REVIEWS OF BOOKS

V. N. Bandera and Z. L. Melnyk (eds.), *The Soviet Economy in Regional Perspective*. Praeger Special Studies in International Economics and Development, New York, Washington, London, Praeger Publishers, 1973, pp. xii, 352, index.

The authors rightly point out that too much attention has been directed to Soviet national aggregates and too little to the regional distribution of either production or consumption. This neglect doubtless stems from several factors: 1) The fact that much of Western concern with Soviet economic performance has been dominated by military considerations which required aggregate figures to estimate military potential; 2) A tendency, at least in the United States, to underestimate the significance of regions. An American region — say the Great Lakes Region — is not a region in the same sense as the Ukraine, whose inhabitants are bound together not only by spatial proximity but also by common history, language and culture; 3) The optimism which prevailed until recently about the prospects for growth. Distributional issues were neglected in favor of increasing the size of the pie.

The papers in this book serve to rectify the imbalance. The book is, broadly speaking, about the equity and efficiency of spatial resource allocation in the Soviet Union. Throughout the papers run several interrelated themes:

(1) The conflict among goals (Koropeckyj, Wagener).

(2) Production, consumption, factor productivity, factor inputs, and demographic data, by republics (Schroeder, Whitehouse).

(3) The effects of institutions upon the regional allocation of resources (Trofimenko, Billon, Ziman, Woroniak, Hamilton, Jensen).

(4) The size and nature of interregional resource transfer (Wagener, Melnyk, Bandera).

(5) The efficiency of the regional distribution of resources (Holubnychy, Melnyk, Bandera, Whitehouse).

By far the most controversial topics are (4) and (5). Wagener, Melnyk and Bandera estimate the value of net resource transfer from the Ukraine at between 10 and 15 percent of Ukrainian national income. Melnyk's estimates are the most systematic, starting from budgetary receipts and expenditures and making adjustments for items omitted from either side of the account. He calculates the capital outflow from the Ukraine at 15.9% of the corrected national income of the Ukraine for 1959-1961. Bandera puts these figures in historical perspective: French indemnities to Prussia (1872-1975), 5.6% of national income; German reparations to the Allies (1924-1932), 2.5%; East German reparations to the Soviet Union were 28.6% of GNP in 1950, 18.4% in 1953 and 7.4% in 1954. Seen in this context, the volume of uncompensated resource transfers from the Ukraine is quite large.

What is worse, there is substantial doubt whether the funds were spent in ways that contributed most to the national economic objectives of output maximization and interregional income equalization. Bandera refers to Koropeckyj's earlier finding that the marginal productivity of capital is higher in the Ukraine than in other republics. Whitehouse's data on factor productivity show that there were substantial differences among the republics in roubles of output per unit of combined factor inputs in 1960; that the differences among republics were even more striking in 1970, with the European republics clearly leading; that the highest rates of growth of factor productivity during the decade 1961-1970 were found mainly among the European republics. Holubnychy raises many questions about the appropriateness of directing a large part of Soviet resources into the development of non-European republics. Combining Whitehouse's data on inputs with Schroeder's data on national income, it can be seen that, of the republics with well-above-average increase in inputs, seven (Belorussia, Kirgizia, Lithuania, Armenia, Moldavia, Tadzhik and Kazakh) had above average increases in output but that three — all of them non-European (Azerbaidjan, Uzbek and Turkmen) — had below average increases in output. At the same time Estonia, with below average increases in inputs, had above average increases in output.

The papers in this volume do not resolve the questions of the degree of equity and efficiency of spatial resource allocation in the Soviet Union; they do, however, raise the question and provide data on a republic basis which provokes us to start thinking systematically about these issues.

Underlying the discussion of equity, at least for the Ukrainians, whose contributions dominate the book, is an implicit evaluation of how the Ukraine has fared as a constituent republic of the USSR. If the Ukraine had been independent, there would not have been uncompensated capital transfers. The distress of Ukrainians over these capital transfers is compounded by the doubt whether the funds accomplished the national objectives for which they were intended. Clearly the Ukraine, which had slightly below averages rates of growth in the decade 1960-1970, would have improved its standard of living significantly if between 10 and 15 percent of its national income had not been siphoned off to use in other republics.

