REVIEWS OF BOOKS

D. R. Adler, *British Investment in American Railways, 1834-1898*, edited by Muriel E. Hidy, Charlottsville, Virginia, U.S.A., The University Press of Virginia for the Eleutherian Mills-Hagley Foundation, 1970, pp. XV, 253.

Developing nations traditionally look to their industrialized trading partners for real and financial aid to establish conditions generally recognized as favorable for the economic growth of the country. A system of transportation is usually considered one of these conditions. The resulting transfer of aid for economic growth generates problems for both the benefactor and the recipient nations. How these problems are best handled for the nations concerned has been, is, and will likely continue to be, a complex issue in a world of have and have-not nations. A close analysis of past experiences of debtor and creditor nations would serve as a guide in future exchanges as well as a clarification of relatively unclear points regarding economic growth of the now developed nations. Further, because the United States stands as a monument to unplanned economic growth, and because that expansion was aided both in real and financial terms by the industrial leader during the century of U.S. development, Great Britain, a study of British investment in United States railroads must be regarded as a benchmark investigation in economic history.

Dorothy Adler was uniquely equipped to prepare a study of British investment in American railways. She was born and educated in the United States before her marriage to a British subject and relocation to England. During her year in England Mrs. Adler furthered her education, taking a Ph. D. at Cambridge in 1958. Her American background, British experience, and a thorough economics education secured in both countries combined to place her in a position to study British investment from a British viewpoint, but at the same time from the vantage point of an American citizen. *British Investment*, then, could have been a truly great work answering the many important questions surrounding sterling and American rails. Unfortunately Dorothy Adler did not survive long enough to write the book she clearly could have completed. What has been published is more a tribute to her scholarship by friends and family who managed to get a lightly edited copy of her dissertation into print.

Beyond the fact that Professor Adler was unable to extent her study through World War I as she intended, the tedium of dissertation style makes the volume painful reading. Few theses reach print, partly because the authors are novices and partly because they are written in a straight-jacket. The text of this volume, for example, is constantly expanded in lengthy footnotes. There are probably more words in the notes than in the text. It is difficult enough to follow in the text material covering involved financial manipulations without encountering a footnote superscript three or four times during the course of a paragraph. While footnotes are essential in scholarly writing they should be employed to support the text, not to divert from it.

A further criticism of this work is the dated nature of the study. Mrs. Adler prepared her thesis for acceptance in 1957-58. The volume was published in 1970. The twelve years and more between research and publication were years of extreme importance in economic history, especially American economic history. The author wrote without benefit of works by Robert Fogel and Albert Fishlow on American railroads and Peter Temin on the American iron industry to name a few revolutionary scholars of the period between research and publication of the Adler volume. The editor did not go far in the direction of mitigating this inadequacy. No editor could have done so without rewriting the book.

Beyond the above the volume stands heir to criticisms on presentation. The author frequently loses her readers in the maze of financial wizardry she is leading us through. Most readers of this book will not be Americans with an excellent knowledge of England and its financial structure. A single map of the railway systems under discussion would have served to guide one over some of the rougher roadbeds.

The contents of the book, while representing no watershed in organizational technique, are adequately divided into three parts. Part I deals with the early years, 1834 into the 1850's, when American railways had yet to catch the London fancy and British iron served to carry Yankee payloads. Part II covers what might be called a middle period, the 1860's and 1870's, some of the more scandalous experiences of the time, and a discourse on the move of British capital into rails situated to carry iron and coal. Finally, Part III provides some insights into how the British handled their investments over the period in question (1834-1898). Part III in general and Chapter VIII « Protection of British Investment » in particular, include Dorothy Adler's contribution to knowledge on her chosen subject.

Overall the volume is worthwhile as an addition to the field of economic history. The contribution, however, was made when Mrs. Adler submitted her dissertation in 1957. Publication has only served to make her work available to those who might otherwise have no access to it. It is recommended to those avidly interested in the subject and already apprised of further developments in the area since the completion of the work.

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K. Allen and M. C. MacLennan, *Regional Problems and Policies in Italy and France*, London, Allen and Unwin, and Beverly Hills, California, Sage, 1970; 352 pp., index, $ 13.50.

Regional development within postwar Italy and France is the focus of this detailed, informative work. The book is the result of largely parallel investigations reported by two UK scholars with personal exposure to efforts designed to affect economic change on the Continent. Subnational problems and policies are set in the context of historical disparities in well-being between wealthy Paris and the « desert of France », and between North Italy and the Mezzogiorno. The authors emphasize the economic dimension of this disparity and the efforts to remedy poverty, focusing on the administrative response to the problems as it came to be more acutely and accurately perceived in the postwar decades. Three policies are a qualified success. The underlying agenda of the work is advice to policy makers in the United Kingdom: how might the imminent entry into the EEC (at the time of writing) affect domestic UK efforts to achieve regional balance and equity in the level of welfare.

The first part of the volume is a review of the Italian regional problem, as it manifested itself in the postwar period, with a detailed presentation of the extensive and varied set of policies to help the nation achieve integration. A useful exploration of certain features of the historical setting is presented reaching back a century to conditions at the time of unification: the authors conclude that the legacy of structural differences from pre-unification days made the South poor relative to the North. They suggest parallels between the effects of Italian unification and current efforts at European integration: parallels which call for a cautionary stance. Allen and MacLennan trace the growing commitments made since World War II to raise the standard of living in the impoverished South — a task made in a sense more difficult as a consequence of the North's rapid rise in well-being and the generally counterproductive « backwash » effects of much of the national development thrust.

The authors describe in considerable detail the legal and institutional structure which has been developed — in particular subsidies to entrepreneurs and the activities of the Cassa per il Mezzogiorno. Together with smaller and less-focussed commitments by various branches of the central government, these constitute a set of strategies, not always supportive of each other, which on the whole seek to reduce dependence on agriculture; on the premise that an industrialized South would be a richer South. Evaluating the experience of some twenty years, the authors feel that absolute gains are a matter of record, but relative to the North, much still remains to be done. For example, unintended or undesirable consequences such as outmigration continue to be a major factor, and is is true that many areas in the South remains outside the scope or reach of programs designed to help the region. There is an interesting suggestion, however, that when Southern Italy is seen as a Mediterranean region, rather than solely compared with the North, major improvements have been registered on relative terms as well.

The second part of *Regional Problems and Policies* takes a hard look at contemporary France, another nation with a legacy of regional disparity. It is less severe than Italy's, though one of the difficulties the authors face, as does anyone attempting similar comparisons, is the lack of parallel data. In contrast to Italy's situation, where the question is one of broad regional differentials, the French, particularly in the quarter century since World War II, see the issue as a contrast between the Paris region and the rest of the nation. The book traces the series of strategies and institutional arrangements designed to promote counter metropoles to the capital. In particular the authors attach importance to the successive adaptations to the national planning efforts (rather than in building new institutions, the Italian model) to take into account the regional and spatial dimension. As with the Italian case, they feel grounds for cautious optimism that the groundwork has been laid to reduce regional disparities in well being.

Allen and MacLennan bring their book to a close with consideration of the nature of regional planning, growth area policy, and the effects of EEC membership on regional problems within member nations. Economic integration they conclude, may well add to the woes of lagging regions. While such problems in the UK are less pressing, « British regional disparities may increase [with EEC membership and]... regional policy may be weakened... » (p. 349).

Several themes run through this work. Regional disparities are real, severe, and persistent facets of the two nations' economic histories; they have broad implications for all aspects of national life. (Though the analyses reach back a century in the Italian case, no such long-run perspective is offered in the authors' presentation of the French material). These differences in well-being, modified somewhat by social security transfers and provision of public services, seem to continue even in the face of growing public intervention.

The study discusses in considerable detail a rather impressive array of public policies and measures. Some are direct, as where infrastructure investment is keyed to regional development; but in the main the effort consists of a series of incentives to the entrepreneur, such as subsidies, tax rebates, and related devices to induce activity in selected locations. Assessing these, Allen and MacLennan conclude that governments will find a growth center thrust more effective than efforts to provide aid over the spread of lagging areas. They also stress the desirability of supporting activity points capable of self-sustaining growth rather than struggling to aid impoverished areas, localities with high unemployment, or sub-regions marked by an economic structure no longer appropriate. On balance, then, they seem to feel more satisfied with the French than the Italian experience, though it must be stressed that the problems of Italy are more severe.

The specialist concerned with the problem of sub-national development will find the book, though an excellent contribution, not yet the definitive work. It is clear that this was hardly the authors' intention, for except peripherally, this work does not present or develop methods of analysis or techniques of planning for regional development (the one exception is a rather detailed discussion of the design of an industrial complex for the Puglia area). There is virtually no assessment of such work or its relevance, accuracy, or adequacy to the task at hand. Nor do the authors discuss the way or the extent to which formal or other models of development, make their way into the decision-making process.

