
REVIEWS OF BOOKS

A. ABOUCHER, *Economic Evaluation of Soviet Socialism*, London, Pergamon Press, 1979.

The appearance of a monograph by Alan Abouchar on the Soviet economic system provides an opportunity to review several issues that arise in the exposition of certain methodological and theoretical propositions. The question is whether the analytical constructions and broad generalizations are sufficiently cognizant of the empirical historical features of the Soviet economy.

To begin with, in discussing the systemic feature of state ownership, the author disregards the difference between the East European and Soviet economies when he claims that in both, "... many small tradesmen own their means of production, and collective farmers cooperatively, but privately, own their equipment," (p. 2). It would be more correct to say that in the Soviet system no tradesman may own or use productive means privately. Moreover, Soviet collective farms which disallow the use of its productive means privately are more akin to state farms than to cooperatives or other types of voluntary associations of the East European or Western type.

Among the basic traits of Soviet socialism the author identifies "... a nominal commitment to equity and social justice, subject to the constraints imposed by the nation's other goals" (p. 3). It should be kept in mind, however, that socialist declarations after Marx do not stress economic equality as an objective, and the Soviets formulate the pertinent law of socialism as the objective to satisfy the growing needs of the population rather than to attain economic equality or social equity. Today, the equity objective is certainly stressed more by Western Marxists than the Soviet official ideologists.

With regard to the problem of unemployment, we are sceptical about the author's generalization that in the USSR "there is virtually no cyclical un-

employment, no secular unemployment and only a little technological unemployment" (p. 12). Such an assertion would require further elaboration. Soviet data on labour resources are among the most secret plan indicators since the difference between them and the number employed would reveal the size of unemployment. And yet, it is well known that there are still excess resources of labour in such less developed regions as West Ukraine. Moreover, in the industrialized regions like Donbas there is high unemployment among women for whom planned development of light industry is still inadequate. Aside from such secular unemployment, there is also so-called hidden unemployment. During the early 1970's, the Soviets experimented with selected enterprises to reduce the controls based on the size of employment, endeavouring to plan instead only the size of output and the wage fund. Although this raised labour productivity and released large numbers of workers, these experiments were soon abolished since the Party could not cope with released excess labour. Undoubtedly, Soviet leaders would wish the author to be right that "... the employment picture in the Soviet economy is good" (p. 13).

The complex problem of inflation would also require more careful analysis. On p. 13, the author claims that "Indexes of prices net of turnover tax have also been extremely stable since the late 1950's, although there is some evidence of rising consumer prices. Increases in nominal wages were sufficient to offset these increases, however." But already on p. 14, he acknowledges the complexity of the problem since "... difficulties always arise in the assignment of prices for new products which were not produced during the year on which the constant prices are based." Certainly, such difficulties are intrinsic to the system in which the price of, say, an automobile is regarded to be raised if decreed officially, but the price index could remain unaffected if a price rises sharply along with some minor improvement. Moreover, it should be kept in mind that prices of inputs in planned production and inter-enterprise deliveries have been rising considerably less than consumer prices, so that combined indexes are biased.

In the spirit of Western Sovietology, yet another problem in price indexing has been neglected by Abouchar. The Soviet Union boasts of price stability of food items, albeit increases did occur under Khrushchev and several times under Brezhnev. But more frequent are the deteriorations in the quality of food items and their nutritional composition. Although such disguised price increases are top secret and never publicised, they should be taken into account by scholars.

While the analysis of income distribution poses well-known difficulties in the interpretation of data, the calculated indexes apparently invite subjective interpretation. Abouchar reconsiders the Wiles-Markowski findings,¹ accord-

¹ The study in question is P. WILES and S. MARKOWSKI, 'Income Distribution under Communism', *Soviet Studies*, January 1971.

ing to which "... the Soviet Union with an interquartile ratio of 1.8 is not too different from advanced capitalist countries such as the United Kingdom (2.07) or the United States (2.64), and is more or less similar to other socialist economies (Poland 1.8, Hungary 1.75). Wiles and Markowski conclude therefrom that any claims about the elimination of social classes under Soviet socialism, and the eventual attainment of full communism and income equality, are unfounded" (p. 17). This broad conclusion seems sustained even by Soviet specialists when they recommend a reduction of differences between high- and low-paid labour, between incomes in the cities and the villages, and so on. But Abouchar disputes the above interpretation. He believes the difference of over 50 percent in the interquartile income ratio for the U.S. and the USSR is significant even though the U.S. index is based on detailed income data while that for the USSR on sparse information. The author takes a further questionable step by comparing Soviet income distribution with that of Brazil, a country "... that was at the same level of development per capita 60 years ago" (p. 18). For Brazil, the income interquartile ratio in the 1960's was over 8 times higher than for the USSR. It cannot be denied that such quantitative comparisons sharpen the argument. However, given the intrinsic difficulties in such international comparisons, we are sceptical about a sweeping assertion that "... undoubtedly, inequality remains greater in Western Europe than in the Soviet Union, considerably more unequal in the United States and enormously more so in Brazil" (p. 19).

Let us consider next Abouchar's treatment of the more specific features of the Soviet system. The section on economic growth is of considerable interest in that it offers a concise survey of Western studies of the Soviet growth record. Moreover, it reviews the methodological problems encountered in the recalculations and interpretation of biased Soviet official statistics so as to account for changes in the quality and prices of the components in the aggregated indexes.

The chapter on efficiency of agriculture begins with an intriguing assertion: "Although Soviet agriculture has generally been viewed by Western observer as being very inefficient, it is not possible to define any single summary index of inefficiency" (p. 70). However, for Soviet consumers such an index takes on a very real form of periodic shortages.

Abouchar distinguishes several kinds of inefficiency in agriculture: (1) micro technical incompetence; (2) incentive structure inefficiency; (3) macro intra-sectoral inefficiency; and (4) macro intersectoral inefficiency. Specifically, he elaborates that "... by technical incompetence we mean bungling or lack of elementary skills or knowledge by individuals in daily operations" (p. 73). Furthermore, "... intersectoral inefficiency implies a distribution of resources between industry and agriculture which is inefficient in terms of the preferences of the planners themselves. That is to say, even taking as given the planners' preference for industrial development, the possibility arises that too little in-

vestment is directed into agriculture which is inefficient in terms of the preferences of the planners themselves." This kind of analysis makes sense in theory. However, in the historical context such interpretation would give credence to the Soviet officials who blame economic shortcomings on such subjective factors as poor planning leadership of certain ministries, lax industrial workers, and unmotivated peasants on the collective farms. One of the authors of the present review who worked until recently in a major planning institute can testify that the planners make valiant efforts to improve the condition of the economy, particularly of the agricultural sector, but this task is being frustrated by lack of motivation and avoidance of responsibilities throughout the economy. With nationalized means of production placed under centralized party controls, the Soviet system is still incapable of finding a working substitute for the mechanism of market competition and economic self interest. The measures introduced by the 1965 management reform were intended to raise the level of economic responsibility and stimulation through material rewards, but the results have been disappointing. While Abouchar's book discusses the technical difficulties found in the Soviet economy, it does not pry into systemic weaknesses that are intrinsic to this particular model of socialism.