This comparison, while thoroughly understandable, does raise thorny questions about the standards of distributive justice under socialism. Socialism has a standard of distributive justice among individuals: to each according to his labor. Income derived from non-labor factors of production is in principle not appropriable by individuals. But what should the socialist standard be among *regions*? Is the Ukraine as a region entitled to the income generated by all the factors located on its territory — the regional analogue of the free enterprise principle? Or is the Ukraine as a region in a socialist country entitled only to the returns to the factor labor — the regional analogue of the socialist principle of distribution? By what standard are we to judge the equity of income distribution among regions within socialist nations? For that matter, by what standard are we to judge the equity of income distribution among nations? One suspects that these questions of distribution among regions and nations, raised in this volume with respect to the URSS, will be the dominant economic issues of the next decade.

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W. J. BARBER, *British Economic Thought and India 1600-1858: A Study in the History of Development Economics*, Oxford, Clarendon Press, 1975, pp. 243.

Pliny the Elder, in the 1st century A.D., noted the influence of India on Mediterranean economic life when he complained about the adverse balance of trade and the great annual drain of specie from Rome to pay for the import of Eastern luxuries. That ancient connection is often exaggerated but certainly since the 16th century the economic links between the Western world and South Asia have been substantial. Aspects of that relationship have been examined in considerable detail but we are still a long way from any balanced interpretation that places it in the context of the emerging network of international economic relations. Typically, attention has been given to the Western impact on India even though until the end of the 18th century the major economic effects most certainly ran the other way. This is true of the British connection, a point which emerges incidentally in Dr. Barber's useful volume. The association with India posed a variety of policy problems for Britain. Virtually every major contributor — heterodox as well as orthodox — to the corpus of Anglo-Saxon economic thought grappled in some substantial way with those issues. Despite the fact, scholars are totally unaware of the significance of the Indian connection in the development of modern economic theory.

Dr. Barber's book should widen the intellectual horizons of many economists. It will also expand the understanding of historians — no specific knowledge of economics is required — who are interested in India, in problems of imperialism or who want to know something of the issues with which British economists were concerned during the 250 years between the end of the reign of Elizabeth I and the transfer of control of India from the East India Company to the British *raj*. The book is divided into four parts, reflecting the dominant issues to which economists responded in the first half of the 17th century, 1770-1813, 1813-1853, and at the time of the the Indian Mutiny of 1857-58.

Barber sees the underlying theme — so his subtitle tells us — as a preoccupation with problems of economic development. This is acceptable only if we think of all economic theory as concerned with improving well-being and thus with development. But given the distinctions we usually make in the discipline, it is too confining a definition of the intellectual issues with which India forced economists to deal. Those which surfaced at the end of the 18th and in the 19th centuries were, it is true, specifically development-oriented but the problems of the first two centuries were of much broader substance.

The chartering of the East India Company in 1600 posed a host of economic problems that reflected the persistent validity of Pliny's complaint. Successful expansion depended on the export of gold and silver which seemed to critics to threaten a drain on the nation's stock of wealth and to pose a variety of foreign exchange difficulties. Thomas Mun published three essays between 1621 and 1664 in defence of the Company's policy and its role as a monopoly. He developed an analysis that showed that it was not the bilateral trade with two countries but the balance of Britain's trade with the whole world that was crucial and that England was truly enriched by the multilateral trade patern that the Company was developing. While Schumpeter dismissed "the overrated Mun", Barber is correct in stressing how Mun's theoretical formulation not only advanced the theory but gave shape to debates throughout the 17th century.