A second question of scope should be mentioned. While the title and the chapter headings — the jacket as well — would seem to promise comprehensive analyses of regional imbalance, strategies, and growth, the first page of the text makes it quite clear that the authors restrict themselves to the *economic* sphere. This provides them, of course, with a manageable focus and one that certainly is important enough. Nevertheless, it also suggests that some critical issues of necessity be slighted. Without asking for an encyclopaedic effort, one wishes the authors gave greater attention to such factors as the educational system, transportation networks, and the nature of public administration. These may well reflect in significant ways the core problem of uneven development as well as add to it. As it is, the volume focuses only on income per capita disparities.

This reviewer, while agreeing with the authors' major conclusions, would emphasize three matters which might have benefitted from more extensive treatment. The first concerns political values; the second the international and time dimension in which the study is set; and the third raises some further questions about the administrative context.

Allen and MacLennan take seriously their role as scientific observers and analysts of regional development. Their own preferences or goals, intrude but rarely and then, usually, explicitly. However, the work does skirt the difficulties inherent in the multiple objective question. One of the core issues in regional development is the interplay of growth and equity considerations. One school of thought is that only when the proverbial pie is bigger does the possibility for its equitable distribution arise. Another approach (certainly not the orthodox one) would hold the opposite: that growth can only proceed under conditions of relatively full employment and social justice. Either strategy seems to call attention to the frequent necessity for trade-offs of some sort: the simultaneous pursuit of the goals of increased income and reduced disparity may well be in conflict. Such 'cruel choices' are not treated adequately, nor are appropriate responses to such issues developed.

Second, there are perhaps unavoidable gaps in the history of the two nations studied. Is it really possible to understand twentieth-century Italy with only passing reference to Fascism? Is France's years-long pre-occupation with Algeria irrelevant to the parallel (at least in time sequence) growing concern with the lagging South and West? One may also question whether the relation of intranational and international development is solely a matter of *economic* integration: foreign trade policies, for example, would appear to underlie much of the agricultural basis of the income disparity, and there is much more that could be said about international migration in the Italian case and its relative absence in the French. And, what about France's concern with military security: France a nation recently overrun by foreign forces, and for part of the period, at least, committed to build up military self-sufficiency. The impact on industrial location, if not on overall regional development strategy, might well deserve more attention.

Finally, Allen and MacLennan do indeed stress the gradual inclusion of the spatial dimension in the French planning process, and the hesitant links between regional development and national planning in Italy. However it is disappointing that one central characteristic of French planning, the variants of indicative planning, are given no more that passing attention. To what extent has the absence of similar participatory structures in Italy affected the nature of the growth process? Are firms given « signals » in the French case of the locational as well as scale and timing dimension of economic processes? Is indicative planning an appropriate vehicle for the construction of *poles d'equilibre?* Or would a more directive system of planning function with greater determination in structuring other metropolitan centers to balance Paris?

Having made these qualifying suggestions, it is nevertheless important to close this review with a comment on the thoroughness of the coverage of the material which Allen and MacLennan have defined as relevant. One cannot but be impressed with the solid documentation, often from primary government sources. (The American reader will, however, miss references to a number of works written on this side of the Atlantic and covering the same subject: an interesting sidelight on the still large lag which remains in the flow of academic information). The reader will also find the book a model of careful and logical organization of the material, and the writing clear if not exactly electric. The statistics in support of the arguments are appropriately presented, together with welcome cautionary assessments as needed. A number of useful maps help clarify the text. Our understanding of the economic dimension of regional growth strategies in Western Europe has been enriched by this careful work.

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J. T. FUHRMANN, *The Origins of Capitalism in Russia; Industry and Progress in the Sixteenth and Seventeenth Centuries*, Chicago, Quadrangle Books, 1972, pp. 376, $ 11.96.

This interesting and detailed account of the conditions under which 57 industrial enterprises were established during the XVIIth century in Russia is based upon previously published primary and secondary sources, and therefore does not entail anything new in terms of information for knowledgeable students of Russian history. Since no new source material is brought to bear upon the subject, the two areas in which the author could have displayed originality are: in the organization of material, or in the broad historical interpretation of the phenomenon. In the opinion of this reviewer, Professor Fuhrman did not exploit such possibilities and therefore the book falls short of being either original or contributing to our understanding of the problems of pre-modern industrial development in Russia.

The greatest virtue of the book is the assembly of information on the circumstances and the personalities involved in the setting up and managing of the enterprises. The author is careful in presenting the historical background, including the political and cultural aspects, against which the actions of the entrepreneurs, the administrative officials and the political decision-makers were taking place. He is also explicit in rendering the reaction of some groups in the Russian population to the new enterprises. Since most of the enterprises were set up by foreigners, the author focusses attention upon the constraints, whether legal, social or cultural, under which the foreigners carried on their economic activity in Russia. The author's description of the activities of the most outstanding representatives of the foreign entrepreneurs is perceptive. The Marselis family, Vinius or Akema, appear as colourful, energetic figures. Even if they were less towering personalities than a Louis de Geer, they nevertheless represented a broad group of European entrepreneurs of the XVIIth century, that contribtued to the expansion of the European commodity trade and to the transfer of technology and managerial skills from the more highly developed to the less developed countries in Europe.

The inadequacies of Professor Fuhrman's book are most vivid in the analytical aspects of his study, starting with the title of the book, namely « Origins of Capitalism in Russia ». According to the author, the XVIIth century industrial enterprises or « manufactories » in Russia represented a form of capitalist production. The author states: « Thus *capitalism* in this study refers to a society characterized by a definite relationship between technological capacity and social structure, a society that follows in time the dominance of the artisan workshop and is based upon new sources of power and methods of labor organization » (p. 4).

I doubt whether the above definition is acceptable to historians as an adequate or meaningful one, regardless of the persuasion of the historian. Historians of our generation usually insist upon a more distinct social content in a historical definition of capitalism than sheer description of « technological capacity », be it wind, water, steam, electricity or atomic energy that provide moving power for the wheels of industrial produtcion. The need for a defined social content is especially evident since we are very much kept in the dark by the author with respect to his definition of « social structure » and « methods of labor organization ». However, from the context of the narrative, one gains the impression that, whether implicitly or explicitly, the author uses as his criteria in describing the industrial activities of the « manufactories » as capitalist in nature the following characterics: 1) the use of water power as the chief source of energy; 2) the size of the employed labor force per enterprise; 3) the availability of capital and the ability to manage a stock of capital and a labor force engaged in industrial production; 4) the profit motive of the entrepreneurs.

I would argue that each one of the above characteristics of the « manufactories » taken separately are still insufficient to consider the « manufactories » as capitalistic enterprises. If the use of water power as the prevailing source of energy would be the crucial element, then we would probably expect flour mills to be among the first « capitalistic enterprises ». That neither capital nor the size of the labor force in the abstract are sufficient criteria for the presence of capitalism is clear from any comparison with mining operations in or even with ergasterias in antiquity. Since the profit motive could also be attributed to some landowners, craftsmen, etc., it was not a unique characteristic of the industrial entrepreneur. Thus Professor Fuhrman's definition of the capitalist nature of the « manufactories » hinges upon the mysterious « definite relationship » that he has left unexplained.

May I suggest that a « definite relationship » that differentiates the capitalist mode of production from other ones may include such elements as freedom to enter into, and legal enforcement of, contracts between individuals, as well as between individuals and state institutions, a reasonable degree of competition resulting from relatively free entry into various areas of economic activity and a sensitivity to market conditions that enable or force the industrial entrepreneur to change the factor proportions of resources employed in his enterprise. The presence of the above « relationships » are crucial in defining the nature of enterprises operating within a capitalist setting. Thus, my own prejudice would prevent me from considering the famous Manufacture Nationale des Gobelins in Paris as a capitalist enterprise, regardless of its capital or labor force, degree of division of labor or use of a particular source of motive power in the production process.

It is the considered opinion of this reviewer, that such an enterprise, as most of the « manufactories » in Russia, represented a transitional form between simple small-scale handicraft and the modern factory system, but it does not yet constitute capitalism, Professor Fuhrman's definition notwithstanding. It is methodologically and historically faulty reasoning to assume that the development of industrial production from the artisan or craft workshop to the modern capitalist factory system had to proceed through one particular route and that therefore one is entitled to label the particular observed transitional stage of « manufactories » as capitalist. In reality, the transition from the small-scale craft production to the capitalist factory did not necessarily have to pass through the « manufactories » stage. This was recognized by some, even quite orthodox Soviet historians who have labelled an alternavite transitional stage, the putting-out system as the « decentralized manufactory ». It is, therefore, curious that Professor Fuhrman appears to be even less flexible than some historians for whom the denial of the capitalist nature of « manufactories » means real iconoclasm.