In our view the crux of systemic inefficiency could be illustrated quite well in the example of the agricultural sector where a vestige of private enterprise still survives. Here, the peasants cultivating about 1.5% of arable land after required chores in the collectives manage to produce one third of total agricultural output; this spectacular result is attained with primitive hand tools since farmers may not use privately either the tractor or the horse. Such unparalleled efficiency of human effort disproves the claim about the incompetence of peasants.

In his evaluation of Soviet industry, Abouchar performs a valuable service by surveying typical methodological issues, but his generalizations regarding Soviet performance and empirical evidence cannot be accepted uncritically. With regard to industrial efficiency, he claims that "Inefficiency in industry... is not as dramatic or as extensive as many writers believe. Many behavioural expectations of managers may either be unfounded, or else may after all prove not to have the consequences anticipated. Our own study of the one important industry, hydraulic cement production, in which there have been large advances since World War II in productivity and extensive experimentation with an adoption of new technology, demonstrates this thesis" (p. 131). However, the conclusion in this and other instances may be challenged. The author cites such shortcomings as the falsification of reports and distortion of output mix as part of his "behavioural analysis of industrial managers" (p. 86). But these considerations are discussed superficially, and it is not always possible to distinguish the causes and the effects in the discussed phenomena. Another weakness of the approach is that a less knowledgeable reader might not easily discern to what extent the conclusions based on a specific industry may be

regarded as representative of the economy. To wit, the cement industry is among the fastest growing sectors which, however, suffers from poor quality of various assortments as shown by the author. The broader issue is whether and why such pathological behaviour is a feature of the Soviet system.

It is commendable that Abouchar's monograph draws attention to questions of spatial economics in a geographically vast imperial structure of the USSR. The chapter on spatial efficiency draws on earlier contributions and methods, but does not alert the reader to significant research published in recent years. Although Abouchar updates H. Hunter's classic study of Soviet transportation, his high praise of Soviet transportation efficiency is hardly justified in view of the extensive criticism of that sector offered by Soviet planners and Party leadership. Abouchar's evaluation of Soviet industrial location policies upholds the thesis that the spatial distribution of investment reflects a preponderance of political-military considerations over economic efficiency. This thesis, however, is supported largely by Koropeckyj's evaluation of Soviet interwar investment but does not direct the reader to recent research by J. Gillula, V. Holubnychy, Z.L. Melnyk, H.J. Wagener, and others.²

Who is to blame for inefficient industrial location decisions when non-economic factors were paramount? We question the contention that "... industry observers, specialists in operations, journalists, and technicians in research institutes, were too timid to call into question the highest level decisions of the industry, i.e., the decisions about where to place new production capacity" (p. 68). Such perception of decision-making in the Soviet economy is reminiscent of the naive socialist utopias about collective decision-making under socialism. Certainly, in the Soviet context, the blame for the decisions should not fall on technicians or economic experts but on autocratic top politicians.

On the whole, Abouchar's book represents an effort to interpret the Soviet economy as a system and goes beyond a mere description of its institutions and historical evidence. To the extent that the book is concerned with the Soviet economy as a system and a peculiar "ism," some of the author's conclusions may be challenged in a manner suggested by this review. However, economic historians should take special note of this scholarly publication because it is concerned with two inseparable aspects: the meaning of analytical tools and the interpretation of historical evidence.

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² Updated extensive surveys and bibliography on Soviet regional economics may be found in V.N. BANDERA and Z.L. MELNYK, eds., *The Soviet Economy in regional Perspective* (Praeger, 1973); I.S. KOROPECKYJ, ed., *The Ukraine Within the USSR: An Economic Balance Sheet* (Praeger, 1977); and U.S. Congress, Joint Economic Committee, *Soviet Economy in a Time of Change* (Washington, 1970).

P. BUSHKOVITCH, *The Merchants of Moscow, 1580-1650*. Cambridge University Press, Cambridge, 1980. pp. xii, 212.

R.E. JOHNSON, *Peasant and Proletarian. The Working Class of Moscow in the Late Nineteenth Century*, Rutgers University Press, New Brunswick, 1979. pp. xii, 224.

The question of whether Russia's modern economic development was unique or followed somewhat of a general European pattern has remained at the centre of historical inquiry since the Populist-Marxist debates in the 1880's. Unfortunately, these discussions have given rise to much terminological and conceptual confusion. It is the merit of these two books that they make more appropriate comparisons with culture areas like Eastern Europe in the seventeenth century and the Third World in the twentieth century which resemble more closely the structure and historical experiences of Russian society. The results in both cases are illuminating and strongly revisionist. The two books also share a solid grounding in fresh archival sources and considerable care and skill in evaluating the evidence. They are, in brief, first rate contributions to the small but growing monographic literature on Russian social and economic history that has progressed beyond old ideological pre-conceptions of one extreme or the other.

The main importance of Bushkovitch's work is its revision of the widely accepted view that the state was the major obstacle to the economic growth of Russia in the pre-Petrine period. The author demonstrates convincingly that the Muscovite merchantry played a vigorous role in expanding foreign and internal trade throughout the period and he also suggests that it was able to extract economic advantages from the state particularly with respect to tax farming. While it is true that the state imposed heavy service obligations on the merchantry, the wealthiest and most influential merchants secured a great deal of influence over financial policy by the very fact of their performing service. At the same time, Bushkovitch reveals the limited extent to which foreign merchants were successful in dominating Russia's foreign trade. After the Dutch replaced the English around 1600 as Russia's major trading partner at Archangel, they failed to maintain let alone expand the privileges granted by Ivan the Terrible to the Muscovy Company. On the basis of carefully compiled statistics, the author concludes that there was "a clear trend toward protection of Russian merchants from foreign competitors first within the country and then also at Archangel." (p. 41). The later part of the book examines the specific regional peculiarities and common features of each major trading area — Archangel, the Baltic, Poland, Persia and the internal market. It is surprising to learn that the overall pattern of prices in Russia in the first half of the seventeenth century was the same as for the rest of Europe, and that foreign trade was not monopolized by the few wealthy merchants (*gosti*) but conducted by the provincial merchantry as well. Actually,

these revelations foreshadow the situation in post-Petrine Russia when foreign trade gradually fell in to the hands of the merchants on the periphery as the expense of those in the centre. It becomes clearer than ever from Bushkovitch's account that Peter's subsequent decision to shift the bulk of foreign trade from Archangel to St. Petersburg was a serious blow to the merchantry of the centre and turned them more and more to the internal market. Bushkovitch concludes that because the Russian export trade depended on leather and tallow, rather than grain and timber, commercial capital was constantly pumped back into the towns. Consequently, Russia escaped the general economic decline which ravaged the urban life of Eastern Europe in the second half of the seventeenth century when the grain ports upon which Polish trade relied began to stagnate and decline. Moreover, unlike Poland and Prussia, serfdom in Russia permitted peasants to engage in handicrafts and trade. This stimulated an internal market and broadened the commercial base. Thus, when compared to societies in Eastern Europe with a similar social structure Russia's economy emerges as relatively strong and prosperous. Although Bushkovitch does not go further than this, his conclusions help explain why Peter the Great was able to build a powerful military establishment in a society which was so "backward" by West European standards. It may be, then in evaluating Peter's achievement, too much emphasis has been placed on coercion and not enough on the existence of a vigorous commercial and manufacturing life which had its roots in the early seventeenth century.