The success of the enterprise which Mun's analysis justified helped expand the British economy beyond the range of policies rationalized by his analysis. As early as 1701 the anonymous author of *Considerations upon* *the East India Trade* already advanced an extremely sophisticated justification of international trade based on productivity gains derived from the territorial division of labor. While Ricardo was probably not influenced by this precocious formulation, the developing debate of the 18th century made the foreign trade model more precise and also sharpened the general intellectual assault on the principle of monopoly. This, of course, culminated in Adam Smith's attack on the East India Company as a barrier to the working out of the advantages of a free trade system.

The first 175 years of debate were concerned only with problems that the India trade posed for British economic life. But by the end of the 18th century Britain's growing involvement in India raised questions about what economic policy should appropriately be followed there. Sir James Steuart's recommendations to the Company — *The Principles of Money Applied to the Present State of the Coin of Bengal* (1772) — involved forms of regulation that were already outdated. The new economic principles were already being worked out. The virtues of competition and free trade which produced an efficient international division of labor were accepted as the framework for further analysis. It was in these terms that the debates analyzed the issue of economic development. How could a poor society like India be stimulated to rapid growth? What were the appropriate policies to follow? Every great name of Classical economics — Smith, Ricardo, Malthus, Bentham, James and John Steuart Mill, McCulloch and Marx, to name only the best known — had something of substance to say. But there was a change. With the exception of Malthus and the theory of rent, it is not obvious that the responsibility for India directly stimulated many more innovations in economic theory. Rather, economic principles that were developing for other reasons were employed to generate "recipes for statesmen". Even if true, it was not an insignificant role. Utilitarianism, too formidable to be applied in Britain, had free scope for application to the problems of Indian development. Malthus occupied the first chair in political economy in Britain at the East India Company college at Haileybury. For nearly thirty years he taught the young civilians who were going out to rule India. Ricardian rent theory provided the intellectual framework for the tax policy of the continent. This and other Utilitarian innovations — including the attempt to establish an Anglo-Saxon system of private property — were designed to unleash individual initiative and break the circle of poverty within which the subcontinent was constricted. We still do not know what effect those Classical prescriptions actually had on the Indian economy. But it is clear that formal theory shaped economic policy more directly and extensively than it did at any other time until Keynes. Eric Stokes' magisteral study, *English Utilitarians and India* (1959), covers this group in much greater detail but Barber's straightforward discussion lays bare for all who can use the insight the link between an economic theory and economic policy. The lessons for the present are there to be learned.

The volume ends with brief discussions of Malthus' successor at Haileybury, Richard Jones, McCulloch, Marx and J.S. Mill. The last, in spite of a life spent in the employ of the East India Company, was apparently not stimulated to any original views by that connection.

One wishes that Barber had extended his volume to explore the intellectual links with India maintained by later economists. Marshall and Keynes are two obvious ones. It would also have been interesting to learn whether it is true that it was a visit to India as much as the example of Germany that turned William Cunningham into a critic of *Manchesterismus* as the vehicle of economic development and thus into a founder of British economic history.

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J. S. Davis, *The World between the Wars, 1919-39: An Economist's View*, Baltimore: The Johns Hopkins University Press, 1975, pp. xii+436.

Since retiring from a distinguished career in universities, in government, and at Stanford's Food Research Institute, Joseph Davis has remained prodigiously active. Now, at 90, such in the year of his death he has published a long, thoughtful, and scholarly book.

While surveying domestic and international political events of the interwar period, Davis focuses mainly on the background and course of the Great Depression. Several distinct approaches to so vast a topic are welcome, and Davis need not apologize for what his book is not. It is neither a work in econometrics nor a statistical compendium, although it does reproduce some figures and does refer to statistical sources. It does not present or test any grand explanatory hypothesis. It is not a smoothly organized chronicle (a semi-topical organization accounts for some jumping back and forth in time). Rather, it is a somewhat subjective gathering of many detailed facts. Prominent among them are facts on personalities, decisions, opinions, and attitudes, Davis's concern with who was thinking and saying what seems to give the flavor of the period better than either a statistical work or a smooth narrative could do by itself. If his sampling is representative, optimism about the economic situation prevailed both before the stock market crash of 1929 and for some time afterward; people really did expect the recession to prove mild and brief. Davis brings to bear the recollections of a professional observer, as well as notes accumulated over a lifetime. His book bristles with quotations from speeches and writings and with citations to journalistic and scholarly publications contemporary with the events and personalities discussed. He has added references to much more recent material, including a few books published as recently as 1969 and 1970 and several still more recent articles. His book should prove useful as a kind of annotated bibliography.

