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## REVIEWS OF BOOKS

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R. DE ROOVER, *Business, Banking, and Economic Thought in Late Medieval and Early Modern Europe: Selected Studies*, Edited by Julius Kirshner, Chicago and London: The University of Chicago Press, 1974, pp. VIII-381.

This welcome assignment provides the present reviewer an occasion to offer some belated words of appreciation and comment about the life and work of Raymond de Roover (d. 1972) with whom he maintained close relations throughout the course of his academic life in the United States. If the personal observations are not reserved for another place as originally planned, it is because he feels that these will now prove helpful in adding depth and precision to the characterization offered by the main editor of the present work.

### I

In some spheres de Roover was the greatest master of our time; in others into which he ventured with tremendous enthusiasm and vitality he could not achieve results which will stand time's tests.

The key to de Roover's strength was in the excellence of his grasp of the buzz and whirl of everyday business, including all the turns in the terms of trade and exchange from time to time and place to place. The utterly best work he did was in this vein. I will admit that I never enjoyed our conversations so much as when they related to the actual histories of medieval, Renaissance, and early modern enterprise and enterprisers.

Our own relations luckily began on a discovery of the complementary character of our interests. If I happily admit to the fact that I was helped by de Roover to have a stronger sense of the need to do sustained research in the shapes and organizations of medieval businesses, I would have to say that I helped him — as he again and again acknowledged in correspondence as well as in print — to acquire a responsible grasp of the enviroing contexts and milieus in which all (economic) action occurred in the medieval and Renaissance eras. Indeed, as he himself declares, it was I who insisted that there was no possibility of appreciating the structures of enterprise in Bruges or anywhere else if there was not a full and clear grasp of the juridical norms which regulated the conduct of business and the juridical forms through which business was obliged to be conducted. Anyone who reads his magnificent book on Bruges will see that he applied this perspective to our great advantage there. He had come to recognize, as I had learned to appreciate, in the spirit of Georges Bigwood and others, that matters stood differently for those who came to be known as the *mercatores*, the *cambiatores*, the *usurarii*, the *foeneratores* — the merchants (or merchant-bankers), moneychangers, usurers, pawnbrokers, etc. All these terms had precise significance and were applied as public law notions by which courts, external and internal alike, were guided.

In the same spirit the forms of associations and of contract were guided by the structures that were forged out of Roman Law texts and fused with the theological materials from the Old Testament and the New. Unlike some recent writers, de Roover was willing to acknowledge that the Church applied great power and effort in seeking to regulate the character of enterprise. He knew that a merely behavioral discussion of what happened would never help us recover the actual forms of activity within the lives of the medieval men and communities.

On this last point it needs to be said that de Roover maintained a deep attachment to life-history methods of the study of enterprise to the very end of his days. Although not himself formally trained as a historical specialist, he recognized that the study of all flows overtime involved continuing reference to the specificities of milieus which underwent change as a result of human interaction. Again and again in conversations with me he expressed his regrets that so much of the American work in present-day economic history had been directed to statistical representations of aggregate trends or to a calculus of the changes in the volumes of business or receipts of product across times. The predominance of a retrospective cliometric interest struck him as inhibiting the carrying forward of the studies of actual histories.

De Roover was not expressly interested in sociology or in the development of a general set of notions on the interpretation of sociocultural process. For this reason his frequent reference to Max Weber and his intentions and

accomplishments prove less than persuasive. Yet his best work reveals an implicit understanding of the most advanced methods and perspectives which are coming to prevail in historical sociology. In my own words, he was concerned with the history of business and economic ideas as parts of socio-cultural process against the settings of all horizons which were of relevance in the lives of peoples.

## II

So far as the traditions of European economic history go, Raymond de Roover stands in the direct line of succession of the great masters. Through his work there course the vitalities and powers of such men as Henri Pirenne, Charles Verlinden, Guillaume des Marez, Georges Bigwood, Armando Saponi, Yves Renouard and others of that stamp. It would probably be best to say that it was from the Americans, N.S.B. Gras and A.P. Usher, that he developed his full grasp of the importance of the history of business practice, business organization and entrepreneurship in economic history. As far as his formal teachers went, it was largely from Schumpeter that he acquired his interest in the history of economic ideas and in specificities of the terms of trade.

De Roover shared the view of Schumpeter and others (including myself) that the history of economic analysis did not originate with Adam Smith. For this reason, as well as for others, he sought to go behind Adam Smith to deal with the medieval writers.

It may be admitted that de Roover did not begin this work with the help of a systematic training whether formally or personally acquired in the fields of law and jurisprudence (Roman, canon, international) and moral theology and philosophy. However, he did work extremely hard to develop strong control of these fields and came to acquire copies of many of the important texts for his own library. In the later years of his life he was vigilant in collecting reprints of publications of medieval canonists, civilians and moral theologians. It was evidently his hope to complete a major work on the history of medieval theory before Adam Smith. Would that he had been spared, that he had been able to live long enough to complete this ambitious undertaking on which he had come to set so high a price:

Unfortunately, this very area which de Roover came to prize most was the one in which he never became entirely at home. The reasons for this are many:

a) Even at the end of his life, de Roover fell short of having assured command of the juridical literature — the Romano-canonical sources including the *consilia et responsa* — and the treatises in moral theology from the XIIth to the XVIIIth Centuries.

b) He never truly felt enriched or challenged by the study of the

structures of casuistry as the jurists and theologians wended their way through new circumstances and new demands made upon them. At various times in the course of his writings he came to think that all this casuistry was simply verbiage without any actual relation to the setting of affairs. Again and again it was necessary to bring to his attention the fact that his own documents did not fully yield themselves up without a thorough knowledge of these materials. This disdain of the historical specificities of these turns of thought in the end militated against his completion of a major study of the making of the economic mind of the modern era.

To speak last about the editorial aspects of this work:

Fortunately there are two sections to the Introduction of this volume. Richard A. Goldthwaite correctly senses that « the most salient characteristic of de Roover as an economic historian was his overriding interest in business techniques and organization ». He, therefore, makes a painstaking effort to locate and assess de Roover's main work in the history of accounting, banking and business organization against the wider background of research before him and in his time. Goldthwaite wisely prepared himself for this undertaking by carefully interviewing Florence Edler de Roover, Frederic Lane, and others.

By contrast, the main editorial essay on « Raymond de Roover and Scholastic Economic Thought » by Julius Kirshner is marred by recurrent arbitrariness of standpoint and overripeness of rhetoric. Indeed, Kirshner's account of the history of research in the field of economic ideas of the middle ages and the early modern period offers an oddly patterned sense of the contemporary thrust and base of research in the field. By contrast to Goldthwaite, he fails to communicate a feeling of the actual structure of de Roover's efforts, achievements, and lasting worth. Much of this might have been obviated if Kirshner had followed Goldthwaite's lead in interviewing scholars who were close to de Roover.

The following lines are set down in the hope of documenting and offsetting excesses of Kirshner's claims:

However high the estimate one might wish to place on the relevance of de Roover's remarks for resolution of the issues posed by Max Weber, it clearly runs to extremes to say, as Kirshner does, that « For medievalists intent on joining the scholarly wars over the Weber thesis, de Roover's work has, over the past decade, become a rich quarry of facts, judicious interpretations, and imaginative assertions ». Time and again at public meetings de Roover found himself having to agree that he had not looked closely enough at the issues to permit him to make a decisive contribution to the Weber problem. I have sought to throw light on some of these questions in several of my papers, especially my « Droit Canon, Protestantisme et "Esprit du Capitalisme". A propos de Max Weber ». (*Archives de Sociologie des Religions*, 34, 1972, 3-23).

Kirshner appears to be at odds with himself through a large part of this introduction. Thus on the one hand he talks continually of *scholastic economic thought* and dates it from Albert the Great and Thomas Aquinas to Nicolaus Broedersen. Surely a better way of referring to these men, one that might have spared the author and ourselves a great deal of confusion, would be to talk of them simply as moral theologians and jurists who related to issues of contract and exchange *now* deemed the special province of economists. Kirshner is not wholly responsible for ambivalence on this issue. Thus, he notes that de Roover « became exasperated with the tortured analogies, muddled metaphors, and ponderous legal jargon permeating scholastic discourse ». Kirshner's ambivalence and propensity for rhetorical flights are never so clear as when he writes in conclusion:

A perusal of recent volumes on economics and on the history of economic theory reveals that de Roover's as well as Schumpeter's advertisements on behalf of the Scholastics have fallen on deaf ears. There are several reasons for this. The overwhelming majority of economists do not believe that economics has a usable past. For a Paul Samuelson or a Milton Friedman, the professional utility of Bernardine's pronouncements on value is zero. It is as useful as a five-hundred year old weather report. Economists resist, moreover, de Roover's bold attempt to award the Scholastics with belated membership in their profession. There is justice to their admissions policy. It is simply a cross-cultural misunderstanding to speak of the aims and methods of the Scholastics and modern economists in the same breath. The Schoolmen's flashes of comprehension of the rudimentary principles of economics do not make them self-conscious economic theorists. They were au fond professional moralists. Spiritual rather than material prosperity and spiritual rather than analytic rigor were the twin destinations of Scholastic economics.