Johnson's monograph attacks a different, but no less crucial, problem in Russia's economic development, namely the social characteristics of the factory labour force in the late nineteenth century. The author challenges the two prevailing theses that the Russian worker was either a hereditary, class-conscious proletarian or a raw peasant recruit dumped in an alien urban environment. Rather he was a bit of both, attached to the factory and the village, living in a complex dual world that defies stereotypes. Beginning with an incisive summary of the Moscow industrial region, Johnson emphasizes the traditional structure of the predominant textile industry with its high labour to capital ratio, dispersal of factories in the rural areas, low level of technology, integration of handicrafts and large enterprises and cautious investment policies. Turning to the pattern of migration from the countryside as the formative experience for the labour force, he shows that the Russian experience differed from that of Western Europe but resembles that of the contemporary third world. The removal of the peasant was rarely permanent. There was a lively movement back and forth between town and country. The migrants were not fresh from the ploughlands but came, in the case of Moscow, from nearby factory towns where they already had acquired some work experience and a different outlook. Most of them retained allotments in their native village although they did not cultivate them but left them in the hands of relatives to whom they sent regular payments until that time, normally after age forty,

when they had accumulated enough capital of their own to return to their land. The most original sections of Johnson's account uncover the thick network of informal regional ties among worker-peasants from the same provinces or even the same counties. Patterns of hiring, arrangement of living quarters, labour agitation, even cultural organizations like the famous Piatnitskii choir all bore the strong imprint of these group loyalties. At the same time, the employers' attempts to control the workers environment from housing to mutual aid societies also stifled individual life. All this does not add up to the picture of the displaced, isolated, alienated peasant. On the other hand Johnson's careful analysis of labour unrest reveals that factories in the countryside were more active than in Moscow, "backward" industries like brick-making more than "advanced" ones like metallurgy. Once again Johnson finds explanations in the network of communications of the worker-peasant sub-culture rather than in the traditional view of proletarian solidarity. He concludes, however, that the very intensity of their sub-culture guaranteed their insularity from the rest of society and prevented them from forming broader, more cohesive protest movements. The demands for workers' control of factories and the redistribution of land to peasants reflected their decentralized organization. The implications of his findings for an understanding of the revolution of 1905 and even of 1917 are far reaching and exciting. Both these excellent books then are far reaching and exciting. Both these excellent books then are classic examples of what a monograph ought to be: thorough, penetrating and original analyses of well-defined problem which illuminate the central issues of an entire historical era.

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N. GASH, *Aristocracy and People, 1815-1865*, Harvard University Press, 1979, pp. 375.

For nearly thirty years, Norman Gash has been a major figure in early Victorian historiography. His *Politics in the Age of Peel* (1953), his Ford lectures, *Reaction and Reconstruction, 1832-1850* (1965), and his very impressive two volume life of Sir Robert Peel (1961 and 1972) are basic reading for any serious student of early Victorian politics. The volume under review, *Aristocracy and People, 1815-1865*, written for "The New History of England," series, therefore, is of considerable interest because it represents Professor Gash's considered synthesis of the early and middle Victorian periods.

Professor Gash has organized his volume in a simple and straight-forward way. The first two chapters, "Country and People," and "Government and Religion," which set the scene in 1815, are followed by seven chronological chapters, which take the narrative down to 1865. Two final chapters, the one discussing foreign policy and war over the whole period, and the other

summing up the meaning of it all, conclude the volume. The simplicity of the organization, however, belies the deftness with which Professor Gash has structured his complex story. He has subordinated the social and economic to the political by moving what was the central fact of Victorian life, the industrial revolution, off-stage. He can thereby deal with its impact rather than with the complicated thing itself. This in turn not only allows him to treat the industrial revolution as essentially a law and order phenomenon, but it also enables him to lead from what is his great strength — political history.

Having thus settled the difficult problem of structuring his material, Professor Gash is enabled to develop those themes that give his structure its coherence. He argues that though the wealth provided by the industrial revolution was necessary to the making of a politically viable Victorian society, it was not sufficient. What actually made the difference in keeping the English people on the political straight and narrow between 1815 and 1850, was the remarkable performance of the English aristocracy. Through their responsible exercise of power, and their efforts to conserve the essentials of the constitution in Church and State, the aristocracy not only insured political stability in a most difficult time, economically and socially, but prolonged their own political power as well — hence the title of the volume and its chronology.

Both Professor Gash's approach and the format of this volume, however, raise serious questions about the possibility of achieving a satisfactory synthesis of this period. Can an effective synthesis of so complex a period be rendered in primarily political terms? The latter-day work, for example, of the two historians to whom Professor Gash has dedicated this volume, W.L. Burn and G. Kitson Clark, certainly pointed in a different direction. They thought that the difficult task of putting the Victorian Humpty Dumpty back together again would have to be done in essentially socio-economic and cultural terms, and that the method, which would have to be employed by all the King's horses and all the King's men, would therefore have to be interdisciplinary. By subordinating the socio-economic and cultural to the political, Professor Gash may be said perhaps to have sacrificed too much in terms of wholeness to coherence.

In any case, the constraints imposed by "The New History of England" series' format, of which Professor Gash is one of the two general editors, has made a hard task even more difficult. Is the period 1815-1865 an appropriate one for synthesis? If it has a political integrity, does it also have a social and economic integrity? Indeed, all those who have previously tried their hand at Victorian synthesis either have confined themselves extended their chronology to achieve the necessary wholeness. The format of the series, moreover, by confining the presentation to England primarily, virtually ignores the Celtic fringe and eliminates the Empire. Whatever may be said about other periods, the Victorian was surely an expansive age, and its vitality was perhaps as much a function of its concept of dominion in a Greater Britain

and Empire as it was of the idea of progress. It may indeed be unfair to fault Professor Gash for not including dimensions he deliberately eschewed in his preface, but the question remains — how satisfactory can a synthesis of Victorian society be that labours under such a handicap?

