve models with several groups of consumers when they are constructed under CGE specifications (because then the problem is to find a solution of a system of excess demand functions, or correspondences); and of course, when income distribution is the main issue, as in Adelman and Robinson, or in Chapter 13 of the book, the model builder is concerned with several groups of consumers. But not all models include this, and the Turkish model used to illustrate CGE models in the book is a one private consumer model ("In the Turkey model . . . the consumer demand functions are identical across groups in the private economy" p. 346). The discussion of "trade policy in a linear programming framework" (p. 85) is also unsatisfactory: of course, tariffs can be taken into account in programming models, and there is nothing wrong in writing a balance of payments at tariff-ridden prices (since agents are actually faced with these prices), as long as the tariff proceeds appear (in the right hand side of the balance of payments) as a lump sum transfer. Finally, the problem of terminal conditions, arrived at on page 87, clearly states the difficulties of dealing with dynamics in a "planning" context and I fully agree that the specifications found by model builders are unsatisfactory. But I do not agree that the solution given by CGE model builders is much better: the "two-stage dynamic formulation" of page 173 et seq. is indeed only a "practical solution (which) represents a retreat from the ambitious attempts of earlier planning models at determining intertemporally efficient or optimal growth paths" (p. 174) and has not very much in common with true dynamics.

The Chapters on CGE model building (5–7, 12) give an excellent and full account of the state of the art (including the weaknesses, like the problem of dynamics referred to earlier). A (not too) inexperent model builder can almost go, feed the computer, and end up with a very successful CGE model. This is also the part of the book most convenient to use as readings for graduate students.

Chapters 8 to 11 illustrate the working of a CGE model of Turkey. The authors examine first the effects on resource allocation of varying tariffs and subsidies under flexible exchange rates. They then turn to explore the welfare effects of devaluations and of two import rationing schemes under fixed exchange rates: fix-price rationing (which allocates foreign exchange to the various sectors in proportion to desired imports; actual imports are then a fraction of desired imports) and premium rationing (the consequence of which is a variable, but uniform import surcharge). In Chapter 10, the 1973–1977 Turkish foreign exchange crisis is analyzed; more specifically, through a series of interesting simulations, the authors try to assess the orders of magnitude of the various factors which caused the change in the equilibrium exchange rate of the Turkish pound. Chapter 11 comes back to more classical simulation scenarios for Turkey, 1978–1985.

Chapter 13 deals with the most challenging application of the book. The authors construct data for three fictitious typical developing economies, and, using the CGE modeling approach, explore the consequences of similar external shocks, especially on the distribution of income among (four types of) workers and capitalists. The various policies are then compared in terms of political feasibility, as if, after the shock, elections were held and the incumbent government judged on its past performance.

In sum, and in spite of the weaknesses and somewhat unfair treatment of programming models, this is an excellent book. Though my background is more of the programming type, the authors have almost convinced me, should I build another multisector model, to make it CGE.

VICTOR A. GINSBURGH
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REFERENCE

120 COUNTRY STUDIES

Books of this kind appear with unfailing regularity every three months and with much the
same aim—to apply economic analysis, without “ideological bias,” as the editor of this work would have it, to current policy problems. It may be said at the outset that this book is superior to most of its kind, largely because of the skill and enthusiasm of the writers rather than because of any additions offered to the stock of feasible policy solutions.

The Mancunians—Pickering, Cockerill and Jones—give the volume a good send-off with a thorough review of the methods of analyzing industrial performance in the U.K., highlighting the division between those who see improvements in performance brought about by a coordinated industrial strategy on corporatist lines (as tried by both Conservative and Labour governments in the 1970s), and those who would confine the government’s role to promoting competition and free trade. As in other contributions, momentum is lost when it comes to the tricky question of giving content to a policy package. Indeed, in the contribution of Sussex-based Freeman, one of the best short accounts I have read of the diffusion of micro-electronic technology and the structural unemployment problems it can create, the author virtually contrats out of any policy discussion. Likewise, Cheshire, also Sussex-based, lays bare the slender basis of energy forecasting, and while arguing strongly for “economic” energy pricing along familiar lines, avoids discussion of the changes in the organization of fuel and power production which could give effect to his recommendations. Kentish-man Hughes would have us make haste slowly in trade union reform, whereas his perceptive use of economic analysis reveals all too clearly how urgently reform is necessary if the Pickering et al. recommendations for improving industrial performance are to have any meaning.