An additional test that appears not irrelevant to the definition of the « manufactories » of the XVIIth or for that matter, those of the XVIIIth century as capitalist, it to look ahead at what their relation was to the nascent, truly capitalist enterprises of the XIXth century. How many of them made the transition to developed capitalism? The answer is unambiguous, since the historical record is available. Most of the « manufactories » disappeared, without being able to make the adjustment to the capitalist factory. If Professor Fuhrman is correct in attributing to the « manufactories » as many capitalist characteristics as he does, then we would expect the transition to new sources of energy (steam power) and the expanded division of labor to be manageable for most manufactories.

May I therefore venture to indicate, that the non-capitalist characteristics of the Russian « manufactories » turned out to be the dominant ones. That their scale of operation was supported by the supply of serf labor; that they were operating quite harmoniously within and became an integral part of a serfdom-based economy controlled by an autocratic state; that the successful acculturation of the leading industrial entrepreneurs into a land-owning and serf-owning aristocracy does not point to an antagonistic relationship that may arise when competition-oriented capitalists face monopoly-ridden markets or state dominance over the freedom of capitalist decision-making. Thus, most of the profits of Russian « manufactories » were not necessarily the result of capitalist profit maximization, but were derived from the ownership of humans, or were the result of preventing mobility of labor and curtailing the mobility of capital. In fact, it had as much to do with capitalism as « the prophet Samuel with the Archbishop of Canterbury », to use a familiar phrase.

It is indeed regrettable that, while Professor Fuhrman described in detail many instances of monopoly rights granted by the State to the entrepreneurs, he neglected to take these into account when discussing the criteria for an enterprise, to be labelled « capitalist ». Furthermore, he missed the opportunity to compute some estimates of actual capital invested by his group of entrepreneurs, or to add up the known sums of direct government subsidies given to the enterprises. This information, available in the sources and scattered in the book, is important since it might indicate the extent to which whether capital or managerial and technical skills were relatively more scarce factors in the development of various industries in Russia during the XVIIth century, and by extension, the real reason behind the willingness to « go to school to the foreigners » on the part of the Russian policy makers. Just as Professor Fuhrmann missed the opportunity to apply the data on monopolies granted to the entrepreneurs in the analysis of the markets for various industrial goods, he under-utilised some of the available documentary evidence on the « iron-manufactories » which could have provided a more incisive analysis of labor as a factor in production.

Last but not least, Professor Fuhrman uses the output of iron as an illustration of economic progress, not only for the period under consideration in the book, but for subsequent centuries. This reviewer is not convinced by « suggestive illustrations » which use a single indicator as an index of long-run economic progress, even if the data for iron production were correct. In fact, some of them are outrageously erroneous (see p. 263).

To summarize this very cursory review: Professor Fuhrman's book, although not original, is nevertheless informative. It fails in most of the attempts to provide a general historical interpretation of the phenomenon of the Russian manufactories, primarily because of the lack of a more rigorous analytical framework.

The publishers and editor of the book share the responsibility for the numerous errors and misprints that at times become a real irritant to the reader.

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J. Kaplow, *The Names of Kings. The Parisian Laboring Poor in the Eighteenth Century*, New York, Basic Books, 1972, pp. xvi, 222.

The names of kings (the phrase comes from Brecht) have dominated historical writing; Jeffry Kaplow wants to redress the balance. To that end, he adopts a classic form of urban history: the portrait of a place, a time, and a people, a portrait divided into panels representing conditions of life, attitudes and beliefs, major subdivisions of the population, and salient features of their behaviour. Kaplow's sketch of the place itself is hasty and unclear; the book contains, for example, only one map of Paris, a map far too cramped and blurred to serve as a guide to the many place names in the text. The time is uncertain; everything takes place before the Revolution, but trends and changes are neither specified nor attached to dates. Yet the portrait of the people is loving, colourful and often persuasive. There the book breaks with its classic predecessors: the people in question are no longer the élite, but the ordinary folk who have only their own labor to assure survival.

By the « labouring poor », Kaplow means essentially people who had to work for their living, and had little or no control over the means of production. He includes artisans (but not master artisans), general labourers, domestics, street merchants and, with some hedging, beggars and criminals. The task of the book is to describe these groups, their origins and their living conditions with an eye to accounting for the extraordinary role of the Parisian populace during the early Revolution. Kaplow disarms his readers by describing the book as a « series of essays » written « to solicit the interest and aid of other scholars ». That it is, and that it will do.

The book is handsomely printed, modestly priced, and decked out with a score of informative illustrations. To be sure, some egregious slips occurred in its production: for example, the dust jacket identifies the site of one of Kaplow's earlier books as Lebeuf; page 93 mentions an Hôpital de la Saint Esprit, and page 142 features the year's most creative typographical error: « Some men, like Louis Fremont, ' thief, pimp, sodomist ', who would ordinarily have been imprisoned at Bicêtre, were exiled 'à la suite d'un régiment' ». In compensation, Kaplow gives us abundant excerpts from contemporary observers, many concrete descriptions of individuals, and a string of thoughtful commentaries on poverty, crime and political activity.

At first, one is amazed to see urban history of this variety emerging from France. The French have, after all, pioneered in the use of collective biograhy as the unifying technique for the study of a locale, in the balancing of *structure* and *conjoncture*, in the synthesis of demographic, geographic, economic and sociological analyses of communities. *The Names of Kings* does none of these things. Puzzlement, then a flash of recognition: Louis Chevalier and Richard Cobb also write French urban history. Kaplow's book is very much in the mode that Chevalier and Cobb now advocate: eschewing comparison over space and time, mistrusting theories, concepts and techniques, striving to evoke a particular place, time and population. The result is an interesting, provocative book, but not one that is easy to use.

The greatest drawback is the absence of sustained comparison — with some general model, with other cities, with Paris itself at other points in time. A firm outside comparison would have kept Kaplow from calling worried attention to such common demographic conditions as the city's low rate of natural increase; of the high ratio of male to female death rates. (It might also have kept him from rejecting, on p. 82, Deparcieux' comparisons of Paris with the provinces on the mistaken ground that Deparcieux' figures refer to life span but not life expectancy). A firm comparison, with other cities or with the nineteenth-century estimates assembled by Jean-Paul Aron, would have given us some means of evaluating Kaplow's rejection (pp. 71-72) of Robert Philippe's figures for Parisian calorific intake. It is a pity, almost a scandal, that Kaplow does not anchor his interesting but fragmentary observations on crime to the vast surveys of eighteenth-century Parisian criminality carried out by Porphyre Petrovitch and his collaborators.

The rejection of systematic comparison leads, paradoxically but inevitably, to the reckless employment of implicit models and generalizations. Kaplow asks us to believe, for example, that beggars commonly turned to crime, that crime was a muffled form of protest, that poverty and rootlessness generated criminal activity. Of course the eighteenth-century authorities often believed these generalizations, as do authorities today. The historian's responsibility, however, is to make such arguments explicit, scrutinize them, and lay out the evidence pro and con. By that standard, *The Names of Kings* is a handsome failure.

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H. S. K. Kent, *War and Trade in Northern Seas*, « Anglo-Scandinavian economic relations in the mid-eighteenth century », Cambridge, University Press, 1973, pp. 240.

The author, Reader in ec. hist. in the University of Adelaide and a long-time student of northern commerce, clarifies the patterns of trade in the North through the period of the Seven Years War, with England as the focal point. He illustrates the amazing complex of regulations and irregularities and the overall « ineffectiveness of their mercantilist practices » (p. 93). Smuggling was rife and evasions and bribery were invited by the multiplicity of controls, prohibitions and contradictory rules. Who knows for what purpose the Swedes took to including with shipments of bar-iron to England « a few barrels of French brandy which, needless to say, did not find their way into the statistics of imports » (78)? The reader is amply warned that he need have no illusions about the reliability of port records that often failed to list imports or exaggerated exports.

Two solid chapters deal with the timber trade and the iron trade. Norway dominated the English timber market, particularly that for the navy, and England was uncomfortably dependent on Scandinavian (and later Russian) sources, not only for timber but for pitch and tar. Sweden dominated the market in iron, especially for the top quality that had no rival for naval and special industrial uses. In return for these basic products the English sent coal, tobacco, grindstones, spices, knives, cloth, woollen yarn (though its export was prohibited), and a multitude of manufactured items sometimes consigned to individuals, such as two nightcaps and a flat-iron for Claus Reimers in Norway, and a pair of spectacles and 16 straw hats for Fru Thommesen. Export of herring, reported the Danish envoy, was of minor consequence except as a means of « civilizing the Scots through giving them something useful to do » (94). Yes, Kent had fun in the archives!