Yet the flight from Anachronism should not lead to Antiquarianism. The gulf dividing modern economics from Scholastic economic theory does not bar the former from contributing to our understanding of the latter. The great merit of de Roover's and Schumpeter's work stems from their application of modern economic theory to Scholastic doctrines in order to shed light on a network of ideas which had hitherto been imperfectly understood and to allow us to appreciate the insurmountable difficulties the Scholastics confronted in their attempt to unravel a skein of economic relationships.

The eleven pieces here anthologized do, indeed, offer a sense of de Roover's major interests and achievements. No one wishing to know de Roover at his best, however, will allow himself to forego de Roover's published volumes, above all his splendid books on Bruges and the Medici Bank. Given Kirshner's focus, it would have been helpful if de Roover's not readily available synoptic essay on Joseph A. Schumpeter and Scholastic Economics » [Kyklos 10 (1957), 114-46] had been added to this anthology.

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J. DE VRIES, *The Economy of Europe in an Age of Crisis, 1600-1750*. Cambridge University Press, Cambridge and New York, 1976, pp. 284.

This is a bold, compact, and attractive synthesis which can be used both as « the state of the arts » by scholars doing comparative work and as a text for advanced students. It emphasizes changing structures rather than growth, as indicated by chapter headings such as « The agrarian economy on divergent paths », « Restructuring industry », and « Capitalism creating its own demand ». Its most unusual feature is its initiating date: the early seventeenth century rather than the sixteenth or midfifteenth, as is the case with other recent preindustrial texts such as Ralph Davis (*The Rise of the Atlantic Economies*, 1973) or Hermann Kellenbenz (*The Rise of the European Economy*, 1976) — not to mention Carlo Cipolla (*Before the Industrial Revolution: European Society and Economy, 1000-1700*, 1976). De Vries makes out a case for 1600-1750 by referring to the « two periods of notable economic expansion » that preceded and followed, and by arguing that structural changes becoming visible after about 1625 were responses to a multifaceted “crisis”. Those who do not accept such an analytical framework will still, I feel, welcome this fine book and use it in their courses and in their own work.

Its dust jacket claims that this book « makes a special effort to apply economic reasoning to the economic forces of the period... ». Nothing elaborate is involved, however; the analysis employed is a commonsensical melting of the findings of monographic studies into moderately-expressed middle-range generalizations. For example, at one point in his excellent chapter on agrarian change, de Vries ties together the stabilization of XVIIth-century population and the increase in food output in certain regions of Europe; this in turn serves as an explanation of how it was possible for the more successful farmers to supply burgeoning cities and armies, and also how the rural poor started on their “proletarian” path before enclosure became an important factor.

Other engaging features of this work are its small girth (something less than half the wordage of Kellenbenz and Davis); useful maps and graphs; and the clever amalgamation of certain topics, for example coupling the discussion on urbanization with that on regional trade. The extensive material on Holland — which we would expect from the author of *The Dutch Rural Economy in the Golden Age*, 1974 — will be especially welcomed. One amusingly up-to-date feature is the section on « energy supplies », with good material on the exploitation of Holland's peat resources. This work also takes advantage of « new social history » and treats it in a more analytic fashion than does Kellenbenz or Davis. And de Vries handles the significance of national economic policy for economic development in a most attractive fashion.

As is only to be expected in a work of this sort, it is easy to find bones to pick. Much of the analysis here, for example, depends on the relative decline of grain prices; this is supposed to have increased consumption of meat, beverages, and manufactures by giving food buyers additional real income. But this depends on the food buyers' money income remaining steady during these "crisis" years, which seems puzzling. Many developments treated in this work are explained as a response to « an age of crisis »; but some of them (for example, the exodus of urban industry to the countryside) were under way during the "boom" of the sixteenth century. And not enough credit, I believe, is given to the effects of European expansion and in particular to the gains from transatlantic slavery. The concept of a 150-year « age of crisis » itself does not go down easily — certainly we can accept it for 1610-50, but it seems less certain for 1650-90, and rather questionable for 1725-50. However, in the richly-woven tapestry of general explanations, specific insights, and satisfying examples this book gives us, such knotty threads can be seen as outward and visible signs of the effort involved in putting such a work together.

Those of us working with preindustrial socioeconomic history are now (so suddenly!) faced with a splendid avalanche of English-language texts. (In addition to the four already mentioned there is, of course, Fernand Braudel's *Capitalism and Material Life, 1400-1800*, 1973). It is to be hoped that de Vries' text, the most usable of them all, marks the coming of age of this specialization in the sense that model-builders primarily interested in the nineteenth century who need early modern history to demonstrate the general applicability of their theses, will henceforth have no excuse for advancing weighty propositions on the basis of one or two slim concepts. What seems more certain is that these five texts (all but Kellenbenz available in paperback) should make teaching this subject less of a chore and more of a pleasure.

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M. W. DOLS, *The Black Death in the Middle East*, Princeton, University Press, 1977, pp. XVII-390.

Pointing out that prices in Southern Europe and the Middle East were closely in line, E. Ashtor (*Histoire des prix et des salaires dans l'Orient médiéval*, pp. 509-11) states that, in the late Middle Ages, the Mediterranean was « an economic unit ». By far the most dramatic illustration of this unity was the Black Death which, originating in the Central Asian steppes, reached Constantinople in 1347, was transmitted, by merchant ships to Egypt and Italy that same year and then spread over Europe and the Middle East. « Undoubtedly the worst disaster that has ever befallen mankind » (p. VII), its impact on Europe has received much attention, but this is the first full

scale, and by far the most thorough, study of its course and effects in the Middle East, more particularly in Egypt and Syria.

Drawing on the vast medical literature on the plague, the voluminous historical writings on medieval Europe, Byzantium and the Middle East and a vast body of manuscripts and printed sources in Arabic, Dols admirably covers in his first chapters the history of the plague before the Black Death, the chronology and geography of its transmission, modern medical views on its nature and Muslim medical, religious and magical interpretations and practices.

Chapter V is devoted to the knotty question of the demographic effects of the plague in Egypt and Syria. Unlike their more fortunate counterparts working on Europe, Middle Eastern medievalists have neither parish records nor censuses on which to draw. Dols sifts the available literary evidence, both contemporary and posterior, but is very cautious in his conclusions. The total population of Egypt and Syria, and of the principal cities, can be conjectured by extrapolation from early modern figures, from cadastral surveys and from estimates of area and density. To determine the mortality caused by the plague, three sources, all incomplete, can be used: the figures given in the official registers (*diwan*), "the number prayed over in the oratories, and the number of coffins counted at the city gates" (p. 207). For Cairo, Dols' estimate of mortality is 100,000, a figure that agrees with those given by Mamluk historians and suggests a loss of at least a fifth — and perhaps considerably more — of the city's population. For Egypt as a whole, the « population of between 4.2 and 8 million may have declined by about one-quarter to third » (p. 218); it may be added that the population was probably much closer to the lower of the two figures. Damascus seems to have suffered more, its population of 80,000 falling by an estimated 38 per cent, while that of Syria has declined from some 1,200,000 to 800,000 (pp. 219-20). Losses in the Mamluk army seem to have been much higher than among civilians, and both directly and indirectly reduced its effectiveness. Lastly, in contrast with Europe, plague recurrences led in the Middle East to « repeated and substantial reductions in population after the Black Death » (p. 231).

Chapter VI deals with urban communal behavior during the Black Death, and here too an interesting contrast is observable. « The European response to the Black Death included the abandonment of religious rites and services for the dead; in the Middle East, on the contrary, there appears to have been a rigorous exercise of traditional Muslim religious practices during the crisis » (p. 253).