The standard of analysis is maintained in later contributions and there is a welcome emphasis on the nitty-gritty problems of economic policy. Hemming and Morris (Institute of Fiscal Studies, London) use a different tack and predict that the kinds of tax reform contemplated by the present Government will take the tax system further away than it is at present from where it should be. Theirs is a piece full of ideas and if most of these are colored by their neo-utilitarian welfare stance (which they naively assume all decent persons will accept!) so much the better if it retains our interest. Yorkist-Maynard, while characteristically exposing inefficiencies and inequities associated with socialized health and higher education provision in expert fashion, fights shy of unqualified support for liberal remedies which subsidize consumers rather than producers of social policies—the symbols of pragmatism have been added to the York coat of arms. The volume is rounded off by an equally efficient review of the tragic history of British housing policy in which state-owned rent-subsidized housing combined with subsidization of owner-occupiers has achieved the worst of all possible worlds in trying to help the worse-off. The author, Ray Robinson (we are safely back in Sussex), keeps his cool and maintains our interest to the end of the volume by a full analysis of the implications of income-based subsidies for housing occupants.

These essays are a good advertisement for British economics. If they do not bridge the gap successfully between policy analysis and its effective implementation, that may only be evidence of the intractability of the problems to be faced if the performance of the British economy is to improve.

ALAN PEACOCK

University of Buckingham, U.K.

130 Economic Fluctuations; Forecasting; Stabilization; and Inflation


JEL 83-0094

In 1979, the Institut Français des Relations Internationales was established in Paris, headed by Thierry de Montbrial, one of France’s outstanding young political economists. By 1982, the Institut had made its mark by producing two unusual reports on the state of the world economic system. The second such report, published in English translation, is a handsome volume, complete with copious
displays of six-tone charts in three-dimensional perspective and printed on the kind of hard glossy paper one expects to find in an IBM annual report.

The process of preparing the report was as impressive as the volume itself. The French authors held various working sessions with American scholars—most of them from the Cambridge-Washington corridor—to check facts and impressions and to debate policy.

What the authors described as their goal was "a truly European vision of the world." What they achieved was a statement whose French origins are as unmistakable as quiche lorraine.

For a book whose main focus is the world economy, the structure is slightly unconventional. Fortunately, Part One is devoted to arms and energy, subjects that deserve much more emphasis than international economists normally give them. Part Two runs through the familiar territory of international trade, international monetary affairs, and the world recession, followed by an exploration of the present crisis within the capitalist and socialist camps. Part Three deals mainly with disappointments and failures in the management of various national economies. And Part Four is a potpourri of geographical forays, beginning with the Islamic world, moving through China, and terminating a bit awkwardly with relations among developing countries.

The specialist in international economic relations will find little that is new in his or her immediate area of expertise. But the report is broad-ranging and occasionally penetrating, so that practically every reader can expect to pick up some rewarding facts and insights. The discussion of future threats to stability in the field of energy, for instance, is especially useful. In addition, the sections on eastern Europe, China and Islam are likely to prove particularly interesting for American readers.

The analysis and prescriptions of the report range over far too much territory for any systematic review. The general tendency in the analysis, however, is to picture Europeans as being largely bound by their geography, their history and their politics; hence, operating with few remaining options. Meanwhile, those outside of Europe are much freer to choose: the United States because it is so richly endowed; Japan because it has a peculiar capacity for national organization; Saudi Arabia because of the luck of the draw.