One of the most fascinating chapters is a detective story about tea. The thirst of the English produced prosperity extraordinary for the Danish East Asian Company and the Swedish East India Company. Whereas the duty on tea in England rose to 100% ad valorem it was but 1% to 2½% in Gothenburg and Copenhagen. Both independently and as illicit colleagues of the English East India Company (which had a legal monopoly) the Swedes and the Danes brought to their ports vast quantities of tea from the Far East, then re-exported directly to England or indirectly via ports on the continent. In value this trade equalled the total of that in iron and timber and all other items. Acts of Parliament could not stop it until at last Pitt got the duties lowered to such an extent that it killed the smuggling trade — and made the Swedish East India Company, and the Danish East Asian Company legitimate.

The Seven Years War brought shifts in commerce, especially from French to English suppliers for the Scandinavian market. In shipping the Northerners gained an immense boost, and for a time even replaced the English in the Mediterranean. But they tried hard and failed to establish the principle of Free Ships, Free Goods. The Danes rested their claims partly on their most-favoured-nation treaty with England and by reference to an Anglo-Dutch treaty of 1674 allowing the Dutch to carry enemy goods. The English were amusingly inconsistent in their responses but always negative. The Danes, despite their bitterness, could not afford to declare war, and the English continued to hope for Danish cooperation. Sweden had no comparable treaty and no case, but persisted in demanding neutral rights. As the war proceeded toward English victory their navy took more and more prizes, their syndicates of privateers preyed mercilessly on Northern shipping, and their government played-off Sweden and Denmark-Norway against one another. The Frenchinspired attempt of the northern neutrals to operate a Neutrality Fleet foundered on the rocks of intra-Scandinavian jealousies driven by the deliberately shifting winds of English policy.

The book breeds scepticism of the wisdom of the English Cabinet which, prior to the outbreak of war, concentrated on the military aspects of international relations and failed to consider the economic significance of the states that provided their timber and their iron and, potentially, vital shipping in war-time. It was due to the merchants in all the countries concerned « rather than their governments that trade in the Northern Seas continued uninterruptedly in war as in peace » (177).

The five useful appendices include one that portrays the attempts of the Scandinavians to lure skilled workers from England, and the British attempts to stop the drain of skills. The sources so effectively used in the study comprise the significant memoir and secondary literature, articles in a wide variety of journals and languages, and archival materials from Danish and Swedish archives state, provincial and local, with exhaustive use of thirteen categories of papers in the Public Record Office. It is a superb little monograph, highly informative, skilfully written.

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B. Lewis Solow, *The Land Question and the Irish Economy, 1870-1903*, Cambridge, Harvard University Press, 1971, pp. 247.

Agriculture dominated the Irish economy in the middle of the nineteenth century. All but a very small percentage of farmers were tenants operating small, inefficient holdings. Several decades later, at the outset of World War I, agriculture was still the major economic activity and source of employment in Ireland. The number and average size of holdings remained essentially unchanged during this period. However, the majority of farmers had been transformed from tenants into owner-occupiers of the land they cultivated and grazed. Dr. Barbara Lewis Solow traces the effect of the successive land tenure enactments upon agricultural production in *The Land Question and the Irish Economy, 1870-1903.*

It is Solow's contention that sufficient data exists for a quantitative assessment of the impact of changes in land-tenure structure upon the patterns and levels of agricultural output. An annual agricultural census of Ireland was undertaken by the British from the late 1840s. It can be demonstrated that this data is reasonably reliable and is consistent through the years. The published census volumes provide data broken down according to the level of political units, by types of crops and livestock in terms of acreage and physical output, and by size categories of farms for the years with which Solow's study is concerned. Time-series data in Irish agricultural prices can be fitted and adapted to the data on physical output to construct aggregate output values. Statistical data of a less reliable, more fragmentary nature is available for selected years over this period on rural violence, which reflects peasant unrest, and on tenant evictions, which suggests the degree of tenant security. The author argues that the traditional approaches to Irish history during this period have generally neglected the available data. She finds most of these interpretations to be highly opinionated and, in her terms, mythological. Solow admits the data has defects and should be used cautiously.

Professor Solow does a careful, workmanlike job of assembling and analyzing the quantitative material on Irish agricultural production but, ironically, confines hereself to dealing with only a few years. She has constructed figures for gross value added in agricultural production for 1876, 1881 and 1886. Solow asserts that Irish agricultural production achieved impressive gains in the few years immediately following the famine but that decline set in, beginning with the land legislation of the 1870s. She states flatly that over the 1870-1903 period « the physical output of Irish agriculture did not rise, and no fundamental changes in Irish agriculture took place ». This leads her to conclude that the reinforcement of tenant security and, eventually, the conversion of tenants to proprietors did little, if anything, to stimulate agricultural production.

Landlords were reluctant to invest in their estates under the precarious legal circumstances of the period and the very real threat of physical violence, according to Solow. In her interpretation, successive enactments, granting tenants greater security and more stable rents and, facilitating the transfer of land title itself from landlords to tenants, reinforced landlord apprehension and a disinclination to invest. The tenants, in this version of events, were unmotivated and incapable of providing sufficient investment to significantly alter the processes and patterns of production. For example, Solow traces the rundown in the stock of agricultural machinery over the years and attributes it to the reluctance of landlords to invest.

Interpretations which could reasonably be developed from data on annual output do not confirm Solow's version. Output value, in deflated money terms, show a modest rise over the years in question. The selection of only a few years upon which to base her interpretation, unfortunately invites distortion, due to fluctuations which only long time-series could dampen. In fact, there is a substantial shift in agricultural recources from crops to livestock production, with a notable rise in the stock of sheep and cattle over the years. If holdings are examined by size category, it is evident that small and large farmers, tenants and owners alike respond to market forces with sensitivity. Solow convincingly argues that peasants were really interested in securing rent reductions and boosting incomes and not merely in gaining security over the land. The analysis of production data by farm size would seem to confirm Solow's point about the income maximizing motivation of Irish farmers. But it also suggests that land reform legislation did little, if anything, to obstruct agricultural progress.

The shift in the allocation of Irish agricultural resources from crops to husbandry was primarily instigated by increasingly favorable relative prices for meat and dairy products over grain in world markets. Since Britain was the almost exclusive destination of Ireland's agricultural surplus, the Irish were sensitive to these changes in commodity prices and acted accordingly. This explains the contraction of the stock of agricultural machinery which Solow attributes to the reluctance of farmers to invest. It is impossible to say whether Irish farmers would have formulated a superior response to these international commodity market forces, within the constraints placed upon them, in the absence of land reform.

Solow convincingly argues that the British measures introduced to alter Irish land tenure were piecemeal and uncoordinated. She asserts that these enactments were no more than expediencies intended to placate a discontented peasantry, in competition with Irish nationalists. The nationalists, as Solow relates, linked 'land reform' and 'Home Rule' in an attempt to win tenant sympathies for the goal of political independence. In explaining the various land laws from the 1870s onwards, Solow provides a useful summary of the detailed accounts of two excellent earlier studies: E. R. Hooker, *Readjustments of Agricultural Tenure in Ireland* (Chapel Hill, University of North Carolina Press, 1938) and J. E. Pomfret, *The Struggle for Land in Ireland* (Princeton, Princeton University Press, 1930).

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C. M. Lovett, *Carlo Cattaneo and the Politics of the Risorgimento 1820-1860,* Martinus Nijhoff, The Hague, 1972.

In this brief and detailed study Prof. Lovett attempts « an overall view of Cattaneo as a political thinker and, above all, an evaluation of his role in the politics of the *Risorgimento* ». The author places her book within the Cattaneo literature which has emerged following the two World Wars, a literature of which one part has probed the question of what went wrong with Italian democracy, hoping to find, in the « return » of Carlo Cattaneo, an insight and perhaps an answer to the rise of Italian Fascism. But this part of the the literature is addressed, unfortunately, only in the introduction, the author confining the bulk of her work to the relevance of Cattaneo's thought and activities on the course of the *Risorgimento*. Of special concern here is the examination of that perspective in the Cattaneo literature portraying him as an engaged and influential participant in the events of Italian unification.

To perform this task Lovett digs out, assembles, and interprets historical data covering a period of more than forty years. The data derive from five sets of orginal archival sources, and some well-known, but limited, secondary sources.