Chapter VII discusses the economic consequences of the plague. Agricultural prices seem to have at first risen and then stabilized, as lower

population and demand was met by lower production. Manufactured goods both rose in price, and declined in quality because of the loss of skilled craftsmen and higher wages. Wages rose sharply, as in Europe, while rents and income from land declined, a phenomenon that had deep repercussions in a society where both the military and religious establishments drew their sustenance from rents and endowments of farm land and urban real estate. Moreover, the reduction in the rural populations « does not seem to have aided the long-term improvement of agrarian technology or the re-allocation of resources — as in Europe » (p. 271). Commerce seems to have markedly fallen off. More generally, the economy of the Middle East entered a long period of decline.

I have only one small fault to mention in this excellent book. On page 279 a misprint « The shrinkage of agricultural goods » (read *land*) has made nonsense of what I think was a rather elegant argument put forward by me some years ago!

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J. D. DURAND, *The Labour Force in Economic Development*. Princeton, N.J.: Princeton University Press, 1975.

Labour force analysis as a separate field of endeavour in the social sciences barely antedates World War II. In the relatively short period since then, concepts and techniques and data collection have advanced significantly, and one of the forces they have documented is the considerable, if complicated, interrelationships between economic development and the changes in the size and composition of a nation's work force.

John Durand, the author of the volume under review, was a pioneer in this field. In fact, in a volume which appeared almost thirty years ago he said... « labour force estimates are usually the cornerstones with which planners begin when they build models of the national economy for future years. Such estimates will be increasingly important in the future as the development of economic planning activities proceeds both in private industry and in government ».<sup>1</sup> This indeed has come to pass not only in the U.S.A. and Europe but in most parts of the rest of the world.

In this volume, Dr Durand examines the economic development — work force connection from a world-wide point of view based on an analysis of data from 100 countries focused on the period 1946-1966. International comparisons, even in fields with much more sophisticated and extensive data bases than labour force, are mine fields with all sorts of traps awaiting the unwary, generated by everything from definitional differences to attitudinal differences stemming from a country's history and cultural heritage. The

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<sup>1</sup> J. D. DURAND, *The Labor Force of the United States 1890-1960*, New York, Social Science Research Council, 1948.

author is very careful in making these overt and the volume is replete with cautionary statements to make sure that the readers understand the reservations and qualifications that have to be taken into account in assessing the book's findings.

Substantively, the volume begins with an overview of international labor force trends, noting the general tendency during the twenty years following World War II for female labor market participation rates to increase while the opposite prevailed for men. The author then discusses regional patterns, dividing the various countries into nine classifications based in good part on their linguistic, religious, ethnic and political characteristics. Readers of this *Journal* will be interested in examining the contrasting trends among one European region including the Eastern, nations and those in Middle and Northwestern Europe.

There then follows a detailed exposition of the complex interrelationships between the process of economic development and the size and composition of work forces in general, and the dynamic of labor market participation among men and women in particular. A final chapter reviewing the author's major findings and a series of appendices (covering almost one-fourth of the book) conclude the volume. Some of the elements of commonality among nations in this arena apparently include the fact that just about all men in the middle-adult years do work; that economic development does seem to be accompanied by declines in worker activity at both ends of the age scale among the men; that women do take on increasing work activity at higher levels of economic development; and that the redeployment of a nation's economy from rural to urban, from agricultural to nonagricultural pursuits is a prime force in generating corresponding redeployment of various age and sex groups in the part they play as workers.

Dr Durand also presents a number of hypotheses which, again, will have to be validated by future research. One of the more interesting ones which he describes under the rubric of « hypothesis of convergence » is that the higher the worker rate among women at the beginning of a country's development, the likelier it is to fall as economic growth ensues — and vice versa: the lower it is to begin with, the likelier it is to go up. What we end up with, therefore, is convergence of these rates at some medium range in the process of economic development.

All in all the volume is characterized by a high level of technical craftsmanship and the presentation of a wealth of data in a highly professional analytical frame. It ought to provide a solid vantage point for future research, especially that which will take advantage of a mass of new data which became available through collections in and around the year 1970. Together, it will represent a correspondingly solid vantage point for helping in the design of programs and policies for transacting with the development process around the world.

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E. FOX-GENOVESE, *The Origins of Physiocracy. Economic Revolution and Social Order in Eighteenth Century France*, Cornell University Press, Ithaca and London 1976, pp. 325.

In commenting upon this most interesting study I must confine myself substantially to what I consider to be its most important contribution. Thus I shall not dwell on the fact that Mrs. EFG very rightly insists that the phenomenon of Physiocracy must be seen as part and parcel of the general movement of XVIIIth century French enlightenment. She does so situate it in her lively and rich presentation of that most heterogeneous « age of reason », and she shores up her picture with a wealth of reading (regrettably not compiled, along with the other literature mentioned, into a full register for the reader's benefit).

Neither shall I dwell on the account of the formative years of the two figureheads of the movement, Quesnay and the Marquis de Mirabeau; although on the whole it reassembles rather well-known facts, it takes particular care, especially for Quesnay, to tie up his philosophical outlook with the major trends of the time.

The central theme of the book grows out of the fact that Physiocracy is not only an economic theory but a theory of government as well; that, while the former is progressive, which, at the time, means the advocacy of freely competitive (capitalist) enterprise, the latter clings to traditional, i.e. feudal institutions.

The existence of this fundamental contradiction which explains why Physiocracy in its developed form could not live, has of course been shown up long ago. Marx was first, e.g. in his *Theories of Surplus-Value*, to lay bare the interaction of these fatal antagonisms: while « the Physiocratic system... is the first system which analyzes capitalist production... (it) presents the landowner as the true capitalist, that is the appropriator of surplus labour... It was the first to explain surplus-value by the appropriation of the labour of others. On the one hand, it stripped rent — that is the true economic form of landed property — of its feudal wrappings and reduced it to mere surplus-value... On the other hand, surplus-value is again explained in a feudal way, as derived from nature and not from... social relations... Hence the conclusions which the Physiocrats themselves draw, the ostensible veneration [Marx wrote « Verherrlichung », i.e. glorification — MK] of landed property becomes transformed into the economic negation of it... all taxes are put on rent, or, in other words, landed property is in part confiscated » (l.c., Part I, pp. 49-53, Moscow 1963).

A specific aspect of the contradiction, the shift occurring in the apportioning of the Physiocratic « net product » among its recipients, was examined by R.L. Meek, who considered the shift as more or less accidental, in his *Economics of Physiocracy* (p. 269 and 359 f.). And on the basis of new material, I was able to locate the break in its definite form as having been

presented as early as 1759 (cf. my edition *François Quesnay, Ökonomische Schriften*, vol. I, p. 441, Berlin 1971): there the net product brought forth by the capitalist farmer — who had been granted a share in it in *Grains* (1757) — goes exclusively to its three feudal “owners”, the landed proprietor, the king, the tithe men. And between these two poles lies a yet more pronouncedly untraditional, or even antitraditional, argument in *Hommes*, where consumption no longer suffices to justify feudal appropriation: « Anyone who participates in the wealth of the kingdom, and in no way contributes to it, is of no use to the state... for if he does not return to the mass of riches the value he takes out of it, its needs will diminish... what he himself fails to produce directly or indirectly, will not be there; thus it would be just as well if that useless man would himself not be there » (translated from the manuscript among the Du Pont papers, p. 124 f.).

Thus the gap — with no indication as to what caused it. Mrs. EFG has bridged it. She gives us the means of understanding how, in all probability, the break came to be made, and to be made for good. She does this by examining the manuscripts drafted by Mirabeau in the time between his “conversion”, by Quesnay, in the summer of 1757, and the publication of Quesnay’s “third” edition of the *Tableau économique*, about autumn 1759. The analysis is intended to show that the influence of Mirabeau on Quesnay has been far greater than has been generally assumed. A very large part of her book is devoted to retrieving Mirabeau from the onus of being considered as no more than « the oldest son of the doctrine ». Most important among the three documents examined for this purpose is the *Traité de la monarchie* subject, as is shown by the many annotations both in the “founder’s” and in the “son’s” hand, of a particularly close cooperation and of intense controversy. Mirabeau, member of the first estate, hereditary landowner, fierce traditionalist, intent on restoring the former “purity” of the monarchy, accepts Quesnay’s totally new conception for the “restauration” of the state’s fundamental source of wealth, agriculture, only up to the point where he senses the threat to the property relations upon which rests the existing social order, feudal absolutism.