The tone of the report fluctuates from sober, insightful analysis à la BIS to racy passages reminiscent of The Economist and occasionally to bitter near-diatribe. Whatever the mood, however, the principal villain in the piece is almost always the same—that aging, inward-looking, ideological colossus, the United States. In energy, Reagan’s reliance on the magic of the market is seen as high on the list of global problems, especially when coupled with the possibility that the United States may one day abandon its allies and fall back on its continental resources. In trade, it is the new U.S. protectionism that is one of the largest threats to the stability of the world trading system. In monetary matters, the blind adherence of the United States to monetary targets is said to have deepened and widened the world’s recession; the possibility that the United States may block an adequate flow of dollars to the rest of the world is said to threaten the world’s recovery.

In foreign aid, U.S. intransigence in the expansion of soft IDA funds stands as a major block to the recovery of the poorest of the poor countries. And, of course, the east-west trade policies of the United States are a central factor in creating tension between the west and the Soviet Union.

These views alone would hardly create a basis for labelling the report as distinctively French. As the report itself insists, these views are widely shared in Europe and are fairly representative of a “truly European vision.” Indeed, any typical set of opinions drawn from economists and political scientists in the Cambridge-Washington corridor, including the opinions of this reviewer, would have much in common with the IFRI report on these points.

What disqualifies the report as European are some of its other emphases. For one thing, the monetarist policies of Thatcher and Schmidt are treated with scarcely more tolerance than those of Reagan and Volcker. For another, the aborted socialist experiments of Mitterrand draw a restrained and tolerant commentary that contrasts oddly with the characteristic tartness of the report, while France’s mischievous role in the NATO alliance and its intransigence in the affairs of the European Eco-
nomic Community are hardly mentioned. Finally, while the problems of Africa span twenty pages of the report those of Latin America are just barely mentioned, reflecting the uniquely French distribution of interests among developing countries.

There is a more subtle omission, however, that hints at its French origins. Ambitious as the scope of the report claims to be, it barely mentions the revolutionary changes that have been occurring in the organization of world industry and the organization of world markets over the past decade or two. In the international markets for automobiles, chemicals, metals, and mining, industrial concentration has been rapidly declining. In the high technology industries, joint ventures between firms of different nationalities have been appearing. These shifts cannot fail to be portentous in the rest of the 1980s. Yet, only where these changes have intruded egregiously into the affairs of national states—as in the case of oil and of money—have they gained the writers’ attention. Implicitly, the forces of international change are seen as coming from governments, not from industry. *Sic semper gallia;* British or German collaborators would have helped to right the balance.

RAYMOND VERNON

*Harvard University*


In this work James Perkins sets out a variety of arguments for his policy solution to stagflation. The basic idea is simple: aggregate demand needs to be expanded to lessen unemployment but it must be expanded in such a way as to not contribute to inflation. This is achieved by maintaining government expenditure while reducing government revenues. The consequent increased government deficit is apparently to be financed by borrowing, although this aspect of the argument seems to be inadequately dealt with. The public is assumed not to change its behavior in response to the increased government indebtedness, simply to react to the direct goods and money market effects of the boost to demand—wealth effects are ignored. Monetary policy is supposed to be “tight” and the closer the economy moves to full employment the more it should be used to reduce the stimulatory effects of the tax cuts. “Full employment” is used in this book in its older Keynesian sense so that while part of Perkins’ policy preference causes reductions in the natural rate of unemployment and part brings the economy nearer the natural rate, he makes no distinction between the two effects. Unemployment is always involuntary in this book.