Given the givens, however, this is a lucid, temperate, well-written account of the making of Victorian England from the political point of view, and Professor Gash has provided us besides with a great deal of food for thought about what is at stake in the science and art of creating a hystorical synthesis.

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P. JAMES, *Population Malthus: His Life and Times*, London, Henley, and Boston, Routledge & Kegan Paul, 1979, 524 pp.

W. PETERSEN, *Malthus*, Cambridge, Mass., Harvard University Press, 1979, 302 pp.

Recent writers have depicted in horrifying detail the quasi-apocalyptic consequences of the population increase that nineteenth-century economists predicted, and have conscientiously traced this concern to the father of modern demographic theory, T.R. Malthus. Yet despite the hold the Malthusian theory of population has clearly secured in the Western imagination, there have been surprisingly few studies of Malthus' life, or any systematic effort to understand his ideas in the context of the events, and the controversies, of his own time, or of ours. There has not even been up-to-now a full-scale biography of Malthus, no less a serious and thorough-going study of his economic system. When we compare the libraries that have grown up about the lives and thought of some of Malthus' contemporaries, most prominently Marx and Mill, the clergyman-economist, so highly praised and so enormously influential, has been grossly neglected.

In the past year, two books have appeared which begin to remedy this long-time deficiency: Patricia James's much-awaited biography, and William Petersen's briefer effort to survey Malthus' life and times, and more especially his demographic theories and the opposition to which they have given rise. Both works are welcome. But pioneer works run the risk of proving not entirely satisfying.

Miss James is primarily a biographer not a historian or an economist. She has collected the facts of Malthus' life, and presented them intelligently, artfully, and persuasively. We can now see Malthus, deforming harelip and all, in more evocative detail than we possessed in Keynes's brief biographical essay: as the son of a father who was *not* (contrary to the *D.N.B.*) Rousseau's literary executor, as Cambridge mathematics wrangler and clergyman of the established

church, as traveler in Scandinavia and professor of political economy at the East India's College at Haileybury, as Whig politician, and as husband and father of three, not the eleven daughters he has been credited with in the past. Miss James is to be commended for having searched out the scattered and fragmentary materials upon which she has based her study of Malthus' life and times. So much of what James, has gathered and given us, however, concerns people and places not especially central to her subject and to this she has moreover added long quotations from Malthus' surviving letters, many of them of slight importance. These are small grounds for complaint, for in her work, we now have a reliable and well-written, though unnecessarily long, account of Malthus' life to replace the error-laden eulogies compiled by two or three of his contemporaries.

Where the author somewhat lets us down is in her grasp of economics and the history of economic thought. While she agrees with Keynes that the triumph of Ricardo over Malthus "constrained the subject for a full hundred years in an artificial groove," she does not tell us why. She gives us some sense of the *Essay on Population* and its reception, for example, but not of the critical *Principles of Political Economy*, without which the *Essay* cannot be fully understood. She has given us Malthus the man but not Malthus the economist.

William Petersen's comparatively brief biographical sections are also strewn with periphera, to the injury of essentials. But Petersen is an emeritus professor of Social Demography not a biographer, and understandably he has depicted Malthus as "primarily a demographer," though this is a moot point. Certainly Malthus' ideas on population occupy the longest and the most useful portion of the study. Petersen focusses on such questions as the poor law, migration, population growth, mortality, and fertility. Where he has chosen to discuss these, both in Malthus' time and our own, from the point-of-view of his special expertise, all is well. These sections alone justify the book, and a reader would have wished him to have devoted more space to this enterprise.

Unfortunately, Petersen has not made himself sufficiently well-informed about the nineteenth-century background of his subject; he would have profited from an earlier and more careful reading of Miss James's book, which he apparently saw in proof as his own was going to press. He discusses the controversies involving emigration, for example, without noting how important Malthus' theories, and his system of economics, were to its chief British proponents, Wilmot Horton and E.G. Wakefield. Moreover, while observing, correctly, that the *Essay on Population* has gone virtually unread, Petersen discusses only the best-known sections of the work. Though he takes up in some detail the socialist (and more particularly, Marx's) polemic against Parson Malthus, he fails to note Malthus' crushing dismissal of socialism in the *Essay*. Falling into the trap of an over-great identification with his subject, he describes the criticisms of Malthus' ideas by his contemporaries, only to dismiss them abruptly and uncritically. Although, unlike James who

rather neglects the genre, he lists and uses many of the recent economic articles on Malthus' system, he is not sufficiently at home with economic theory to employ them effectively.

Petersen tells us, quite properly, that "Malthus's was not a simple mind." Yet by concentrating almost exclusively on the principle of population, he has presented his subject in an overly simple fashion. Much more is required. Malthus was not merely a demographer; he was above all an economist, and his economic system, though somewhat overlooked by his countrymen, was as well-regarded by continental economists of his time as it has been by Keynes and his followers in ours. If we hope properly to understand Malthus' demographic ideas, they must be seen in the context of his economic system as a whole.

Despite these cavils, serious though they are, both these volumes have made a valuable contribution to Malthus scholarship, and economists and economic historians can profit from reading them. It would be unfair to expect definitive treatments when so little has previously been done, but we are now much closer to such an effort. Those who attempt a definitive study in the future, however, will have to make themselves more familiar with economic thought and history if they hope to be successful.

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S. MEACHAM, *A Life Apart, The English Working Class 1890-1914*. Cambridge, Mass.: Harvard University Press, 1977.

Standish Meacham, Professor of Modern English History at the University of Texas, here turns away from the biographical form of his earlier books, and from their concentration on religious, intellectual, and political concerns at the top of English life in the nineteenth century.

To capture life at the bottom, Meacham has used the large number of surveys of working class life carried out in the 1890's and on up to 1914, the work of well-known figures such as Charles Booth and Seebohm Rowntree, together with the findings of about fifty other lesser-known investigators. Most adventurously Meacham has gone to "recent sociological surveys and commentaries" (268) — Elizabeth Bott, Madeline Kerr, Peter Willmott and Michael Young — in the belief that these accounts of workers' lives in the 1950's and 1960's provide glimpses and interpretations of a way of life that had not changed in some significant ways since the turn of the century.

The prose is clear and clean. The notes stand in comfortably large type,

though maddeningly grouped at the back, which is now the cost-conscious but not the most cost-effective procedure, considering the annoyance that results. The intent of the book is praiseworthy: W.L. Burn and Walter Houghton figure in the first paragraph of the preface, where Meacham subscribes to the refusal of each to leave "his readers bewildered by trees as he searches out the shape of the forest. Complexity is not permitted to deny pattern."