The picture of Cattaneo which emerges from the analysis is adumbrated in two chapter titles: « The Reluctant Revolutionary » and « An Ambiguous Legacy ». Regarding Cattaneo's *political* thought, and despite the fairness with which the data are presented, no systematic core can be shown to exist in his expressed political ideas, nor do they manifest a sense of progressive continuity over the years. Further, it is difficult, if not impossible, to identify what is characteristic in Cattaneo's political ideas, either in the way established ideas were reorganized by him, or in the creation of original ones. Rather, the political ideas associated with Cattaneo seem to derive from the intellectual and revolutionary ambience of the times. Moreover, Lovett argues that Cattaneo's advocacy of ideas which one can term « political » was fluctuating, ambivalent, and contradictory. Enough detail is presented to support Lovett's counter-thesis that Cattaneo's political thought constitutes a slender legacy. But this part of the study would have been strengthened by a broader, more penetrating analysis of the larger European ambience shaping Cattaneo's ideas.

With respect to Cattaneo's active involvement in the crucial events of the *Risorgimento*, Professor Lovett contends that a serious distortion has been produced by historians who have stressed, almost exclusively, Cattaneo's role in the 1848 Milan revolution, and his presence in Naples after Garibaldi's occupation in 1860. Lovett's documentation suggests strongly that Cattaneo was a reluctant, vascillating, and unreliable political activist, not only in the revolutionary episodes of the *Risorgimento*, but also as an elected parliamentary spokesman for the republican opposition to Cavour, following the initial 1860 unification under Piedmont. To those for whom Cattaneo is a revolutionary hero — rather than the liberal reformer Lovett argues for — it may be demoralizing to learn that in his « revolutionary » 1860 visit to Garibaldi in Naples, a substantial part of his time was taken representing the interests of his Tuscan banker retainers who were trying to win concessions for railroad construction. As Lovett states, much more needs to be done before Cattaneo's role in the *Risorgimento* is clarified.

But if, as interpreted by present historical research, Cattaneo's impact on and commitment to the evolving *Risorgimento* was ambiguous, what do we have left? Lovett's book seriously undermines only that portion of Cattaneo historiography which has stressed his role as an engaged and consistent revolutionary. However, historians will be heard from even on this modest claim. What would appear to be left is the Cattaneo of the *Annali Universali*, *Il Politecnico*, and the brilliant reports, some astonishingly modern, on special problems of economic growth and change. His most consistent and permanent interests were to publicize, plan, and plead for the modernization of economic life. These interests naturally led to the roles for which Cattaneo is most known, namely, journalist, adviser, and sometimes-representative for the implementation of specific programs of investment and development. This is not to say that Cattaneo was simply a technician-journalist-lobbyist with little political passion. His work shows clearly an acute awareness that economic questions are imbedded in a political context, and this awareness often translated itself — albeit ambivalently — into the politics of his beloved Lombardy.

We are left, perhaps, with a « cleaner and leaner » Cattaneo, stripped of that constant revolutionary heroism which has attached itself to his biography. Lovett's study is not extensive enough or strong enough to be considered a « summing-up » of this remarkable man, but should she be close to such an ultimate judgment, we are left with the Cattaneo outlined in Greenfield's brilliant study. Cattaneo's principal roles may be more appropriately a part of economic history than of political thought and action.

And here the study could have been more helpful. The author had data in hand to distinguish more forcefully these two sets of roles, but the Cattaneo of economic change is sacrificed to the « correction » of the revolutionary thesis. Despite the brevity of Lovett's study, her thesis could have been developed in one-half the present pages, allowing space for what could have been a useful interpretative realignment, a balancing off, of Cattaneo's place in economic and political history.

The weakest part of the study is the contrast between Cattaneo and Mazzini. Of the many (some unimportant) differences discussed, Lovett fails to bring to the surface a master difference which explains the tension between Mazzinians on the one side and Cavourians and liberals on the other. Mazzini spoke to and for the unarticulated interests of the working classes, not in the paternalistic liberal sense of Cattaneo and Cavour, but in the sense that workers were the base and the object of the social revolution he preferred to the political revolution. Mazzini's view of unification was regarded as an explicit threat to the future structure of power as envisaged by liberals. As events soon showed, even common membership in the Italian National Society failed to resolve these tensions, acting merely to suspend them temporarily.

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M. M. Postan, *The Medieval Economy and Society. An Economic History of Britain, 1100-1150*, University of California Press, Berkeley and Los Angeles, 1972.

M.M. Postan's latest publication may be greeted with some mild — though distinctly pleasant — surprise. First, it is a book, and although his career has certainly been a remarkably productive one, his readers have by now become accustomed to the fact of his revealed proference for the shorter scholarly essay over the full-length book, as printed medium. Second, though he has of course published other books before this, they qualify as rather untypical products from the pen of one whose established and commanding professional expertise is in the field of medieval European economic history. He authored one volume and co-authored a second in the British ' History of the Second World War: Civil Series ' (1952 and 1964 respectively), studies which certainly do represent a most legitimate and relevant application of the abilities of the experienced economic historian and do constitute, in a very real sense, economic history. His next full length book again dealt with a period similarly far removed from that in which he had so firmly rooted his reputation. This was his 1967 publication of *An Economic History of Western Europe, 1945-1954.* (Would it be going too far to speculate that his work on the war industries histories, with their concern for such matters as investment and technological innovation, contributed directly to the appearance of his study of post-War European economic performance?) This most recent effort, then, constitutes Postan's first book-length treatment of medieval economic history.

Much more in his style, over the past four and one-half decades, has been the succession of scholarly journal articles, many of a truly seminal nature. From one of the very first, « Credit in Medieval Trade » (1929, in the *Economic History Review*), to one of the most recent, « Investment in Medieval Agriculture » (in the December, 1967, issue of the *Journal of Economic History*), they have set the standards and become classics in the literature: the authoritative reference. (A useful bibliography of his writings, to 1965, is included in that year's August issue of the *Economic History Review*, being a Festschrift from a few of his colleagues and students, on the occasion of his retirement from the Cambridge Professorship).

The most common, though by no means exclusive vehicle for these contributions over the years, has been the *Economic History Review*, with which he has been so closely associated. Though this journal alone accounts for almost a third of them, not a few of his published essays have by now become difficult of access. This is all the more reason to applaud the very real service rendered by the Cambridge University Press in its decision to collect this scattered bounty and assemble it in three volumes. The first of these, a mixture of the philosophical and the semipopular, appeared in

1971, under a title derived from one of the included pieces: *Fact and Relevance. Essays on Historical Method.* The others were announced in 1973: *Essays in Medieval Agriculture and General Problems of the Medieval Economy*; and *Medieval Trade and Finance.* Members of the profession warmly welcome the appearance of such convenient collections.

Between these numerous articles and the rarer books fall a number of items difficult to categorize. Such are, *inter alia*, Postan's important introduction to the *Carte Nativorum* which be co-edited with C. N. L. Brooke for the Northamptonshire Record Society (volume XX, 1960); his study of « The Famulus » in the ' Economic History Supplements ' series (number 2, 1954); his Neale Lecture, published separately as *The English Gentry in the Thirteenth Century*, in 1972; and his contributions to the *Cambridge Economic History of Europe* (volume I, 2nd edition, and volume II). Mention could also be made of his various reviews in the *Economic History Review*'s ' Essays in Bibliography and Criticism ' series. This impressively prolific record is all the more remarkable when regarded in the context of the very large variety of editorial duties which Postan has so consistently discharged — for the *Cambridge Economic History of Europe*, the ' Cambridge Studies in Economic History ', the World War II series noted above, as well as for the *Economic History Review*, not to mention individual volumes. It is not exaggerating to say that the man himself has become something of an institution.

The separate components of Postan's vast and varied bibliography form a surprisingly coherent whole. Indeed, so strong is the central theme which runs through so many of them that a recent — and to date the most extended — assault upon it has dubbed this argument « The Postan Thesis » (H. E. Hallam, in the April, 1972, issue of *Historical Studies*). This critique challenges Postan on a number of specific points, and thus raises some questions about the fundamental interpretation. Though it may be noted in passing that the discussion in the second chapter of the volume here under review may meet some of the objections raised by such critics as Hallam, this is not really the place to attempt an assessment of the merits in that debate — perhaps a review of the first of the 1973 volumes of collected essays mentioned above will afford someone the appropriate opportunity for doing so. Yet some account of the basic argument must be offered here, for it recurs in the present volume once again — which is hardly to be wondered at, given the subject of the book. Readers familiar with Postan's earlier work will not fail to perceive the book's derivation therefrom. They will detect, for example, his Royal Historical Society paper on « The Chronology of Labour Services » in the present volume's discussion of the conditions of the villagers, his « Rise of a Money Economy » in the chapter dealing with trade and industry, his contribution to volume II of the *Cambridge Economic History of Europe* behind Chapter 12, and his section in the revised edition of volume I of the same series throughout the book. To observe that he has drawn heavily from and built upon these and other of his previously published articles is not to gainsay the value of this new book, but rather simply to reaffirm that so much of Postan's work is of a whole.