Mirabeau’s insistence on the significance of property relations was of course well founded — and so was Quesnay’s initial perception that a changed economy needed amended property relations. Political economy sees precisely in the particular form of the ownership of the main means of production the specific characteristic of the succeeding modes of production — slavery, feudalism, capitalism, socialism. Obviously, Mirabeau was able to deflect Quesnay from the road along which he had begun to travel. The result is a compromise, for good: to the end of his life, Mirabeau will see the basic factor of all social advancement in the (capitalistically produced) net product, and Quesnay will turn back and adopt the outright feudal mode of distribution.

Indeed, « the manuscript of the "Traité" helps piece to together the tale » (p. 14) of the doctrine's ultimate development. And furthermore, Mrs. EFG neatly ties the tale up with the specifically Physiocratic concept of government by the "legal despot" who derives his authority from the correct interpretation of (divinely created) natural laws — which are economic laws — and by ensuring their smooth functioning. In all this, we must not forget, and Mrs. EFG does not forget it, that Quesnay always favoured reform, not upheaval; nor should one forget that already in *Hommes* Quesnay does plead — and this is insufficiently tied up with the *Traité*, on pp. 128-131 and 181 ff. — for a monarch rather closely resembling the "legal despot".

On the occasion of a possible second printing of the book — (it is, incidentally, beautifully printed and contains hardly any printing mistakes, the errors in the short bibliography on p. 38 f. being of quite a different nature) I would wish for a number of changes.

First and foremost: the broader socio-political approach toward which the author seems to incline so much more than toward the approach via political economy should not lead to the repeated dismissal of contributions by Quesnay as mere technical matters (e.g., p. 169, 216, 154); nor should it lead to the assumption that an intellectual feat such as the *Tableau* could possibly have been « first devised in part to instruct (the) disciple » (p. 169); still less should it lead to the reproach that Quesnay "admits" the *Tableau* to be an abstraction (p. 258), and least of all to the following juxtaposition: only « Quesnay himself appears to have understood the mechanism. And Quesnay ended his life, studying geometry, convinced that he had squared the circle » (p. 268 f.). So great a man as Lenin has warned us that achievement should be measured by what preceded it, and not by the progress of succeeding centuries.

In spite of castigating « the scholarly mania for intellectual pedigrees » (p. 96), Mrs. EFG fortunately does not neglect that useful exercise; but I miss precise chapter and verse in the case of quite a number of authors thus mentioned — Shaftesbury, Mandeville and others. And I would like much more proof for Quesnay's having to be considered an historian, even « a great economic historian » (pp. 54 and 97): the example of China does not seem to point that way; and is the possession of many historical dictionaries really an argument? Occasionally the erudition of the book is marred by slips — class being determined by income levels (p. 238), or Quesnay and Mirabeau being said to "*develop*" [my italics - MK] the argument that agriculture alone assures the wealth of the state » (p. 198) whereas this had been a commonly held view for centuries. The pertinent statement follows a few lines further.

Wishes such as these mostly concern details which could easily be

attended to. The overall fact remains; one lays down the book with feeling that one is the richer for having read it, that it indeed adds much to the understanding of that complex phenomenon, Physiocracy.

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J. GIMPEL, *The Medieval Machine: The Industrial Revolution of the Middle Ages*. New York; Holt, Rinehart and Winston, 1976. pp. XI, 274. Illustrated.

The idea behind Mr. Gimpel's book was a useful one. In the reading public at large there may still be many people who think of the Middle Ages only in terms of the Romance of Chivalry and the Age of Faith. They should be told something about the work done since the Second World War on medieval technology and the medieval economy. Gimpel's execution of this idea, however, leaves much to be desired: especially a sense of the difference between the present and the past and the difference between historical and economic analysis and the mongering of superficial parallels. One hopes that the general reader will be knowledgeable enough to recognize the work for what it is. (Reviews in the press suggest this will be the case). So much time would have to be spent correcting erroneous information and pointing out anachronistic interpretations that the book would be useless for undergraduate teaching.

Not that Gimpel has conceived of his task narrowly. Quite the contrary. Chapter one discusses water mills. Two, the threefield system; the heavy wheeled plow; the horse collar; changes in diet and changes in climate. Three, mining. Four, the destruction of forests and the smell of burning "sea coal", with a brief passage through public baths. Five, the working conditions of the miner, the cloth-worker, and the mason. Six, the sketch-book of Villard de Honnecourt. (Gimpel is the author of an earlier book, *The Cathedral Builders*, from which this has been lifted). Seven, the mechanical clock. Eight, natural science speculation among university teachers. Nine, a hodgepodge of miscellaneous late medieval disasters. He has gone to the best secondary literature: Carus-Wilson, Le Roy Ladurie, Duby, Forbes, Sicard, White, and others. When he follows them closely he is accurate if unexciting.

The entire work, however, is cast into a thesis defined by the subtitle (the original title in French) and spelled out in a self-indulgent epilogue where Gimpel constructs a parallel between the rise and fall of the medieval economy and the modern American economy, written in the now classical style of *chic* gloom. Cistercian houses are equal to "factories" because they used mills. Mining stone and mining coal are economically equivalent because

they both involve taking resources from underground. The invention of the mechanical clock led to "time is money". Repeatedly Gimpel latches on to those moments of rhetorical overkill to which almost every academic author is occasionally prone in order to give scholarly credentials to his argument. He wishes to convince us that Europe in the Middle Ages went through a period of technological innovation, risk-taking, and economic expansion precisely comparable to the Industrial Revolution. The disasters of the fourteenth and fifteenth century that brought an end to this will soon beset us in their modern forms. Needless to say, no serious analysis of either the medieval or the modern economy emerges to support his argument, which is based solely on a set of "equations" of the sort mentioned above.

FREDRIC L. CHEYETTE  
Amherst College

G. C. HERRING, JR., *Aid to Russia, 1941-1946: Strategy, Diplomacy, The Origins of the Cold War*, Columbia University Press: New York, 1973, pp. XXI-365.

This study eliminates a great deal of misconception about Soviet-American relations during World War II. A persistent view, originating in the forties and fifties, is that President Roosevelt coddled Stalin with overly generous aid, thereby merely enhancing Soviet power. More recently revisionists have contended to the contrary that American aid was the instrument of a calculating, self-interested policy of prying open Eastern European markets. Herring convincingly argues that the United States expected a great deal from the ten billion dollars it devoted to Russian lend-lease, and that the formulation and failure of its postwar aid policy is a far more complex story than the revisionists suggest.

Not naive and unrequited generosity but shrewd strategic calculation motivated Roosevelt. So long as Russians were killing Germans, he believed it to be in the highest interest of the United States to provide every means possible to assist them. With delivery of goods painfully short of commitments on account of the U-boat and staggering logistical problems, and with the Western allies forced again and again to postpone invasion of Europe, it seemed all the more important to assign absolute and unconditional priority to Soviet requests. Keeping in mind that the Red Army alone bore the brunt of German power and that twice before, in 1918 and 1939, the Soviets had made their peace with the Germans, Roosevelt was determined to demonstrate good faith and reliability as an ally. Lend-lease ultimately contributed substantially to the common victory, less, in Herring's estimation, by way of weapons than automotive and rail transport, food, clothing, and factories. Two-thirds of all major Soviet industries were rebuilt with Amer-

ican equipment or technical assistance. Granting that lend-lease helped to restore Soviet power, he finds it impossible to conceive how the outcome could have been more satisfactory to the United States without it; indeed, the result would probably have been much worse.

Herring makes an equally impressive case against the alternative strategy proposed by Averell Harriman, ambassador in Moscow, after the years of dire crisis passed. Ever more insistently in 1944 and 1945, Harriman argued that Soviet-American relations should be a two-way street, the United States requiring Soviet concessions in return for lend-lease and postwar credits. A *quid pro quo* policy would make the Soviets more amenable to American conceptions of postwar order in Eastern Europe. Herring is rightly dubious that any aid could be sufficiently attractive to modify Stalin's aims in a region of such paramount importance to Soviet security. Nevertheless, the Harriman approach was at least constructive in calling for a "well-forged instrument" (p. 154) of diplomacy. As it turned out, American aid was never effectively employed, either as enticement or reprisal, in modifying Soviet behavior.