Yet the book is not exciting either by virtue of its content or by the impact of its analysis. My guess is that Meacham wrote it too carefully. He stuck too closely to his note-cards. Paragraph after paragraph begins with a topic sentence that is then elaborated by further details; one stack of cards done for, the next is shoved into place. This cautious mode of presentation deadens the impact of Meacham's innovative decision to use the interviews of working-class men and women born before 1900 conducted within the last decade or so by Paul Thompson and Thea Vigne. Tailoring that material into well-ordered snippets destroys the narrative vitality so arresting in John Burnett's *Annals of Labour, Autobiographies of British Working-Class People 1820-1920* (1974). A few places (pp. 115, 116) suggest that Meacham restrained himself stylistically, an impression strengthened by the verve evident in the essay "Late Victorian London and Its People" that he contributed to a collaborative work, with Roy Flukinger and Larry Schaaf, on *Paul Martin, Victorian Photographer* (1977).

Analytically, as well, the book lacks the effectiveness that it should have had. One instance: Meacham's material on the origin and fate of neighbourhoods cries out for treatment in terms of David Ward's searching questions in "Victorian Cities: How Modern?" (*Journal of Historical Geography*, 1975), and "The Victorian Slum: An Enduring Myth?" (*Annals of the Association of American Geographers*, 1976).

Another instance: on the matter of working-class habits of thought Meacham subscribes too readily to an all-forgiving cognitive cultural relativism: "When they ['Middle-class sympathizers'] charged the working class with an inability to think abstractly, they were often lamenting its unwillingness to think in abstractions akin to their own." (196) Precisely: as we think, so we are. (See Michael Cole and Sylvia Scribner, *Culture and Thought. A Psychological Introduction* [1974]).

Had the working class been able to manipulate abstractions with the freedom and daring that marked middle-class thinking, it would have ceased to be the working class. Meacham set out to trace "what I perceive to be the patterns of working-class consciousness." "To penetrate the 'mind' of any particular collection of men and women at any particular period in history demands an ability to perceive evidence thoughtfully and with some measure of imagination." (p. 7, both quotations). The mind depends upon how life is lived and itself orders how that life is carried on. Meacham has laid a good

foundation through his reconnoitering of socio-economic circumstances, but he has not really shown the mind at work. To do that, he would have to remove the quotation marks with which he guardedly walled off the term.

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R. MOUSNIER, *The Institutions of France under the Absolute Monarchy, 1598-1789: Society and the State*. University of Chicago Press, Chicago, 1979, XVIII + 783 p.

This massive work provides us with a virtual mine of information and interpretation of Old Régime social institutions and social relations. It is impossible within the scope of a short review to convey an accurate impression of the wealth of material contained here. After an introductory chapter on how social relations were viewed by important XVIIth and XVIIIth century social theorists, Mousnier presents the main body of his work as ways of looking at society through relations systems: fealties, lineages, orders, "corporations" (formal associations), and communities. Included is a brilliant hundred-page chapter on the Catholic clergy and short chapters on Protestants and Jews. Two final chapters headed "The State" contain essays on kingship and "the state's resources," that is, its territory, population, education, and finances. Most of the book seems to have been written expressly for this volume and not merely lifted from the score of Mousnier's illustrious monographic and textbook contributions to the literature of the Old Régime; an exception is a long preface in which Mousnier gives us his conception of what institutional history is and how it should be studied. This is taken almost word for word from his *La Plume, la faucille et le marteau* and it includes Mousnier's key concept of the role of "idées directrices" in institutions. Brian Pearce, who also translated Mousnier's *Peasant Uprisings*, has done another fine job here.

While this book would be considered mainly as a handbook of Old Régime social institutions and relations — "how things worked" at a specific point in time — there also are some dynamic features included. The first chapter, for example, shows how over the course of these two centuries France's leading theorists changed from advocating a society of hierarchies to arguing in favour of a class society structured by only "wealth, talent, and mode of life." Such theory, Mousnier believes, parallels the reality in which "the formation of the future class society (can be seen) inside the society of orders." The king is shown as strengthening his public power by increasingly dealing directly with individuals rather than associations. The chapter on the clergy contains a discussion of "the shift from Christocentrism to anthropocentrism" by the XVIIIth century. And the chapter on lineages concludes with

observations on how the family was changing from being an element in a lineage to a *ménage* (conjugal family).

This is not to say that Roland Mousnier has provided the readers of this *Journal* with all they need to know about the Old Régime. The book's main fault, from our point of view, is that economic institutions are touched on very little, and then mainly to show how they affected social institutions. The few pages on joint stock companies, for example, are here to demonstrate that such associations were "moral persons," that the king was "in effect, (their) seigneur." Fiscal officials are discussed, but in the context of their status not their functions. There is little here on commerce as such, or on industry, credit, or even agriculture. From this point of view, Pierre Goubert's much shorter manual, *The Ancien Régime*, is a more satisfactory introduction for economic historians; and Robert Mandrou's Nouvelle Clío series volume, *La France aux XVIIe et XVIIIe siècles*, is even better, since it gives us not only a good picture of economic institutions but also some economic history.

Scattered throughout Mousnier's book, however, are bushels of nuggets for economic historians: salaries of officials and incomes of noble families and of each branch of the clergy; the panoply of crafts in the commune of Beauvais; the taxable income of a peasant in the area of Hurepoix; how long it took to travel by various conveyances from one part of the country to another, and countless others.

The main purpose of Mousnier's final two chapters is to underline his overall interpretation: that the state had "extensive political powers but only feeble resources" — feeble, that is, compared to the daunting task of keeping France's complex society from shattering on the rocks of its antagonistic interests. At the end of his massive chapter on the nobility, for example — a hundred-page tour de force that can be considered the heart of this book — Mousnier states that "the different degrees, the different estates of the French nobility in constant struggle, the one with another." But much of what we read in this very book could be interpreted as cohesiveness not divisiveness. It is true that Mousnier gives us a marvelously detailed picture of what set off one element of the nobility from another. We learn about the constant quarrels over armorial bearings, the nicer aspects of "point d'honneur" (duelling), and that princesses of the blood were allowed two carriages when travelling but other peeresses only one. Doubtless such "degrees" were causes of much resentment and ambitious striving; but while some of us might interpret such bickering as superficial indications of the nobility's high valuation of what its topmost ranks had to offer, Mousnier sees in it explanations of the need for absolutism.

It is concerning his perception of the relations between kingship and society that some of us will want to quibble with Professor Mousnier. Granted that the refusal by the "real" nobility to accept into its ranks even the richest and most accomplished "ennobled persons" rankled in the breasts of the

latter. But this may have gratified the upper crust about as much as it displeased the rest, thus offsetting antagonisms by commitments.

In fact Mousnier's book is not organized so as to show us on what he bases his politico-social analysis. While chapters 15 and 16 are labeled "The State and the State's Resources," they take up only 100 out of the 744 pages of text, and no attempt is made even to sketch out the major features of political machinery and power. The subtitle, therefore, is misleading insofar as it can be read as promising equal treatment of the state and society. This seems so palpable that we cannot help wondering what Professor Mousnier intends to do about this in volume two. (While the French edition [1974] is clearly labeled "volume 1," no such implied promise is to be found in the English translation.)