In its most concise characterization, the Postan interpretation is 'neo-Malthusian', a mode of analysis which seems lately to have been undergoing something of a renaissance among economic historians. Briefly stated, Postan's view is that demographic trends, through their impact upon relative factor prices (rents, wages, etc.), broadly determine the course of economic change — or its absence — in the relevant period. More particularly, secular population growth leads to colonization movements. This brings about the working of increasingly marginal quality lands, resulting in declining yield rates. Also, the fall in average size of land unit brings tendencies toward the deterioration of the integration of livestock husbandry with arable farming, monoculture in cropping and, in the manner of Gresham's Law, the poorer grains replacing the better. Ultimately there occurs a genuine Malthusian crisis that fundamentally alters the price ratios of the factors of production. This scenario forms the basic construct here, as in so much of Postan's work.

The book is organized topically — rather than as a chronologically continuous account, as the sub-title might mislead some to expect — and the distribution of space as between 'rural' and 'nonrural' topics is a faithful reflection of their relative importance in the stability of medieval society. Yet the subtitle, possessing the defect noted, is nevertheless an indispensible corrective to the main title itself which, standing alone, would be seriously misleading, promising a treatment considerably broader than the restrictions introduced in the sub-title. Indeed, these restrictions themselves are not altogether accurate, either — and happily so; for, though it is explicitly limited to Britain, portion of the text do possess a larger applicability to Europe beyond England. Chapter 5, on the origin of the manor, is not the sole instance that could be offered of the very considerable relevance and value which Postan's comments possess for those primarily interested in medieval society on the Continent. His chronological limits are also — and again to our gain — violated, at least in one direction; the first chapter, by way of example, present a brief survey of the backdrop to the Conquest in 1066, and a moderately revisionist posture in its emphasis upon continuity from Celtic through Roman to Anglo-Saxon and finally Norman Britain.

The expectations generated by title choice aside, this book is a sheer delight to read. It is an honest representative of the author's achievements as a writer — the anticipated charm and elegance of style, masterful syntax and idiom, the trenchant and concise composition, and closely reasoned and tightly argued analysis are all present. The lapses from this general rule only serve to point up the overall high quality of the volume, and may thus be indicated with derogating it.

Thus we find Postan noting, in Chapter 3, that estimates of aggregate population in the Middle Ages lack precision, continuity and adequate frequency of incidence for many purposes. He then argues that this is a relatively unimportant problem, for it is trends rather than levels that are of significance, and the former at least are clear. They can, he avers, be reconstructed from a study of their effects, e.g., colonization movements. Yet it is troubling to have such manifestations employed as evidence for establishing the rate and direction of demographic changes, and then find the population trends themselves used to account for fluctuations in those same activities. That introduces certain problems of circularity. Again, Chapter 6 may frustrate those with a low tolerance for ambiguity, for here Postan appears to be saying that the same outcomes can issue from circumstances of both improving and degenerating economic conditions, for the solution of the landlord's perennial dilemma: to hold his land in demesne or to farm it out. By way of a final point of this nature, one can turn to pages 153-154 in Chapter 9, where Postan asserts that our knowledge indicating that its greatest intensity occurred in areas where freeholding was most widespread, warrants the conclusion that the great peasant uprising in 1381 was in fact not occasioned by attempts of landlords to reestablish or enforce in full their claims to rights of compulsory services. Many social observers, starting at least from de Tocqueville, have argued precisely the opposite: according to this persuasion what is to be expected under the posited circumstances, in the presence of such unwelcome tendencies on the part of the lords, is that resistance would be strongest exactly from the locales of greatest relative advancement. (See the treatment of the latest authority on the subject, Rodney H. Hilton's forthcoming *Bond Men Made Free. Medieval Peasant Movements and the English Rising of 1381*).

Each reader will react in his own manner to the many and varied features of the book, but all will profit from it. As we have come to take almost for granted in Postan's work, this volume is rich in insights, and provocative in its arguments. On the former quality, the succinct, dense yet eminently readable seventh chapter on « The Village » can stand as epitome. For the latter, reference can be made to his comments in Chapter 11 and their implications for our interpretation of that much debated topic, the economic history of the Renaissance period. Possibly the most fitting testimony to the quality of the volume, and tribute to its author, is that the most glaring deficiency is probably the absence of a concluding chapter, or some other kind of summary statement. This book will not disappoint those who have come to look for nothing less that the best from M. M. Postan, and his audience will continue to eagerly await his next efforts.

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N. J. G. Pounds, *An Historical Geography of Europe, 450 B.C. - A.D. 1330*, University Press, Cambridge, 1973, pp. XIV-475, mps.

The term « historical geography » has always been poorly defined; if there is any consensus among scholars working in the field, it is to interpret historical geography as a reconstruction of the spatial distribution of political, economic and social phenomena at selected dates in the past. Professor Pounds adopted this definition, and selected five periods in European history for his book. These five are the mid-fifth century B.C., the age of the Antonines, the age of Charlemagne, the year 1100, and the early fourteenth century. No one could quarrel with the author over his choices, yet this reviewer wishes that, in addition to his stressing the importance of the availability of sources of information, he had given the reader a little more insight into the cultural values characterizing these periods. True, twelfth-century Europe was, in the author's words, « in a condition of rapid change and growth », but the importance of the twelfth century to Europe was more strikingly — and more accurately — described by Kenneth Clark when he spoke of it as having « an extraordinary outpouring of energy, an intensification of existence... ».

In a very real sense, this entire work is permeated by evidence of hard work, of highly competent scholarship, and of a nearly complete lack of inspiration. One cannot find fault with the enormous amount of work distilled in these pages, the shifting of evidence, the exercise of a finely-honed critical sense, yet after reading an entire chapter, one is struck by one question: why was this particular period of time more important than others?

Each chapter is organized into major segments, starting with a survey of existing political organization into states, and includes estimates of population based on available sources, distribution of linguistic-ethnic groups, forms of urban and rural settlement, agriculture as characterized by crop combinations and farming techniques, mining and manufacturing, trade and transport, and a brief conclusion. The author omits from his discussion Britain and Russia, concentrating on those parts of Europe located between these two. One may well accept the exclusion of Russia, even though this reviewer prefers to base such a decision on the separate ways Russia followed in its historic, artistic, and economic growth, ways that differ in fundamental respects from « western » Europe, rather than the mechanistic statement, « its inclusion would almost have doubled the geographical area covered by the book ». It is a good deal more difficult to agree with the author that an « adequate discussion of Britain would have made the book a good deal longer... ». Is there so much more information available for Britain than for the rest of Europe, or is Britain worthy of a much more detailed discussion-description than the adjacent continent?

On one important point this reviewer wishes to take issue with the author: his discussion of urbanization, in the 12th century A.D. It appears that, in order to ' balance ' his treatment of the subject, he dispenses with the Mediterranean city in something like a page and a half, whereas cities in northwest Europe deserve eight pages. It would be ridiculous to deny the significance of Flanders, that region described by the contemporary chronicler as « quasi urbs continua », but the cavalier fashion in which the Italian cities are presented does leave something to be desired.

Professor Pounds' book is a work of considerable importance, and it is surprising that so many points of detail seem to have been passed over during the making of it. Some of these concern matters of procedure: in a set of bibliographical references that is imposing not only by sheer volume, but by the judicious and far-ranging choice of sources, it would have been helpful to translate titles in less widely read languages, e.g. Polish, for the benefit of the uninitiated.

More distressing is the lack of attention to accuracy. In some instances it is incorrect location of places, e.g. the Roman frontier, town of Brigetio was on the *right* bank of the Danube, near the present Szőny, not on the *left* bank, near Komarno. In some cases, the maps present inaccurate spellings, e.g. Ianiculm (sic), map 3.7; in others map and text do not marry, e.g. the early capital of Bulgaria is spelled Pliska in the text, Plisca on the map placed directly above the textual passage. And in a few instances the author fails to check his own statements: thus on p. 174 he describes the Avars as an « Ural-Altaic people », while on p. 189 he refers to the same group as « an eastern people of Turkic or Finno-Ugric linguistic affinities », compounding the confusion of the reader trying to deal with a tribe of Uralic-Altaic origin.