Herring's most valuable contribution is his analysis of the complex and conflicting currents that destroyed any possibility of a coherent diplomatic strategy for dealing with the Soviet Union in 1945. To begin with, Roosevelt, while perhaps raising Soviet expectations of postwar assistance, failed to prepare the American Congress and public for it. Lend-lease had no sanction beyond that of prosecuting the war. In addition, the shrivelling effect on generous impulses of Soviet expansion in Eastern Europe, the declining importance of the Soviets for victory over Japan, vigorous anti-communism among top advisers, and the pinch-penny, nationalistic, and partisan spirit of Congress all militated against deployment of aid as a bargaining lever. Equally destructive was the bureaucratic confusion attendant on conversion to peace and transition from ailing Roosevelt to insecure Truman leadership. The result was brutally abrupt cancellation of lend-lease on V-E Day and misplacement of papers (not deliberately, according to Herring) approving a Soviet loan in principle. Vacillating at first, the Truman administration moved steadily toward a posture of standing up to the Russians, withholding assistance to create the impression of firmness. If the United States did indeed seek to extend its economic power into the Soviet orbit, it could not have chosen more counterproductive tactics. As this cogently reasoned book shows, an aid policy of supreme mutual advantage to the Soviet Union and the United States ended in a slough of mutual suspicion. The United States, though not for the reason assigned by economic revisionists, did contribute to the coming of the Cold War.

WALDO H. HEINRICHS Jr.  
Temple University

S. HOWARD - D. WINCH, *The Economic Advisory Council, 1930-1939, A Study in Economic Advice During Depression and Recovery*, Cambridge, Cambridge University Press, 1977, pp. VIII-424.

This book consists of 164 pages of texts, followed by 189 pages of selected reports of the committees of economists in Britain in the 1930s, known variously as the Committee on the Economic Outlook, the Committee of Economists, and the Committee on Economic Information, together with various appendices listing the Economic Advisory Council committees, and their reports plus the various persons involved. Taken as a whole it presents a fascinating account of the increasing use of economic advisers by the British government in the 1930s and the evolution of Keynesian doctrine which made that advice more consistent and more effective.

A reader who wants a total view rather than to cull information on one or more points would do well to move back and forth from the text to the selected documents, both chronologically organized, reading each report upon its first mention in the text. This necessitates first ascertaining which of the long list of reports prepared have been selected for publication, since the text does not always specify when it mentions a document later included. The selected reports are also valuable in themselves as material for a study of the history of economic thought (rather than economic history). Many more proposals for action were advanced than were acted upon.

A number of themes emerge from the authors' treatment: 1) the difference between committees of representatives of such constituencies as employers, labor, government, trying to agree on policy recommendations through compromise, as opposed to the technocratic views of expert economists, concerned with data, causality, repercussions, etc.; 2) the conflict between long- and short-run recommendations, not always neatly separated, as exemplified by the persistent notion that exports should be increased through rationalization of industrial-production techniques, as opposed to such short-run measures such as devaluation and subsidies; 3) the frequent changes of mind of Keynes (mentioned especially on pp. 88, 89, 92, 96, 98, 137) until he developed the Keynesian system of the *General Theory* that gave him a consistent theory to tackle macro-economic problems of depression on the one hand, and relieving the pressure of rearmament on the other; 4) the conflict between policies designed with Britain alone in mind — e.g. import tariffs, and devaluation, and the relatively few occasions when the reaction of British moves on the world were thought about. Under the last count, the reviewer found particularly interesting the Fourth Report of the Committee on Economic Information, « Survey of the Economic Situation, July 1932 », that contained the Keynes-Henderson plan for an international note issue by the Bank for International Settlements, closely conforming to the Special Drawing Rights of the International Monetary Fund of 1962. The memorandum cites Bagehot's *Lombard Street* and the financial crisis of 1825

(p. 277) and 1866 (p. 273). It does not use the term « lender of last resort » as R. G. Hawtrey does in his contemporaneous *The Art of Central Banking* (London, Longmans, Green & Co., 1932, p. 228). Hawtrey seems more realistic, suggesting that the Bank for International Settlements might one day be the lender of last resort, but that meanwhile the function could only be discharged by a foreign central bank or a group of foreign central banks in concert. A fascinating letter from Keynes to Prime Minister MacDonald on August 5, 1931, however, suggests that since it is certain that Great Britain will go off gold, it is unwise to borrow to keep it on (p. 89). This was after the first Franco-American loan for sterling, but before the second.

The struggle between parochial and cosmopolitan economic interests runs through the deliberations of the economists. From time to time, but not often, they wonder about the impact of various policy courses for Britain on the rest of the world. It crosses the collective mind of the Committee on Economic Information in September 1931 in its first report dealing with the balance of payments that world prices must recover for Britain to recover, so that the country should not think exclusively in domestic terms (pp. 253-254); and in March 1932 it is noted that the depreciation of sterling may have contributed to the further fall of gold prices (p. 262). On the whole, however, the debate from October 1930 between tariffs and devaluation, in which Keynes changed sides frequently, was conducted in partial-equilibrium analysis, ignoring retaliation, multipliers, feedbacks of all sorts. As one contemplates the authorities of the United States, Japan, and France jockeying for exchange-rate advantage in the summer of 1977, it is hard to detect much progress.

The text of Susan Howson and Donald Winch is scholarly, fully annotated, detailed, dense, and in the early pages a trifle dull. Perseverance pays large dividends, however, and the book as a whole is a highly informative chapter in the history of the application of economic theory to policy. It would be useful to have parallel studies for this and other periods, but especially for the 1930s for the United States, Germany, France and Japan. Wilhelm Grotkopp's *Die grosse Krise, Lehren aus der Überwindung der Wirtschaftskrise 1929/32* (Düsseldorf, Econ-Verlag, 1954) does the task well for the critics of the Establishment. What is needed, however, is an account of the advice produced officially, and the extent to which it was followed.

CHARLES P. KINDLEBERGER  
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J.R.T. HUGHES, *Social Control in the Colonial Economy*, Charlottesville, University of Virginia Press, 1976, pp. VI, 178.

Convinced that the « free enterprise system » does not exist now and that it never has existed in America, the author has gone back to the colonial

period to seek the roots of the tradition of non-market control at the governmental level. What he has found seems new to him, although it should hardly come as a surprise to an economic or legal historian. Drawing his findings largely from secondary materials, mostly at least a generation out of date, Mr. Hughes points out that the Supreme Court in the Granger Cases of the 1870s reached far back to the early seventeenth century in America in upholding the government's authority to regulate business affected with a public interest.

The author's quest has taken him back to the origins of the system of land tenures, modified and simplified in America to make common socage in fee simply amount to meaningful ownership. He then examines the controls exercised by a variety of governmental agencies over immigration and labour, particularly of public callings, scrutinizes the local market controls prevailing in the colonies, and finds that the various restrictions on sales as regards times and places were gradually eroded both in rural and urban areas. Pointing to the early introduction of price and quality controls, he shows how even sumptuary legislation was transplanted to these shores for a time.

This well-intentioned peregrination has been undertaken to demonstrate that early America was anything but a hotbed of *laissez-faire* philosophy. Perhaps in pummeling this straw man Professor Hughes has performed a useful service, but he has not necessarily persuaded the reader all along the line. The point should be made, but in this rather superficial survey it is not, that most of the non-market controls were on their way out long before the American Revolution. Even by his own evidence the author shows that the English laws of trade ran counter to that relaxation of non-market controls which one finds in eighteenth century America, and that these cross-currents greatly exacerbated the tension between the colonies and Mother Country. In short, the picture is not as clearcut as Hughes would have us believe. Certainly the debates waged in the Continental Congress over the enforcement of wage and price schedules during the Revolution and the failure of Congress, the states, or local governments to curb inflation or successfully place ceilings on prices and wages that would stick suggest that many of the Founding Fathers, along with Pelatiah Webster, were becoming converts to a *laissez-faire* philosophy. So far as internal non-market controls were concerned, most of the members of the Federal Convention who discussed the issue registered their opposition.

If we cannot put the clock back to a *laissez-faire* time that never in fact existed, neither can we produce evidence that federal or state governments have regularly and persistently advocated non-market controls. Rather does the evidence point to a zigzag pattern, with both the neomercantilist and *laissez-faire* traditions demonstrating enormous vitality in our own day,

and with nobody, even the monetarists, quite sure whether the kind of worldwide inflation we now are experiencing needs more or less governmental intrusion to bring it under control.

RICHARD B. MORRIS  
Columbia University

B. Moss, *The Origins of the French Labour Movement. The Socialism of Skilled Workers, 1830-1914*. Berkeley, Los Angeles and London, University of California Press, 1976, pp. XIII-217.

Professor Moss, with reason, decries the chronicle of unions, strikes, congresses and leaders which has too often posed as a history of the labour movement. His aim is a wider one, namely that of assessing the importance of organization and ideology in making the working class an « active historical agent ». He seeks to provide « a concrete analysis of ideology in its historical connection and context », by combining a Marxist inspired approach with an industrial ideology inspired primarily by Alain Touraine. The objective he sets himself is, in short, to relate institutional and ideological development to the social realities of working-class life.