A reader must finish this volume dissatisfied with the conclusion that "As compared with the big industrial states of the XIXth and XXth centuries, the French state of the XVIIth and XVIIIth centuries was the victim of a cruel inadequacy of resources." Of course; but many of us would say that a state which could produce the palace of Versailles and all that went with it, conduct one frightfully expensive war of expansion after another, and subsidize a galaxy of creative talents was not "brought to the limit of its resources" except in the sense that any state is, in any era and everywhere.

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J.C. RILEY, *International Government Finance and the Amsterdam Capital Market, 1740-1815*. Cambridge: Cambridge University Press, 1980, pp. 365 + ix, bibl., index.

What do capital markets do? An economist's answer would be that they deal primarily with a "factor of production." By insuring that savers invest loanable funds at the highest possible rate of return (given the riskiness of the asset), capital markets play crucial roles in the competitive model of neo-classical economics and in dynamic models of economic growth.

Historians know better. Capital markets have existed in the Western world for many centuries, but in the past they have done little in the way of resource allocation, and have had only marginal effect on the process of capital accumulation. Instead, capital markets were traditionally an instrument which funnelled resources, not from savers to investors, but from savers to dissavers. The greatest dissavers of all times have been governments, and the European nations between 1740 and 1815 excelled in squandering away the meagre tax revenues produced by their economies and ever requiring more.

James C. Riley's monograph on the Amsterdam capital market tells the

tale of a fascinating institution which emerged in the eighteenth century. Half a dozen or so budding nation states found themselves embroiled in increasingly costly wars, after 1740, facing deficits worthy of their sixteenth century ancestors. But in the eighteenth century European governments found a new solution to their credit problems. The Dutch Republic had accumulated large assets during its "golden age," and needed outlets to invest its accumulating profits. With their trade and industry in decline, the Dutch capitalists supplied vast loans which financed the insatiable demands of royal extravagance and belligerence.

The Amsterdam capital market resembled the economist's concept in name only. Information, the indispensable lubricant of capital markets, was often impossible to come by. Lenders generally had no clue as to the creditworthiness of their customers, and the bankers and brokers had no incentive to provide such information and at times withheld it deliberately to maximize their commissions. Riley points out that the investors paid little heed to the liquidity of the assets used as collateral for the loans extended. Paradoxically, when information was available, it was counterproductive: the better accounting procedures adopted by the British government were detrimental to its ability to borrow. The less the Dutch capitalists knew about their customers the better. By 1793, the Dutch had lent between 500 and 650 million florins to foreign governments and at least as much to their own. In the next twenty years much of this wealth was lost through a series of suspensions, repudiations, and unilateral interest reductions. And yet, after 1815 the Dutch continued to lend to foreigners rather than invest in their own economy. The great question is why.

It is on this issue that Riley throws all caution to the wind and produces an answer in the cliometric spirit of "the-market-knows-all:" Dutch industry was not invested in because the rate of return on foreign and government loans was higher. A plausible and logical answer, but is it true? The evidence for Riley's assertion is a series of "realizable" rates of return for the period 1795-1814 on (Dutch) government loans. The rate of return computed by Riley on these assets was about 7.7 percent which he argues exceeded the (purely hypothetical) rate of return that could have been earned in manufacturing. Unfortunately, these data cannot bear the heavy burden of Riley's hypothesis. First, the series of rates of return on Dutch government bonds he produces (table 9-2) is obtained by simply dividing the coupon rate by the market price, without taking into account the capital losses resulting from price declines. Secondly, the "realizable" rates computed are nominal rather than real. Elsewhere Riley discusses the effects of inflation on creditors holding nominal assets, but he does not make the connection in this context. Thirdly, the decline in market prices reflected the increasing riskiness of lending to the ineffective and unstable governments which ruled the Netherlands in the Napoleonic era. The nation was suffering from severe economic ma-

laise and an alliance with a predatory Empire, and inevitably this reflected on its credit rating.

It is not likely that the answer is that simple. Capital markets in Victorian and Edwardian Britain seem to have suffered from defects similar to the ones Riley points to in Holland. Some historians, especially William P. Kennedy, have maintained that these market failures were responsible for a misallocation of resources which contributed to the economic decline of the late Victorian economy. In our own time, U.S. financial institutions seem astonishingly naïve in lending vast amounts to such dubious risks as the governments of Zaire and Brazil, while displaying great caution in lending to American entrepreneurs. There appears to be a fundamental malfunctioning in the operation of capital markets in some "mature" economies which biases them in favour of lending to foreigners and governments. Riley fails to recognize the historical parallels, and thereby has missed an opportunity to provide an answer to one of the most mystifying questions in economic history.

Other interpretations of Dutch economic history too, may be questioned. Riley asserts for instance that the Dutch could only have industrialized under a regime of protective tariffs. He also claims the existence of a "social imperative favoring saving" which is an intriguing view unfortunately not further substantiated. The propensity to save is variously estimated by Riley to be at 25 per cent or 10 per cent (pp. 27 and 239). As far as the borrowing nations are concerned, Riley assesses that the Dutch loans probably had little economic effect. There seems to have been no "crowding-out effect" on private investment, and no major balance of payment effects, although evidence for the absence of those effects is unavailable. At the most, Riley submits, the Dutch loans had a slight expansionary effect on the borrowing nations, and allowed their governments to conduct somewhat more expensive wars.

In spite of such shortcomings in his analysis, Riley has written a path-breaking and fascinating book. His meticulous research and profound knowledge of Dutch capitalism and European public finance assure that this book will be indispensable reading for anyone interested in the fate of the Dutch in the postgolden age era or in the nitty-gritty of eighteenth century public finances. The hundred pages of small print footnotes alone provide ample evidence of the depth of Riley's curiosity and scholarship.

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J. VANIŠ (ed.), *Liber rationum regalis civitatis Lunae ad annos 1450-1472 et 1490-1491 pertinens* (The Book of Accounts of the Royal Town of Louny for years 1450-1472 and 1490-1491). Prague, Academia 1979, 968 pp.

In the past decade the medieval European town has come to the forefront of historical research. This undoubtedly reflects their exceptional importance

as centres of handicraft production and trade and of the significance of urban economic and political conditions in the changes taking place in late medieval society. The rapid change of class structure in urban society strongly affected the growing changes in medieval society in general. As social institutions towns were indeed an alien element within the feudal system of production and their multilateral expansion had a disruptive effect on feudalism and slowly but surely furthered the development of the rising social class of early bourgeoisie townspeople, who acted as the advocates of progressive productional and social tendencies.