The arguments are presented entirely verbally, without a single equation, and there are just three small tables of aggregate OECD data. Thus, while the book might be about the British and Australian economies as is suggested in the preface, one does not feel that the book is adequately grounded in the domestic institutions of any country. Lacking an anchor to a specific country, and without a mathematical model to impose consistency, the reader tends to feel that the author is rambling around without a specific structure. The ideas generally appear easy but the presentation confused—sections often seem to bear little relation to those which preceded them. Part of this problem arises from the basic conception of the work. Perkins tells us in the preface that his earlier *The Macroeconomic Mix to Stop Stagflation* (1979) “contains a fuller exposition of the ideas underlying the basic policy proposals” so that the work under review is devoted to developments of the ideas, expansions, responses to earlier criticisms and other miscellanea. It is very much an addendum to the earlier work and, by itself, exceedingly difficult to follow. The author claims that with additional evidence available on the deficiencies of the preferred policies of most Western governments, the time is ripe for a rethinking of alternatives and (implicitly) for a reconsideration of his own ideas. One wishes that he had written a revised edition of his earlier work integrating criticisms, developments and new evidence.

In an Australian context the most dubious assumption of the work is that employment would increase if taxes were lowered. It is not at all clear that those presently working would be unable to capture virtually all of the benefits of the tax cuts with none flowing over to the unemployed in terms of new jobs. Currently the high real rates of interest available seem
to be associated with little additional investment and real wage increases run in front of price increases. It is difficult to see how the Perkins' strategy attacks the nervousness of investors or the bargaining abilities of the union leaders. The discussion of the reasons why such policies as Perkins proposes are not followed, ignores the real possibility that much Australian unemployment may be union-voluntary. A national level policy of tax reductions imposed by a federal government provides no basis for union leaders to claim any successes which would justify their position in office. While wage gains are something they can be seen to have won for their members, tax cuts are not, so that unions are very likely to go on seeking wage rises despite the tax cuts and may well accelerate them in a climate of more buoyant aggregate demand. It seems beholden on Perkins to design a policy mix which provides some gains which union leaders can claim to have won if he expects to moderate wage claims while the economy is stimulated. If he is talking about real world economic strategies then he must define his policies to deal with these types of real world problems.

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REFERENCE


200 Quantitative Economic Methods and Data

210 Econometric, Statistical, and Mathematical Methods and Models


JEL 82-1105

The early developers of game theory expected it to have a dramatic effect on economic thought and methodology. Oskar Morgenstern took every opportunity to criticize conventional economic models as being inadequate to account for many phenomena of strategic interaction. In viewing game theory as having the potential of displacing some parts of economic theory, Morgenstern writes:

... should game theory prevail, the break with conventional economics would go much deeper than the one that occurred in the 1870's with the arrival of the marginalistic schools. The latter were a fairly continuous development within the framework of the concepts that had originated in the eighteenth century... It was von Neumann's firm conviction that the state of economic theory now is comparable to that of physics before Newton (1958, p. 172).

Martin Shubik's book provides the most complete account of modern developments in game theory since Luce and Raiffa's book (1957). Shubik's volume reviews developments in most branches of game theory and, aiming to demonstrate the theory's great usefulness, provides an extensive review of the wide applications of game theory in the social sciences. Because of this comprehensive coverage Shubik's book offers us an opportunity to evaluate the dramatic expectations of the early developers as reflected in Morgenstern's thoughts. We start, however, with a short statement of the nature of the volume at hand.

Shubik's book is oriented towards the general audience of social scientists in order to assist researchers in the use of game theory as a vehicle to model a large number of diverse phenomena in the social sciences. The emphasis is on concepts and ideas; thus all formal proofs are omitted and the basic concepts of the theory are explained in great detail with the aid of examples and diagrams. In covering such a vast arena, the author was forced to allocate only a limited amount of space for advanced topics. The book can best be used, therefore, as comprehensive reading material covering most of the basic concepts of game theory. The exposition of the more advanced topics is relatively brief and therefore less satisfactory. In fact, some advanced material receives such limited coverage that the only readers who will understand it are those who are already familiar with it. There are instances in which the discussion of some subtle concepts does not go beyond a heavy list of references. For these reasons the reader who seeks advanced material in game theory is likely to use this only as a guide to the literature. These critical points should not obscure the fact that the book fills an urgent need and is destined