It has to be said from the outset that he does not really achieve the ambitious aims set out in his preface. The work becomes an improved version of precisely the kind of analysis he condemns. He subsequently excuses himself for this on the grounds that working-class life needs to be explored on a local basis, and that the essential monographs have yet to be written. This is not altogether the case, although the coverage provided by such studies is certainly patchy. One might feel that he could have done something himself to explore the local backgrounds of some of the provincial organizations he mentions.

Thus the fundamental criticisms one is led to make of this book are those Professor Moss makes of his predecessors in the field. How influential were the ideas and organizations he mentions? These were movements of a small minority of skilled workers. To what extent were their ideals and aims more widely shared? Is it really enough to suggest that the influence was far greater than membership statistics suggest and that it grew in moments of social crisis? This is almost certainly correct, but by what mechanisms does this extension of influence occur, and did it have any longer-term consequences? Why did the vast majority of workers remain outside labour organizations? If « between class and ideology lie mediating political and cultural structures » should not Professor Moss tell us more about these? He stresses the importance of networks of relationships developing within urban neighbourhoods but does not enlarge upon this point even for Paris, the strategic centre of the labour movement. Surely also a more detailed analysis of the actual membership of the key organizations should have been

possible, and also a more specific examination of the relationships between leaders, militants and rank and file members?

Later in his work Professor Moss appears to revise his goal, which becomes that of providing a « conceptual framework and historical overview » of the development of labour ideology and organization, as a prelude to the essential local monographs. In this respect — in providing a kind of sociological model, to be tested by future researchers — he is more successful, and the lack of meat on his skeleton becomes more excusable.

He identifies a “core ideology” underlying the many changes and variations in French socialism from the producers’ associations of the 1830s to 1870s to the revolutionary syndicalism of the turn of the century. Both these and politics were conceived of by their supporters as a means by which workers would emancipate themselves from the imperatives of the capitalist wage system.

It is this constant expression by skilled craftsmen of resentment of their cultural and material deprivation, of the erosion of their traditional incomes, security and status as skilled craftsmen, not necessarily due to mechanization but through commercial and financial concentration, and the division of labour by sub-contracting, which is seen to lead the more militant into organizational activity. Their tactics are seen by Professor Moss to have varied as the *political* situation changed — rather than in response to growing industrial concentration, the pace of which, as he shows, should not be exaggerated. The producer’ associations had little practical significance but they symbolized aspirations and provided hope. Their success depended however on government sympathy and support and thus a turning point came with the consolidation of the bourgeois republic in 1879. This confirmed the old distrust of the motives of radical republicans, and finally turned militants from the pursuance of socialism by peaceful transition towards revolutionary aims. Subsequent divisions within the labour movement were about tactics and organizational forms; about the relationships between union and political activity and between leadership and rank and file, as leaders were time and again tempted towards reformism. It was difficult to maintain revolutionary aims in a situation increasingly favourable to reformist gains. It was difficult to avoid increasing centralization within organizations as the focus of social action moved from the local trades and communities to the level of the national industry and centralized state.

The story is familiar, but Professor Moss, on the basis of substantial archive and library work, has within the limits stressed above, provided us with new and sensitive insights. He deserves to be congratulated and his work to be widely read.

ROGER PRICE  
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W. KULA, *An Economic Theory of the Feudal System*, London, Humanities Press, 1976, pp. 191.

Witold Kula's study of feudalism is a major contribution to scholarship, and its translation into English (from an Italian edition) 14 years after its original publication in Polish gives English-language scholars an opportunity to become acquainted with a work that has had a significant — and well-deserved — impact on European scholarship. The objective of the study is to provide a theoretical model of the feudal economic system of Poland between 1500 and 1800. In fact, Kula's study is, in western terminology, primarily a study of manorialism rather than feudalism. In the course of the development of the model, the author provides a thoughtful analysis of the fundamental methodological issues involved in exploring this economic system.

There are two introductory chapters entitled « What Questions Must an Economic Theory of Feudalism Answer? » and « The Construction of a Model » followed by two long essays on the short-run dynamics of the feudal system and then the long-run dynamics. This is followed by a brief chapter entitled « The Possibilities of Verification » and then Chapter Six, « The Rationality of Economic Activity in the Feudal System ». This chapter in many respects should be read early since it provides a thoughtful analysis of the constrained range of choices available in an economic system in which markets are extremely restricted, particularly for the peasant. There is a brief concluding chapter exploring the changes that took place in the nineteenth century and contrasting them with the earlier period as well as an attempt to discuss the nature of model building in the context of what has gone before.

In Kula's study the key to the short-run dynamic was the effort of the landlord to expand the demesne area at the expense of the size of the peasant's plot. This was the means whereby the lord was able to increase the "surplus product". The dilemma for the lord in the context of fluctuating harvests was how to so adjust the plot size of the serf that simple reproduction could occur, which included not only reproduction of the serf and family as a labor force but also so that plots which produced feed for the work animals would also be maintained. If the lord adjusted the plots to be too small, the peasant would either starve the animals and use the produce to feed himself or use next year's seed as food and then come to the lord and ask for seed for the following year. If the lord allowed the peasant's plot to be too large, then the major margin which determined the lord's standard of consumption would be less than it could be. In short, the key margin of decision making for the lord was adjustment of the size of the serfs' plots.

The serf, on the other hand, persistently attempted to achieve some

measure of output above his immediate consumption needs and the payment of rents and obligations to the lord so that he would be able to sell his surplus produce in the towns and thereby purchase goods from town artisans. The key to the income received by both lord and serf is that the fluctuations in the harvest were greater than the fluctuations in prices so that a year of large harvests, despite the fact that it frequently meant lower prices per unit, led to a substantially larger income for both lords and serfs. And conversely, a year of poor harvests was one in which the revenue was decreased because prices did not rise enough to compensate for the lower yields in output.

Kula concludes the section on the short-term dynamic by contrasting the characteristics of the feudal system of Poland with those of capitalism. Under capitalism, he claims, the rise in price leads to an expansion in the growth of national income and the greater utilization of the resources (Kula explicitly has in mind a Keynesian underemployment model). In contrast, under a feudal system there are no reserves to be expanded, and, as noted above, it is in periods of lower prices that the income of both serfs and lords were greatest.

In turning to the long-run dynamics, Kula attempts to explain the structural transformation occurring in the Polish economy between 1500 and 1800. The same tensions are here at work; namely, the persistent effort of the landlord to reduce the size of the plots of the serfs so as to maximize his surplus, and the converse effort of the serf to be able to expand his output sufficiently to have something left over to be able to market and, therefore, to buy goods from the town artisans. This persistent tension is set within a number of changing secular conditions: 1) the growth of an interdependent market for wheat and rye; 2) the changing secular terms of trade which are different for the upper nobility, the lesser nobility, and the peasant; and 3) the growth of autarchy in the inland areas where transportation costs evidently make for more self-sufficiency and less access to the world grain market. Kula observes that the proportion of the revenue of the lord coming from the sale of vodka in local village pubs which he controlled was significantly rising. Kula leaves as indeterminate whether this reflected a rise in the peasant's income or, more likely, a shift in the pattern of consumption of the peasant, since overall he seems to come down on the side that the peasant's standard of living was probably falling.

Overall, this is an impressive study, and, particularly impressive when one is reminded that it was written 15 years ago at a time when the self-conscious use of statistical data, prices, and model building were not widely accepted in economic history and, particularly, by Marxists or the Annales School, which was an important influence on Kula. Kula's intimate knowledge of the subject and his concern with testing the hypotheses all lend credence to his analysis. Certainly, the major point of his study — the lord's

persistent effort to adjust the size of the serf's plot — provides a logical and convincing structure to this case study of feudalism (manorialism).

There are some major gaps in the analysis of which the most important is that, in fact, the study really does focus on manorialism and not on feudalism. We are given only the sketchiest accounts of the hierarchy above the lord and the relationship of the lord with the policies of the state, and, clearly, these are essential if we are to understand the degree of freedom of the lord in unilaterally adjusting the size of the serf's plot. Kula does mention that the serf could, and did, flee, but we are provided with no clear-cut understanding of the institutional structure and changes in that structure which would have raised or lowered the costs to the serf of fleeing.

The behaviour of the lord also introduces perplexing questions. Why does the lord only maximize at one margin; namely, that of adjusting the size of the serf's plot? We are left with the conviction that in all other regards the lord was only interested in conspicuous consumption that determined his relative social status in the system. Yet if the lord was maximizing in one dimension, why not in others? Kula implies that the institutional structure militated against productive investment, but this issue is never really explored.