Davis does not explicitly reject other economists' explanations of the Great Depression. Although he does not actually examine the monetarist hypothesis, several incidental remarks suggest coolness toward it. He repeatedly cites Milton Friedman's and Anna Schwartz's *Monetary History of the United States*, but only to document points of fact rather than to come to grips with that book's thesis. (Strangely, Davis cites Clark Warburton only once, and cites none of Warburton's pioneering monetarist studies). On the other hand he does say (p. 366) that after the depression struck ,« The need to counter deflationary forces by bold monetary expansion and foreign lending was far too little appreciated », Davis has little to say about the relapse of 1937-38.

Davis's own « Analysis and Interpretation », saved for the third and final part of the book, is eclectic. He gives a long list of supposed weaknesses in the American and world economies before 1929, many of which he discusses in detail. The demand for consumer durable goods was relatively saturated; many branches of industry and agriculture had excess productive capacity; crookedness was at work in high places in industry, finance, and government; several major currencies had been repegged at wrong parities; international short-term capital showed "restlessness"; a mania was gripping the stock market; the U.S. banking system was shaky; and expansion of debt and of bank credit was excessive. (Davis shows more respect than is usual nowadays for the thesis that inflation, although not apparent in commodity prices, was an ominous reality in the 1920's). Calling both the depression and World War II « inevitable, though inherently preventable » (p. 418), Davis means that both disasters could in principle have been prevented by proper policies, which, however, mere human beings could not adopt, working as they were with their inevitable defects of knowledge and character. Davis makes much of the accidental element and the human element in history — illness, premature death, ignorance, excessive expectations of what governments and economists could accomplish, and excessive trust in the decency and good faith of persons and governments.

In a context concerned with Western inertia, pacifism, and wishful thinking in the face of the menace of Hitler's Germany, Davis expresses some insights that have broader application. With approval he quotes Winston Churchill (p. 334): « Virtuous motives, trammeled by inertia and timidity, are no match for armed and resolute wickedness ». Aptly in his context (p. 338), Davis quotes Harold Macmillan's characterization of a certain British cabinet minister — « a good man in the worst sense of the word ».

Evil does exist in the world, and there is such a thing as culpable blindness to it.

In some passages, Davis rises to eloquence. One need not agree with all of his economic diagnosis in order to appreciate his book and even find inspiration in it.

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R. DAVIS, *The Rise of the Atlantic Economies*, Vol. I of *World Economic History*, ed. Charles Wilson. Cornell University Press, Ithaca, New York, 1973, pp. xiv, 352 Foreword, preface, tables, maps, select bibliography, index.

Ralph Davis, professor of economic history at the University of Leicester, is the author of numerous articles and a well received monograph, *The Rise of the English Shipping Industry in the Seventeenth and Eighteenth Centuries* (1962). The present book is the first volume of a new series, *World Economic History*, edited by Prof. Charles Wilson. It is concerned with the countries on the western fringe of Europe — Portugal, Spain, France, England and the Netherlands — and the colonies they established or had dealings with in North and South America. It extends from the discoveries of the Portuguese in the fifteenth century to the beginning of the American Revolution and the publication of Adam Smith's *Wealth of Nations* in 1776.