The character of late medieval society is revealed very clearly through the study of registers of municipal accounting or, more accurately, the financial registers which record much essential information which other contemporary sources are unable to provide. They contain data on incomes and expenditure, on systems of salaries and payments, on monetary rates, taxes, the general price policies in a given period, and the financial economy of towns in both the narrower and the broader senses of the word. Such information makes possible the comparative study of development in other similarly structured towns. When suitable methods of analysis are applied, the published data enable us to arrive at a general understanding of the forms of economic evolution not only in the town and region in question, but of other towns as well, and to identify the leading sectors of this evolution and the causes of the overall social and class stratification of towns, which are important for establishing the economic and political profile of specific types of town.

Although increasing attention is being devoted to municipal financial records in other countries, Czech municipal records of this type have been unduly neglected. But a detailed catalogue had already been published in 1963, thus adequately establishing a classification of the importance of certain municipal records for analytic study.¹ This, in itself, does not, of course, mean that the importance of these financial records had been underestimated by Czechoslovak medievalists. On the contrary; but the full evaluation of these important sources was hindered rather by lack of demand for published versions which would make them widely available for scientific research. Without these external stimuli it is indeed impossible to bring to a successful conclusion the exacting and time-consuming labour preparing the sources for publication.

Thanks to Bedřich Mendl, one the first pioneers of the study of the economy of medieval towns in the Czech Lands, and who was greatly influenced by the example of the French economic school of historians, the "*Knihy počtů města Brna*" (Books of accounts of the city of Brno), which showed the full scope of the possibilities for using this abundant source of information in the

¹ R. Nový, *Soupis městských knih českých od roku 1310 do roku 1526*; (Catalogue of Czech municipal books from the year 1310 to the year 1526), Acta Universitatis Carolinae, Philosophica et Historica 4, Praha 1963.

analysis of the economic and social conditions in towns of the XIVth century, in particularly with regard to the origins of the social crisis of this period,² were published. Nevertheless, Mendl's initiative found no response in the pre-war climate and it did not become a stimulus for a sustained editorial endeavour. In fact, not even the post-war period of developing Marxist historiography impressed the need for continuous reference to this kind of source material to a sufficient extent. It is indeed a paradox that Mendl's efforts and his contribution to the typology of the social structure of medieval towns and to the study of social roots of the Hussite revolution, is only now being developed and appreciated.³

Thanks to Mendl's studies and the efforts of a few of his followers, we now have a good knowledge of the nature of the social crisis in the towns of Pre-Hussite Bohemia and of its development into the Hussite revolution, at least in general terms, but we know very little of the consequences of this social conflict for the economies of Bohemian towns, and the little we do know has been derived largely from superstructural phenomena. The new edition of the municipal accounts of the royal town of Louny comes as a welcome contribution to fill this major gap and will be a stimulus towards new research in a field which after its early development has been virtually neglected.⁴

The period the edition makes accessible was of considerable significance for the economic expansion of the town and the growth of its power, as well as for the social and political life of Bohemia in its early phase. The people of Louny were initially adherents of Emperor Sigismund and then allies of revolutionary Tábór. They made every effort to strengthen their economic position and the profits gained during the period of revolutionary expansion, as well as their remarkable role in the political life of the country. However, they lost everything during the land revision in the Post-Hussite economic struggle. Nevertheless, their support for King George of Poděbrady won them other significant privileges. The editor describes these eventful years, and the dramatic reversals of the late XVth century, and shows how they were reflected in the social structure of the town and in the re-grouping of social

² Cf. *Knihy počtu města Brna z let 1343-1365* (Books of accounts of the city of Brno in the years 1343-1365). Ed. B. Mendl, Brno 1935. The editorial techniques used in this outstanding work of Mendl provided J. Vaniš with a model. For conclusions drawn from the abundant data of the period covered by Mendl's preparatory work, see: B. MENDL, *Sociální krise a zápasy ve městech čtrnáctého věku* (Social crises and struggles in towns of the XIVth century), Praha 1926 (reprint from *Český časopis historický* 30-32, 1924-1926).

³ M. MAREČKOVÁ, *Přínos Bedřicha Mendla k typologii sociální struktury středověkých měst* (Contribution of Bedřich Mendl to the typology of the social structure of medieval towns), *Historický časopis* 28, 1980, pp. 112-120.

⁴ Cf. J. MACEK, *Třídní struktura Loun v roce 1450* (The class structure of Louny in 1460), *Časopis Společnosti přátel starožitností* 58, 1950, pp. 157-170, 220-229.

and political forces in the short introductory chapter (pp. 9-25). In the next section he describes the unique qualities of the financial register and presents a comprehensive analysis.

The edition of the extensive codex itself is partly presented in the form of tables. To avoid masking the structure of the source itself by this procedure, the editor provides a description of the manuscript (pp. 28-32). Of course, the tabular presentation only covers the records of those receipts (*percepta*) which could be partly summarized. On the other hand, a similar procedure was not suitable for summarizing the disbursements (*distributa*) as this would have concealed the reasons for and the recipients of the payments which were very varied, except in the case of the more regular payments for guarding of town gates, for haulage, and other regular expenses. But these only formed a fraction of the range of outgoings recorded. The editor therefore avoided a two-column summary of the data on expenses which would have caused confusion and also have made the data very difficult to read. In addition there is also the fact that, over the relatively long period which the published material covers, the organisation of the records changed as a result of changes of compilers, as can be seen by comparing the records made in 1464, 1469, 1470, 1471, etc. However, the two-column system could be applied to the short summary of a fraction of the Louny register of interest payments and taxes, called "collecta", of 1460 (pp. 937-960).

The form of publication of the edition makes it usable in a variety of ways. It is not a mere source of particularly valuable economic data and of the volumes of compulsory taxes raised and payment made, but also a valuable source of information on the currency situation and for all the changes relating to the transformation of the social conditions of the inhabitants of towns, of the relations between the town and its tributary villages, etc. The individual records in the account book are also a very important source material for the study of Latin philology and its Bohemian and German forms, since Louny was located on the boundary of the two zones of language which explains the poor knowledge of Latin of the local scribes and their assistants who lacked a wide Latin vocabulary. Also important is the abundant data contained on the historical topography of the town. All this demonstrates the validity of the style of presentation adopted; had the approach been less well organised and the effort to save space excessive, much of the value of the source might have been lost. The editor was fully aware of this and he analyses all the possible ways of using the reference and the economic and financial data in the second section of the introduction (pp. 46-63). He also explains the editorial principles in the fourth part of the introduction (pp. 64-67).

He successfully deals with the palaeographic problems posed by more than twenty compilers, although the manuscript presents considerable difficulties in many places in this respect. He has also provided a critical series of historical references, listing in full all the abbreviations used, so making an

understanding of the financial data much easier for the less well-versed users. Abundant footnotes, which reveal the peculiarities of recording adopted by the individual scribes, their errors, misunderstandings and mistakes, complement this excellent work.