The third major difficulty in the study relates to the use of prices. While Kula gathers a great deal of statistical data on prices and even calculated terms of trade, the price analysis is weak. There is no overall analysis of the evolution of the grain market internationally and the way by which that evolving market impacted on the Polish economy. The contrast that Kula makes between the price/quantity relationship and national income under feudalism as compared to capitalism is confused to say the least. He confuses causation with association. In general, Kula, in company with other *Annales* and Marxist authors, simply does not understand price theory. Whatever shortcomings emerge from using neo-classical theory in history, its great strength is that it can make sense out of the volumes of price statistics that *Annales* and Marxist scholars like to collect.

These criticisms should in no way lead the reader to underestimate the importance of this study. It is a major contribution not only because it does provide an insightful analysis of the structure of feudalism (manorialism) but, more important, the author is self-conscious and undogmatic in his approach to economic history. Western and Marxist scholars frequently write (and talk) past each other. This study, in contrast, in the very best sense of the words attempts to be scientific, objective, and thoughtful about the basic issues that are of concern to all economic historians.

DOUGLASS C. NORTH  
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B. Z. KEDAR, *Merchants in Crisis. Genoese and Venetian Men of Affairs and the Fourteenth-Century Depression*. New Haven and London, Yale University Press, 1976, pp. XII-260.

This book is a stimulating contribution to the debate on the « depression of the Renaissance ». The problem of the depression is presented as a comparative study of the changing fortunes of Genoa and Venice from about 1280 to 1400. The book attempts to assess the prevailing mood of the merchant class during the passage from the heroic age of commerce (p. 23) to the depressed age of contracting markets.

Kedar begins by presenting what evidence there is to indicate that neither Genoa nor Venice escaped the effects of the general economic depression in Europe that was characterized by the end of direct commercial contacts with Further Asia and the contracting markets caused by the demographic crisis. But his major concern is to show that the state of mind of Genoese and Venetian merchants reaching maturity around 1380 was also rather depressed. He analyses the reaction of merchants to the crisis in much the same way that art historians have studied painting between Giotto and Masaccio. He finds men of business more cautious and less ready to confront adventure and risk; prone to use new notarial formulae like *prudens* and *circumspectus* to describe the best attributes in a man; preferring saints' names to secular names for their sons and their ships, in the hope of gaining the intercession of saintly patrons; turning to maritime insurance to hedge against the risks of piracy and shipwreck. The picture he paints is one of rather clear contrasts between an age in which some men were ready to set out for the unknown in search of adventure and extraordinary profits, and the age of the sedentary merchant who retrenched and routinized commerce in the « inner area ». The new scene is bleak: in the two maritime republics « the fever for commercial gain was ebbing » (p. 66); alternative professions gained in popularity.

The author makes an especially good point when he says that the revolution in commercial technique beginning around 1300 which made possible the sedentary merchant was reinforced by the coming of the depression since the rationalization and routinization of "inner zone" commerce became even more necessary. But there may be a tendency to exaggerate the transfer of the merchant « from the poop to the desk », which resulted in a lack of « disposition for mobility and versatility » (p. 122). Traveling in fact remained an essential part of commercial apprenticeship and even important merchants still traveled — examples are myriad.<sup>1</sup>

<sup>1</sup> In a letter of 1404 Ruggiero Contarini commented, « io son grande e non posso navigar », which the author renders « I am a great man and cannot go on a maritime voyage » (p. 122). In Venetian usage *grande* means rather "of advanced age". A

A further comment. The author holds that the new markets which were opened up at the end of the fourteenth century did not compensate for the sharp decline in commerce during the crisis. If Professor E. Ashtor is right, however, that Levantine industry was destroyed around 1400 through a policy of "dumping" of European products, Italian entrepreneurs must have made some important innovations during the period of the depression, and this bears further investigation.

The historical sources analysed by the author are extremely varied. Since neither Genoa nor Venice produced *ricordanze* of the Florentine type, Kedar turns to the indirect testimony of numerous sources rarely used in economic history, such as herbals, dictionaries, chronicles, *novelle*, sermons, *Les prophécies de Merlin*, and the like. Even where such traditional sources as notarial acts are used, the author extracts unusual evidence on mood from the changing formulae and the names of men and ships mentioned in the acts. Nearly all these sources have been published and they are listed in an impressive and useful bibliography.<sup>2</sup> But the author uses very few Genoese archival documents and none at all from Venice. There are relevant unpublished sources. Testaments have been used fruitfully to provide a direct insight into merchant mentality. There is no lack of them in the notarial archives of either city. Was there during the depression a greater tendency to make bequests *pro anima*? Was there a change in the kinds of bequests? Was there a growing concern for making reparation for ill-gotten gain (*pro male ablati incertis*)? This source is important because its redaction was generally the product of considerable thought and planning and its use would make possible a more systematic analysis of changing mood. A second invaluable source is the thousands of letters written in Genoa and Venice and sent to correspondents in the Datini network, 1382-1410. To be sure, the great majority were written by Florentines but even they reflected the mood of the two marketplaces under consideration. Of course the results of such studies might not differ radically from Kedar's.

These comments point to a different kind of project — and a lengthy one. They do not detract from Kedar's essay, which stands as a valuable and provocative contribution to the on-going debate and a model for the

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reading of the whole unpublished letter shows the writer complaining of ill health — he had been seriously ill for forty days — and thus he presses the addressee in Paris to look after a business venture for him. This is probably not evidence of a "disapproval of the impulse for travel". Furthermore, the writer's own brother Andrea Contarini made at least eight voyages to the Levant between 1392 and 1405. See the 47 letters in *Archivio di Stato di Venezia, S. Giorgio Maggiore*, b. 174 and the extracts — which Kedar used — published by dalla Santa in «Nuovo Archivio Veneto», n.s., 32, 1916.

<sup>2</sup> One might note the omission of the two important volumes on fourteenth century finances and money prepared by R. Cessi for the *Documenti finanziari della Repubblica di Venezia*.

mining of both traditional and unusual historical sources. The author has with *ingenuousitas* heightened our awareness of the problems and offered in a highly readable manner a new approach to their resolution.

REINHOLD C. MUELLER  
University of Arizona (Tucson)

R. A. WEBSTER, *Industrial Imperialism in Italy, 1909-1915*, University of California Press, 1975.

Of the numerous issues in Italian history which still provoke heated controversy in academic and popular circles, the origins of Fascism and Italy's defection from the Triple Alliance are among the most common. Long characterized by stereotypes, clichés and dogmatic assertions generating more heat than light, these questions are of late receiving much more thoughtful, objective, scholarly examination. With the current trend toward sociological and psychological methods for the understanding of historical phenomena, Fascism has lent itself easily to renewed scrutiny. Diplomatic history, momentarily less in vogue, attracts rather less study. But Richard A. Webster, in his *Industrial Imperialism in Italy, 1908-1915*, has made a masterful and virtuoso contribution to the understanding of both problems. Indeed, his is a seminal work.

Rarely has this reviewer seen a book which breaks so much new ground at one stroke. Divided into two distinct parts, this work describes the take-off of Italy's industrial and financial complex in the years prior to World War I and follows the same banking and industrial organizations in their search abroad for markets to absorb the production their homeland's frail economy could only partially accommodate. More complex, subtle, and significant, however, is Webster's narration of the growth of Italian banks and industries (with a small but powerful oligarchy making up the interlocking directorships of a far broader and more diverse establishment and the outlets these organizations found and/or devised (through an unusual degree of support and collaboration by the state). These tightly knit élitist relationships are precisely and conclusively described and documented. The growth of the great Ansaldo, Breda, Terni, Montecatini and Banca Commerciale empires are exposed to that public view their extreme reticence always caused them to avoid.<sup>1</sup>

The first captains of modern Italian industry and banking — Orlando, Breda, the Perrone family, Joel, Weil, Toeplitz, Volpi, Stringher, *et al*,

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<sup>1</sup> For the developments of the preceding decade see Antonio Confalonieri's monumental study, *Banca e Industria in Italia 1894-1906*, 3 volumes, Milan, 1974-1976.

are revealed with a detailed account of the vitally important rôles these elusive figures played in Italian industrial and financial development.