Davis's book consists of eighteen chapters which may be conveniently divided into three parts. Portugal and Spain and their Atlantic enterprises are treated in the first four chapter. Linking the two nations was Columbus who was a Genoese in the service of Spain, but owed everything to Portuguese methods and experience. Portugal pioneered the trades with Africa, Asia and Brazil, while Spain built a new society in America on a European model which was extremely destructive to the Amerindias. American silver enabled Spain to pay for wars on foreign soil which were disastrous. Yet Davis contends that the monarchs of Spain managed to turn the American frontier of plunder into a frontier of settlement and diversified development which survived the recession in silver mining.

There are six chapters which examine various aspects of the Atlantic economies in the sixteenth and seventeenth centuries. Davis explains why privateering was supported by landowning families, merchants, financiers and officials in England, and by Huguenots in France. He delineates the main features of the great age of silver from 1550 to the 1620, the major agricultural innovations in Holland, the peopling of the English, French and Portuguese colonies with indentured servants and African slaves, the seventeenth century decline of Spain, the rise of haciendas, commercial ranching, plantations, workshops and debt peonage in Spanish America, and the end of sugar prosperity and the discovery of gold in Brazil.

In the last eight chapters the scene shifts from the Iberian nations and Latin America to Holland, England, France and their colonies. Holland inherited much of the old commercial and financial activity of the Low Countries and built a commercial empire that extended from the North Sea to the spice islands. England escaped the worst of the disasters that overtook much of Europe in the sixteenth and seventeenth centuries and, on the basis of agricultural, mining and industrial innovations and expanding colonies in North America and the West Indies, managed to achieve an industrial revolution. France overcame numerous difficulties and forged ahead of England in certain branches of industry, colonial enterprise and overseas trade but failed to transform her peasant agriculture which continued to be a brake on economic expansion.

Davis is concerned with the common economic forces which pervaded western Europe and the Americas. At the same time he points out that development was modified by each country's particular endowment of natural resources and political and social structures, and that at certain times and places influences were exerted by such factors as climatic change and epidemic disease which cannot be attributed to any kind of human action. He believes that the main influences on European economic development arose within the countries of Europe themselves, although these developments were powerfully affected by developments in the Atlantic basin. Agricultural improvement is of central importance in Davis's model of economic growth. Indeed, he shows how Dutch innovations in animal husbandry, new crops and rotation systems spread to England and provided an indispensable basis for industrialization by increasing the supply of food faster than the number of people.

A commonly neglected aspect of Atlantic development which Davis deals with in a competent manner consists of the tropical colonies in America. He traces the stages of development of the lowland tropical territories which were occupied by Spain, Portugal, Holland, England, France and Denmark, and cultivated chiefly by African slaves. He examines the technical and economic aspects of sugar plantation production and the great importance of sugar and other tropical commodities in the trades of western Europe and North America.

The British mainland colonies receive full treatment by Davis who combines outline economic histories of individual colonies and regions and analyses of major industries and patterns of trade. He says that regional and class differences should not obscure the common feature of a widely diffused prosperity which derived from the availability of almost unlimited areas of land that was virtually free. Continued development and prosperity of the mainland colonies depended on finding outlets for surplus agricultural produce in the British, French and Dutch West Indies.

Davis's emphasis on the economic history of several countries which differed in resources and institutions but had a common ocean frontier and interest in colonial enterprise and trade affords him wide-ranging opportunities to engage in comparative history. These opportunities have not been neglected. To cite one example, he observes that tropical sugar plantations changed colonial societies in much the same fashion that the factory for a time changed English society; « the efficient scale of operation required a large concentration of fixed capital, and the owner of the capital wanted a completely subordinated and rigidly disciplined labour force » (p. 257).

Neither has Davis neglected to examine and pass judgment on the major debates that have engaged modern economic historians. These include the price revolution and profit inflation theses, the capitalism and slavery thesis, the general crisis of the seventeenth century thesis, and the burden of the Navigation Acts thesis. Though his treatment is rather cursory and undocumented, Davis's discussion of these debates is full of interesting insights. The book contains useful tables, maps, a select bibliography and an index.

The Rise of the Atlantic Economies is a well-conceived, well-balanced and well-written book in which the author achieves a high level of scholarship in his synthesis and interpretation of the works of specialists.