Professor Pounds' book is a monograph, it marshalls information that is not easily obtained. But this book is presented to the American public at the price usually associated with « coffee-table » books, retailing at $ 24.50, that will keep it away from those vitally interested in this scholarly work, and restrict it to a few select libraries. One cannot help wondering at the rationale behind the decision to make a scholarly monograph, devoid of any illustrative material except sketch maps, inaccessible to its public.

George Kisch
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G. Probszt, *Osterreichische Munz- und Geldgeschichte von den Anfangen bis 1918*, Wien-Koln-Graz, Hermann Bolhaus Nachf., 1973, pp. 684, ill. 422.

The author is a leading authority on Austrian numismatics and in this book we find the results of a life's research and collecting of historic Austrian money and coins. The book also summarizes his innumerable publications

— in the general bibliography they number 69 and include such masterly contributions as his « *Quellenkunde der Munz- und Geldgeschichte der ehemaligen Osterreichisch-ungarischen Monarchie* » — and provides a lucid guide to the theoretical and practical problems of this complex subject.

The book is composed of three sections. The first deals with the basics of the history of coins and money, providing an introduction to the subject and explaining the relationship of the history of money to neighbouring sciences, the first of which is economic history. It also describes the historiography of numismatics and the territorial evolution of the Hapsburg empire. In the second section the author gives evidence for the origin of the metal of the coins, which until the 14th century were silver. The third section deals with the different mints in which coins for Austrian sovereigns were struck. The geographical area covers practically all central Europe, that is all the territories once ruled by the Hapsburgs or their predecessors, including the archbishopric of Salzburg. Thus all the mints and coins not only of present day Austria but also of Bohemia, Moravia, Silesia, Hungary, Belgium and Northern Italy are described. Particular attention is given to the mints of Venetia for the year 1793-1866, which in addition to imperial coins also produced those of the « *Alleanza dei popoli liberi* » (1848) and those of Milan (1707-1859), with the coins of the «*Governo provvisorio di Lombardia*» in 1848. Probszt examines, too, the private mints such as that of the archbishop of Olmutz. So the reader receives a very accurate idea of state affairs and crises, and of political and economic events which were reflected in these coins. Chronologically the book covers more than two thousand years, beginning with the coins of the Celts and ending with the breakdown of the Austro-Hungarian monarchy in 1918.

The book contains a high standard of scholarship, a remarkable grasp of both detail and perspective, a clear style and a rare capacity for reducing a multitude of facts to order and discipline. The illustrations have been carefully selected and grouped to represent concisely and vividly the development of the coins.

A bibliography of about 1300 titles, among which there are many relating to economic history, as well as guides to different problems and three indices facilitate the use of this extremely important book.

After this review had been written the author of the book died, at the age of 86. His death is a very great loss for the study of economic history.

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E. P. Thompson & Eileen Yeo (eds.), *The Unknown Mayhew*, Merlin Press, London, 1971

' In this life — said Mr. Gradgrind — we want nothing but Facts, Sir: nothing but Facts '. Facts, facts and more facts. In the early decades of industrialization in Britain facts were to be called on increasingly to arbitrate between conflicting interpretations of the nature and behaviour of the new industrial economy. The more that the harmonious and optimistic conclusions of the political economists were ravaged by the effects of cyclical booms and depressions, the more that their theoretical explanations were contradicted by the economy's actual development, the greater the demand for facts. Hence the Blue Books, the royal commissions, the inquiries of local municipal statistical societies — when in doubt look to the facts, for the facts would tell all.

Against the background of this generally uncritical thirst for pure empirical information which would in the end, it was assumed, reveal the truly harmonious working of the capitalist economy, Henry Mayhew's studies of the labouring poor in London at once stand out as exceptional and new. For middle class contemporaries Mayhew's picture of the conditions in the trades and on the streets of London's East End in the early 1850s were too exotic and sentimental to warrant serious consideration. Rather than a serious or scientific — for which read ' statistical ' — social investigator, Mayhew was ranked with the growing body of adventurers and raconteurs who provided glimpses of that stark world of poverty, the existence of which Mid-Victorian society tried so hard to disguise. Mayhew was lumped together with a genre which was at best superficial and patronising, at worst prurient and voyeuriste. And subsequently Mayhew has not been treated with greater respect by historians. Although inevitably cited, *London Labour and the London Poor* (first published in 1851 and then again in 1861) has generally been regarded with suspicion by social historians, few of whom have been willing to go beyond Professor Dyos' judgement that ' Mayhew's work was essentially a form of higher journalism, not of social analysis '.

The intention of the editors of the present volume is to re-establish both the author's standing as a perceptive and sophisticated social investigator and the value of his studies for the social history of the metropolis in the mid-nineteenth century. The reason that the essays collected here — they include detailed surveys of the Spitalfields Silk-weavers, the Slopworkmen and Needlewomen, Tailors, Boot and Shoe Makers, Toy-makers, Merchant Seamen, Woodworkers, Dressmakers, Hatters, Tanners and Curriers — represent an ' Unknown Mayhew ' is that they are drawn from the articles which Mayhew wrote for the London 'Morning Chronicle' between October 1849 and December 1850. Although it was this series of investigations which

Mayhew undertook for the 'Morning Chronicle' that led him on to the larger venture of compiling a 'complete Cyclopaedia of those that will work, that cannot and will not work', *London Life*, which was the nearest he came to realizing this over-ambitious project, took on a rather different character from the original inquiry. Although many of the 'Morning Cronicle' articles were included, Mayhew's interest shifted from the trades to the «Street Folk» of the capital. The reason for this would seem to have been largely commercial, but as Mayhew is best known for *London Life* it also accounts for subsequent attitudes toward the author. The result, as Edward Thompson points out in his introduction, is that: « In this century, Mayhew the systematic empirical sociologist, has almost been lost sight of, and has been replaced by a Mayhew who was a gifted impressionist with an eye for 'character' ». In publishing the many of the original studies which were not subsequently included in *London Life* the editors hope to redress the balance.

In his short but valuable essay Thompson managed to reconstruct from the most fragmentary of sources Mayhew's career, illustrating the way in which the author's interest in the poor of the capital developed from a normal journalistic assignment into a real commitment, which then in turn burned itself out. The period of commitment coincided with the writing and researching of the articles published here, and led Mayhew into direct conflict with the orthodoxies of political economy of the day — resulting in his dismissal from the 'Morning Chronicle'. In revealing the appalling consequences of cheap goods and increased demand for many of the traditional trades, he found himself attacking robustly the confortable assumptions of the Free Traders. He also came into conflict with Lord Ashley's charity movement whose sanctimonious hypocrisy he castigated: 'this overweening disposition to play the part of pedagogues... to the poor, proceeds rather from a love of power than from a sincere regard for the people'. The reason that Mayhew found himself the target as well as the critic of these multiple voices of orthodoxy was due not only to the fact that he was free of what Thompson calls 'middle-class moral halitosis', but also because he was not satisfied merely to let the 'facts' speak for themselves — he attempted to use his research not simply to provide a description of poverty, but also to construct some analytical explanation of its cause. He refuted the stock-in-trade belief that poverty was the product of intemperance and lack of thrift and pointed more directly to its economic roots. Referring to the needlewomen of the East End he wrote:

'As a class, I must say the workpeople that I have seen appear remarkably truthful, patient, and generous; indeed, every day teaches me that their virtues are wholly unknown to the world. Their intemperance, their improvidence, their want of cleanliness, and their occasional want of honesty, are all that come to our ears. As I said however I doubt very much whether *we* should not be as improvident if our incomes and comforts were as precarious as theirs...'.

But the value of Mayhew's work lies not only in his humanity. It was the desire to reveal the causes of poverty which led Mayhew to examine in particular the poorer sectors of the traditional London trades. Very often these were the sectors in which the traditional craft workshop had been replaced by the 'show-shop' or 'slop-shop', which represented the advent of mass-consumer society. In place of goods made to order by individual craftsmen, the 'show-shops» were stocked with goods which had been farmed out by middlemen and manufactured on a piece-work basis. In the articles reprinted in the present volume Mayhew constantly contrasts the prosperous and the poor ends of the various trades, which usually coincided with the contrast between the 'Honourable' — or artisan — and the 'Dishonourable' — or piecework — sectors. The 'honourable' artisan tailor working in his neat shop is compared with the destitute 'slop-worker' — 'almost brutified with their incessant toil, wretched pay, miserable food, and filthy homes'. The same distinction is to be found between the 'society-men' and the 'non-society-men', that is between the workers who belonged to trade associations and unions, and those who did not. For Mayhew the distinction could be summarized 'in the language of political economy,... those whose wages are regulated by *custom*, and those whose earnings are determined by *competition*'. For Mayhew increased competition was the root of the problem. It was increased competition that reduced the value of the wares of the Spitalfields silkweavers. In the case of the shoe and boot makers, it was the increased competition from France resulting from the Free Trade tariffs, together with competition from the factories of Northampton which created widespread destitution among the traditional craftsmen. One of Mayhew's most detailed studies, of the various branches of woodworkers, from carpenters and joiners to cabinet makers and shipwrights, also provides a typical example of victims of technological progress, the sawyers. But for Mayhew the general law which he concluded from these investigations was: 'over-work makes under-pay' and 'under-pay makes over-work'.