In the second segment of his work, Webster describes the intricate and often difficult endeavors undertaken by the new banking and industrial groups to penetrate the Balkans (notably Montenegro and Albania) as well as the Ottoman Empire through projects which often became inextricably mixed in a single thrust of both the business oligarchy and the Italian state in schemes where the interests of the two merged into one. In their attempts to enter areas where other Great Powers already had a foothold, these undertakings inevitably led to new and different contacts, increasing potential for controversy and ultimately conflict with Italy's Triplice allies as well as with France, England and to a lesser degree, Russia. From such initiatives, Italian actions brought about, *ipso facto*, a menacing transformation of power relationships which led directly to Sarajevo. Thus Italian foreign policy (i.e., treaty commitments) and national economic development tended to merge and effected a wholly revised position for Italy, thus altering European power alignments. The interplay of personalities — French, German, Austro-Hungarian, Albanian and, of course, Italian — fills the story out in fascinating detail.<sup>2</sup>

With no intent of detracting from the worth of this study, several observations are essential. Most notable is the discovery that the American edition of Webster's book is a truncated version of the original Italian edition (Einaudi, 1974). Although the author states in his preface that the shortened version «left [his] thoughts and wording substantially intact», surely he was only attempting to make the best of a preposterous disservice done him by his American publisher. The sub-title of the Italian edition, deleted in the American volume, is telling evidence of the mutilation: «Studio sul prefascismo». In the same vein, graver still, and without justifiable explanation, is the total elimination of Webster's preface and conclusions (entitled, respectively, «Il problema di base» and «Questo studio può insegnare qualcosa sulla natura del totalitarismo dei nostri giorni?»). Although the Italian edition is quite long (607 pp.), no valid reason exists for depriving the English-speaking reader of those vitally important 29 pages where the author's fundamental thesis and overall conclusions are presented. In short, those two brief segments are a *sine qua non* for the fundamental point the author has sought to convey: «... questo lavoro è uno studio sul prefascismo...». Without the more fully developed arguments from the Italian edition, the book's thesis and, to a degree, the book's *raison d'être* are lost.

Doubtless many scholars will wish to contest or discuss the assertions and particularly the conclusions of Prof. Webster. However, the majority

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<sup>2</sup> The recent work of K. E. Kirova, *Italianskaia ekspansia v vostochnom sredizemnomorici*, Moscow, 1973, also sheds interesting light.

of these differences ought to be positive or constructive, for such is the potential to be derived from his original and, in some degrees, audacious views. For example, (and here the Italian edition is the source), is it not too narrow to assert that « ... la conquista dello "spazio vitale" è l'unico elemento di fondo comune ad ogni programma di tipo fascista... »? Webster leaves no doubt when explaining this generalization that he has identified a major factor in the growth of fascism, but that it was "unique" is dubious. Even so, the author seemingly has given flesh and blood to one of Alan Cassel's generic forms of fascism: the "progressive" type found in states seeking to make a leap forward.

Prof. Webster is deserving of unqualified praise for his extensive research in the archives of the Banca Commerciale, the Banca d'Italia and many other collections. Even his secondary sources, so often rare and little known, make an unusual contribution.

On the technical plane one must observe, however, that the dates 1908-1915 are somewhat misleading; the bulk of the study concentrates on the years 1911-1914. Also, while the two-part structural division of the work is unavoidable, the reader would be assisted if Balkan questions were dealt with in one package; and the same must be said for those concerning Anatolia. At least a brief statement seems essential also for the annexation of Bosnia-Herzegovina and the simultaneous declaration of Bulgarian independence, events which certainly played some rôle in this story; but without any reference to them we are at a loss to know how they fit the overall scheme of things.

In conclusion, a few errors must be noted. Beyond the occasional contradictory statement here and there, one finds the more serious assertion (p. 220) that in 1908 Tittoni, in a counter-move with Russia, France and Serbia against Austrian initiatives for a trans-Balkan railways, gave « the first indication that Italy might reverse her alliances ». More than one preceding event demonstrates that Italian allegiance to the Triplice was already more than dubious; Luigi Albertini asserted as long ago as 1942 that the Prinetti-Barrère agreement of 1902 was the "nadir" of the Triple Alliance. Algeciras in 1906 is yet more famous in the same sense. Also the Anglo-Franco-Italian agreement on East Africa (viz. Ethiopia) was completed in December, 1906, not 1904 (p. 333). And ought irredentism not to be viewed as coming earlier than 1913? Or does Prof. Webster use that date as the time when it became a rational and pragmatic issue as opposed to one that previously was more sentimental?

A disturbing lacuna in both editions is a detailed map essential for the location of those many minor locales central to the story but normally found only in the most comprehensive of atlases. The lack of bibliography in both editions, if not a crucial omission, is at least unfortunate.

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J. K. WHITAKER (ed.), *The Early Economic Writings of Alfred Marshall, 1867-1890*. Free Press 1975 Two Volumes pp. XXI, 296 and pp. IX 404.

This is a major addition to the literature of the history of economic thought and it is especially welcome as it is a genuine addition to the material available to students of Marshall, rather than yet another exercise in chewing over old bones. The standard of editing is impeccable both in what has been done and in the explanation of what has been done. Even today, editing standards are widely variable; and Whitaker's comes in the highest class. Apart from Marshall's own material, which makes up the bulk of the two volumes, there is an extremely valuable introduction of more than 100 pages; and it must rank as the most important discussion of the development of Marshall's thought which has yet appeared. There is also biographical material, particularly concerning Marshall's trip to America in 1875 to study protectionism. The picture of the gentle and scholarly Marshall in the Nevada of the 1870's is fascinating; and there is also an account of a remarkable interview with the 83 year old Carey. As an observer of American life Marshall was not, nor would he claim to be, a De Tocqueville; but what he has to say is of interest for the light which it sheds upon Marshall himself.

The first volume contains, apart from this introduction, Marshall's early essays, the bulk of which were probably written in the years 1867-74. These cover value, money, wages, capital, rent, and international trade. None of these essays is without interest and the editorial selection has been exercised with great discretion. The major part of the second volume is concerned with a reproduction of as much as is extant of Marshall's abandoned book on Foreign Trade dating from the late 1870's. This proves to be a rambling and discursive work and the analytical tables of contents which the editor has inserted are of the greatest assistance in following the thread of the argument. This part of the work includes the material which was reprinted in 1930 by L.S.E. as Marshall's *Pure Theory*; and anyone who wishes to appreciate the full quality of the editing in this volume should compare the material as it appears here with the 1930 edition. The remainder of volume II contains: a series of miscellaneous notes on economic theory including Marshall's notes on Cournot, Von Thünen and other economists; some material from a mathematical notebook of 1867-72; some notes on economic theory from the years 1873-87 which includes interesting material on taxation and distribution; and, finally, some writings on social thought and economic policy including the text of Marshall's address on American industry after his return from the United States.

All this is full of interest and no short review can possibly do justice to it. We see the development of Marshall's theory, especially his distribution theory which was the slowest part to mature. It is apparent that,

subjectively at least, he has equal claim with Fleeming Jenkin to the use of curves in demand and supply analysis; and yet the process by which he freed himself from the wage fund doctrine was a slow one. *The Economics of Industry* emerges, despite Marshall's later disdain for the book, as an important stepping stone along the road. The extent of Marshall's debt to Mill is now much clearer. I have always considered Shove's view of this debt greatly (not to say grotesquely) exaggerated; but having now seen Marshall's working papers it is quite clear that Mill was so often his starting point that there is considerable truth in what Shove had to say. But the originality of Marshall himself is undeniable. From the very earliest work in these volumes he was constantly producing ideas which can now be seen to be of significance in the development of economics; and indeed it is apparent that, because of his interminable delays in publication, Marshall achieved less than he might have done. For instance there is in these volumes a macroeconomic growth model which never clearly appeared in his published work; there is the first known application of the phase diagram method to economic theory; and there is an anticipation by nearly 40 years of Barone's initiation of the direct/indirect tax argument.

Having worked through all this, Professor Whitaker quite obviously has views upon Marshall's standing; and some readers may feel that he is less than generous. He is distinctly doubtful of the extent of Marshall's mathematical expertise; he can see little merit in Marshall's attempts to use empirical material; he believes Marshall's debt to Jevons was significantly underplayed by Marshall himself; and he believes (I think mistakenly) that there was a significant change in the meaning which Marshall attached to "Normal" between 1880 and 1890.

But Professor Whitaker is fully entitled to his views. Every succeeding Marshall scholar is going to be greatly in his debt. Stigler once wrote that no one could live long enough to read everything written about Marshall; and no sensible person would want to. But amongst the things which anyone seriously interested in Marshall must read are these two volumes.

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