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A. Di Vittorio, *Gli Austriaci e il Regno di Napoli 1707-1734. Ideologia e politica di sviluppo*, Naples, Giannini, 1974, pp. 535.

Italian economic history is still largely lacking in studies of economic development which succeed in overcoming the narrow and traditional limitations of the period and area studied. If one excepts certain studies on prices, exchange, banking and, to some extent, agriculture as well, the discussion of the relationship between development and the structural and ideological factors connected with it (as a result of its dependence on economic policies) still needs to be properly and comprehensively organized (see for example the remarks made by Luigi de Rosa in his paper to the II National Conference of the Historical Sciences at Salerno in April 1972).[1]

For this reason Antonio Di Vittorio's study, which deliberately sets out to reconstruct the framework of economic development in the Italian south in the early decades of the 18th century, is of great interest and deserves careful attention. The theme of Austrian rule in the Mezzogiorno is not one that is new for the author. In an earlier volume (*Gli Austriaci*

[1] Now in "La storia economica tra cicli e trends", *Rassegna Economica*, Naples, a. xxxvii, 1973, n. 1.

ed il Regno di Napoli 1707-1734. Le Finanze Pubbliche, Naples 1969) he studied other important aspects of this, and in the latest he provides a broad assessment of the economic policy pursued by Vienna in southern Italy in that thirty year period which lies between the end of Spanish rule and the establishment of the Bourbon monarchy.

In comparison with the later Bourbon period, the economic history of the years of Austrian rule has received little attention, partly as a result of the predominantly negative assessment which historians have accorded the political outcome of the period of Austrian rule [2]; a view which has tended to colour recent detailed studies of the economic history of the Mezzogiorno in the 17th and 18th centuries.[3] In fact, however, there were during the years of Austrian rule clear signs of an economic revival, the causes of which can be seen to lie partly in "natural" factors.[4] It was this which paved the way for a broader process of economic growth which became evident in the later 18th century. The author sets out not only to study the origins and scale of this revival, but also to show how closely it was connected with the encouragements provided by the Austrians' economic policies.

He starts by examining the economic and political situation of the Austrian empire in the early 18th century in order to show the stages by which Vienna's economic policies towards southern Italy developed. After the difficult first stage, when Naples was totally subordinated to the imperial economic system due to the War of the Spanish Succession, the decade from 1718-28 saw a genuine blossoming of economic ventures and undertakings which allowed the Neapolitan kingdom « a period of intense economic activity, as witness the very high revenues produced in these years from customs dues, from the *arrendamenti*, and from taxes and gabelles of different sorts » (p. XXI). But the final period of Austrian rule, leading up to the advent of the Bourbon monarchy, was again accompanied by « years of political tension, preparation for war and of actual and full-scale warfare » (p. 64), and by a general halt in the economic policies pursued by Austria, which after the Treaty of Seville in 1729 was concerned only to find new economic resources in the Neapolitan kingdom which would enable it to renew the war against Spain.

Austria's economic policies were strongly influenced by the ideas of mercantilists like Becker and Von Hornigk. They were convinced proponents of the principle of the complementary nature of the economy of the Danube basin, and were therefore inclined to draw the economy of southern Italy into the huge economic area contained within the empire. Vienna's "plan"

[2] See, for example, F. VALSECCHI, *L'Italia nel Settecento. Dal 1714 al 1788*, Milan, 1959.

[3] See R. ROMANO, "Economia e Finanza", in *Storia di Napoli*, Naples, 1970, Vol. VI, pt. 1, p. 592.