The degree of care taken in the preparation of this edition is also shown by the fact that the editor includes a full bibliography of the literature concerning the town of Louny, as well as abundant comparative literature and published sources, which he uses to explain the forms and methods of municipal accounting in general in the Introduction. In this way he provides a welcome stimulus for further comparative studies and a more profound understanding of the tax system in the royal towns of Bohemia in the Post-Hussite period, as well as pointing the way to a more precise terminology which is still often vague and imprecise. However, J. Vaniš did not only indicate the possible way of using the edition of the Louny register, but gave evidence of its practical importance for studying the administrative management of the town in the late XVth century.⁵

We have already stressed the value of this source for a general understanding of the economy of a certain type of Czech royal town. But it should also be said that this is not simple a local source, but one which characterizes and reveals the common features of those Czech royal towns which had extensive rural hinterlands. It also enables us to describe the type of town which was created in Bohemia, and elsewhere, at a moment of rapid change in the late medieval society. In particular it reveals the specific features of the Post-Hussite period in those regions of Central Europe which were subject to the influence of the Hussite revolution and where favourable conditions developed for the sudden feudalization of the urban ruling groups and its subsequent transformation, in a period of major social and political instability.

The commentary in Czech on the Latin text are perhaps rather inappropriate in this otherwise carefully prepared edition, but they do not constitute an insurpassable obstacle for foreign users, since they deal mainly with the graphical features of the manuscript, and with deviations and alternatives to commonly used Latin terms. Photographs of various parts of the Latin manuscript are included in the conclusion and enable the reader to form a concrete idea of the appearance and nature of the manuscript. The index only includes local names and place names, but not persons, and a subject index was omitted because it would have involved extensive repetition of the records described.

This new edition is an example of painstaking scholarship which required

⁵ J. VANIŠ, *Hospodaření královského města Loun v druhé polovině 15. století* (The accounts of the royal town of Louny in the second half of the XVth century), Praha Ústav čs. a světových dějin ČSAV 1979.

many years of effort. It will be particularly appreciated by experienced medievalists, and by specialists in the field of medieval economic history, especially in the field of economic comparative studies.

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K. WARREN, *Chemical Foundations: The Alkali Industry in Britain to 1926*. Oxford Research Studies in Geography. Oxford University Press, 1980, Pp. xi + 208.

The history of an industry can be approached from many different perspectives. One can emphasize technology, institutional/legal forms, finance, entrepreneurial individuals and families, or market structures. Kenneth Warren is a geographer, and his study of British heavy chemicals focuses on location. In view of the somewhat marginal status of the discipline in American social science, it may be stressed that the geographic perspective works well indeed in the present case. The book is clear and informative, while avoiding excessive technical detail, and it is generally readable despite the fact that the analytical argument remains somewhat veiled behind narrative. A goodly number of maps, tables, charts, and even photographs lighten the text.

The chemical industry went through three locational phases. In the first, which Warren designates as eotechnic, small-scale works were sited near raw materials or user locations, more frequently the latter. Although raw materials were typically bulkier than final products, the latter were difficult or dangerous to transport. In many cases, material orientation amounted to location in or near seaports, since such things as sulfur and barilla were imported.

In a second stage, beginning around 1825 in Britain, domestic coal and salt became the principal raw materials. By this time, the environmental problems associated with chemical manufacture proved unacceptable to many localities, an eloquent statement in view of the standards prevailing in mid-Victorian England. Fleeing litigation, chemical manufacturers concentrated in a number of "chemical towns," notably along the banks of the Tyne and in southern Lancashire. The latter concentration, which proved the more vigorous of the two, formed part of a highly articulated regional complex anchored on the great port of Liverpool to the west and the textile nebula around Manchester to the north and east. Widnes and St. Helens were ideally situated to benefit from access to coal and salt (as well as sulphur from pyrites smelters), while remaining close to the important alkali markets in soap, glass, and textiles.

In the last stage, beginning around 1870, the prime locations came to be single works set up in open country, often near salt pits and always convenient

to water transport. Rail lines were never far, while coal, though bulky, could easily be brought to the salt which was typically used in brine form. Around these isolated works, not dissimilar to the great German establishments, worker settlements were laid out. A far-reaching paternalism was perhaps the extension of a necessary concern for safety. Interestingly, these company towns and integrated works have proved durably viable in the twentieth century, while the old chemical towns still retain the disamenities inherited from their prosperous days.

Warren's central argument is that entrepreneurial factors and good access to the stimulus of markets were more critical to the success of given ventures and branches than the more tangible influences on cost. Before 1870, this is seen in the relative decline of Tyncside alkali and the success of the Merseyside (Lancashire/Cheshire) producers. After that date, the entire Leblanc-process alkali industry failed to meet the challenge of the more elegant Solvay process. The formation of the alkali trust (United Alkali Company) in 1880 was purely defensive, allowing an orderly, if profitless, retreat under the guise of "rationalization."

Warren's evidence, mostly in the form of comparative cost data, makes the argument in a negative way, showing that the less successful producers faced no great obstacles beyond their power to control. However, the abundant cost figures also testify eloquently to the fixation with static cost minimization on the part of early managers. By contrast, a number of quotations make it clear that the much greater potential of totally new techniques was systematically undervalued when any notice was taken at all. As a result, British alkali producers locked themselves into a game they could not win. The loss of early advantages, from a near-monopoly in such growing markets as North America and Scandinavia to cheap backhaul rates in importing raw materials to Newcastle, could not forever be made up by shaving pennies off processing costs. At that, a succession of undramatic improvements greatly lowered the cost of Leblanc soda ash throughout the century (75% from 1830 to 1886 alone, p. 40). But import substitution overseas combined with the competition from totally new technologies, including electrolytic splitting of salt, to finish off Leblanc producers early in the new century.

It may be unfair to take Warren to task for having limited the scope of his inquiry, but it would have been useful for him to look at least briefly at the related manufacture of organic chemicals. The dominant German producers typically combined the two, as the name *Badische Anilin-und-Soda-Fabrik* makes clear. Britain also had many advantages at the outset in dyestuffs and other coal-tar derivatives, from a large coking industry to the enormous market of her textiles. Thus, the failure to follow up on the English beginnings of the industry in Perkin's 1856 discovery of Mauve is important both for the chemical industry as a whole and to Warren's argument about the importance of entrepreneurial and other dynamic factors. At a time when Britain

seems once again to be leading the West, this time down the far side of the economic development curve, these early failures deserve another look.

If Warren is correct that entrepreneurial and technological vigour depend on nearness to markets, this helps explain the frustrations and persistent difficulties encountered by regional redevelopment efforts, from Appalachia to the Mezzogiorno. Static cost disadvantages can be overcome by fiscal advantages and investments in infrastructure, but the stimulating effects of active market-places are harder to provide. Therefore, human resources continue to flow irresistibly from the backwaters to the favoured centers, no matter how great the congestion there.

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