It is due to the way in which he went about his research that Eileen Yeo in her excellent introduction seeks to establish Mayhew as a social analyst comparable at least to Booth and Rowntree. In his house to house inquiries, in his constant explanation of the methods which he adopted to arrive at calculations of wage levels, and in his attempts to classify both the subjects of his studies and their economic and social situation, Dr. Yeo argues that Mayhew emerges as a sophisticated precursor of sociological techniques of social survey. But this does not derive only from Mayhew's awareness of the methodological problems attending social investigation — he was also able « to see poverty in the round, as the product of an economic system, with devastating moral and social consequences and yet varied cultural manifestations... ». And, defending Mayhew against charges that his studies become incidental and uncoordinated, Dr. Yeo holds that the thematic unity of the investigations derives from his systematic desire « to establish the conditions of employment, especially wage-levels, in the metropolitan trades, relate these to the life styles of the poor and, at the same time, explore the industrial causes of low wages and poverty ». As a result of this central preoccupation he was able to provide a description of the stratifications of the various trades, an examination of the effects of changing economic conditions on the different state, and an understanding of the unique sub-cultures which evolved from the nature of different forms of employment — of worlds, that is, that the moral assumptions of the Victorian middle classes made generally incomprehensible — that probably cannot be surpassed. In conceptual terms however Mayhew was at a disadvantage in that he was at best an untutored economist, as Dr. Yeo points out, and despite his insights he was never able to evolve any more substantiative economic theory around his general wage law that — 'the more work there is to do, the less the workpeople will get from it'.

Although the debate will certainly continue, in the hands of such able and outstanding champions as the editors of the present volume Mayhew's reputation would seem to be safely established. But while one may follow Dr. Yeo's assertions of Mayhew's superiority over other contemporary investigators, the criteria by which she judges his work to be full of ' sociological fascination ' are not always perhaps sufficiently explicit. At the same time, to criticise Mayhew's statistics and calculations would be carping and niggling, and in any case would seem to derive from a misguided and hopeless search for some impossible 'objective' source for the social history of mid-Victorian London. Mayhew puts his credentials and his calculations on the table, and we can correct his arithmetic if we wish — but figures alone can never tell the whole truth, and Mayhew's studies provide an essential introduction to so many aspects of the organization and condition of London's poor that are simply not quantifiable. If there is sleight of hand it lies in the fact that the poor come to dominate the whole canvas, edging the more prosperous ' honourable ' craftsman and ' society ' man into the margin. But Mayhew cannot be blamed for this, for he makes no pretence that his subject is the working class as a whole, but rather the ' condition of the poor '. And if the poor do fill out the canvas, perhaps the perspective is in any case more revealing than deceiving.

After looking again at the wealth of information which these studies provide for such a range of occupations and groups, not to mention the details of social organization and custom, and even of political behaviour, which they contain, one can hardly do more than endorse Eileen Yeo's statement that « Mayhew's industrial survey offers the social and economic historian a veritable storehouse of riches ».

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E. ZALESKI, *Planning for Economic Growth in the Soviet Union, 1918-1932*, The University of North Carolina Press, Chapel Hill, 1972, pp. 425, $ 15.75.

Since American students of the Soviet economy are slow to keep abreast of the substantial French contributions to the field, we are fortunate to have one of the most important reference works on the Soviet First Five Year Plan in a recent English translation. This work is Eugene Zaleski's *Planning for Economic Growth in the Soviet Union, 1918-1932*, first published in 1962 as *Planification de la croissance et fluctuations économiques en U.R.S.S.* and now translated and edited in its English version by Marie-Christine MacAndrew and G. Warren Nutter.

The book describes the gradual evolution of Soviet planning institutions and procedures from the rationing of deficit supplies to full-blown administrative allocation and it compiles the detailed statistical record of plans and performance during the First Five Year Plan era. Zaleski writes for those who are already well versed in the political and economic history of the early Soviet years. For those who are not, this book has to be read side-by-side with other accounts of the period — E. H. Carr and R. W. Davies, Maurice Dobb, Serge Prokopovicz, Naum Jasny, Alexander Erlich, Nicholas Spulber — in order to put the frequent changes in goals and policies into an understandable context.

No other source describes as well as does Zaleski the forces behind the development of the Soviet planning institutions. In theory, economic planning was intended to serve as a means of rationally administering the nationalized sector of the economy. But, in practice, the planning mechanism became the means of diverting resources toward politically set goals in an environment of inflationary excess demand. The early reliance on *ad hoc* rationing devices such as the « allocation of deficits » can be understood more as an attempt to gain control of an economy where only the printing presses were operating at full capacity than as an attempt to develop a prototype annual balance. « Thus, suppression of the market preceded planning, (290) ». Similarly, the substitution of administrative for flexible planning techniques followed from the inconsistency among the targets of long-run plans, control figures, and operational balances for the economy.

Zaleski makes a convincing case that planners and political leaders alike did not take the goals of the plan seriously. They were well aware that the plan was unrealizable. Quarterly operational plans for economic units called for far more modest targets than those in the control figures, and even those operational plans were not alway fulfilled. Rather than presenting a blueprint for rational allocation, the First Five Year Plan was basically a « vision of growth ». Zaleski argues, « The presentation of an incoherent plan has some practical advantage in that it makes it possible to delay choosing among various goals, or at least not to divulge the real intentions of the planner, (293) ». Thus, by 1930 (after the arrest and imprisonment of many of the former members of the planning agencies), the role of the economic planners was decidedly more political than the simple provision and analysis of data. The economic planners functioned to justify and attest to the feasibility of politically set goals, to convey the impression that *ad hoc* governmental policies were scientifically grounded in a rational plan, and, where possible, to hide the full cost of government measures.

Zaleski's interpretation of the First Five Year Plan as a political rather than as a strictly economic document has some formidable support in a path-breaking and as yet unpublished working paper by Holland Hunter. Hunter uses the statistical data for 1927-28 summarized in Zaleski's book to test the feasibility of the plan. Hunter's test is to apply a linear programming model developed by Richard Eckaus and Kirit Parikh for their analysis of Indian planning to check the intertemporal and intersectoral consistency of the Soviet plan. He asks whether the First Five Year Plan could have been feasible even in the absence of unfavourable developments (such as the effects of collectivisation) that were unforeseen at the beginning of the plan. The results of his linear program are negative. Hunter writes, « In light of the circumstances under which the first Soviet Five Year Plan was issued, it is not surprising to learn that its targets, taken all together, were infeasible both algebraically and politically. With the original targets for consumption, and using all the other constraints and targets, no initial feasible solution to the linear programming problem could be found. The targets were too ambitious. Too much was called for, simultaneously, both in output increases and in transformation of the economy's structure. No allocation of resources among the six sectors and over the various plan years would enable the terminal-year levels of capital and output to be reached, along with the intended levels of household consumption and other final uses ». Final year targets could be reached, but only if the level of base year consumption were set at zero!

Hunter's use of the First Five Year Plan data points up the value of Zaleski's book as a research source. Zaleski has drawn on a voluminous literature of primary sources. The summary tables provide data from early Soviet sources that have been available previously only in Russian. Not surprisingly, these early Soviet sources are often at variance with later measures issuing from the Central Statistical Administration. Some of the most useful data presented spans the gap between the pre-revolutionary period and the early plan era, a period that is covered by relatively few studies. (Nutter's study of industrial growth also provides estimates for this period).

Another recent book, Albert Vainshtein, *Narodnyi dokhod Rossii i SSSR*, makes use of the same early sources. (Vainshtein was one of the economic planners arrested and imprisoned about 1930 who was freed only in 1956). The student of national income accounts will be interested to compare Zaleski's and Vainshtein's reconstructions of the national income accounts for 1928-32.

In sum, Zaleski's *Planning for Economic Growth in the Soviet Union* proves to be both a valuable source of rearly Soviet data and an authoritative account of the evolution of Soviet planning institutions.

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