[4] See P. VILLANI, *Mezzogiorno tra Riforme e Rivoluzione*, Bari, 17973, p. 1.

was then decidedly mercantilist, its main objectives being to strengthen sectors such as mining, manufacturing, commerce and shipping, which « would create a surplus balance in both commerce and payments, derived from a surplus of exports and services » (p. 11). The results did not, however, always match up with the Austrians' expectations. As Di Vittorio shows, this was due to the difficulties surrounding Austria's attempts to intervene in an economy with a very under-developed infrastructure (see for example the passages on roads and communications in the kingdom), and also to the inability of the Austrian administrators to overcome the opposition of certain economic interests within the kingdom. The fate of the Sinzerdorff plan for encouraging manufactures, which was fiercely opposed by the Neapolitan magistracy, and the failure of the attempt to build a "free" port at Pozzuoli, provide the most important examples of the obstacles which confronted Vienna when it attempted to put its mercantilist policies into practice.

The author gives a detailed assessment of the results of the Austrian policies in the second half of the book, which contains an analysis of the structure of the southern economy in the early 18th century, and from which it is apparent that Austria's economic policy did produce results. Demographic developments in the Kingdom in the first decades of the century bear the first witness to this. Detailed research by the author, involving use of information drawn from a large number of parish and diocesan records, shows that population increase occurred throughout the Austrian period, and must be related to the stability which accompanied it and to the absence of harvest failures and epidemics. But the Austrian policy showed even more tangible results in the increase in production in both mining and manufacture, both of which illustrate the degree to which the kingdom's productive system reacted to the reforming policies of Austria. In the first decades of the century there was an increase in production in linen, cotton and hemp manufacture, as well as in other sectors such as ship-building. Corresponding to this increase in manufacturing there was a marked commercial expansion after 1718, which increased exports and provided a surplus on the trading balance, hence realizing one of the fundamental aims of Austria's mercantilism.

The study of the structure of Neapolitan commerce, hitherto neglected in the early 18th century, is particularly important. Di Vittorio describes especially the measures taken by the Austrians to widen the kingdom's trading area, both through new trading agreements with the Porte and by drawing Neapolitan shipping into the trading system of the empire. He also examines the role of the Neapolitan merchants in the process of economic development, which is of particular interest in view of the reluctance of the mercantile groups to participate in Viennese projects. From this he concludes that the absence of the great financiers and merchants from such undertakings, evident for example in their reluctance to participate in the Trieste fairs, proves that the commercial basis of the 18th century process of economic development was inadequate. He does note, however, that « at the base of the pyramidic structure of the merchant class of the kingdom » (p. 329) a very different spirit was evident, and many smaller merchants played an active role on the principal commercial markets throughout Italy.

This view is in line with that reached by the author on the sources of the investments on which economic growth was based. Di Vittorio's analysis confirms the conclusions reached some time ago by Luigi de Rosa in his study of the tax-farms or *arrendamenti*.[5] Di Vittorio concludes that « it was the social groups with agrarian, or mainly agrarian, incomes, such as the nobles and wealthy bourgeoisie, together with the corporations, especially religious corporations which were endowed with huge properties, which provided within the kingdom the main source for, and the main investors in, commercial activities » (p. 373). The policy of economic growth was then closely related to the pace of expansion in agriculture, and to the will and ability of the Austrian government to bring about changes in this sector. The sporadic and unplanned interventions in agrarian policy between 1707 and 1734 were, however, completely inadequate. As the author reminds us, « Vienna did not consider that the agricultural sector could make any valid contribution towards increasing the wealth of the State, and as a result it was limited to the function of satisfying the primary needs of the population, and in this — especially in so far as the capital was concerned — it was the object of careful observation » (p. 173). But even bearing this in mind, it is also apparent that southern agriculture was in the early years of the 18th century in a phase of gradual expansion. Di Vittorio attempts a structural analysis by zone and type of product, and modifies various views held about the evolution of southern agriculture in the 17th and 18th centuries. In contrast to statements made by Sereni,[6] he argues that, in certain regions at least, arable farming had begun to rival pasture « paving the way for a process which would come to a conclusion many decades later » (p. 123). This transformation was not attributable to any direct piece of agrarian policy nor did it result in a corresponding increase in production, because yields did not exceed those of the Spanish period and stagnation remained the chief characteristic of agricultural production. It can, however, be related to the developments in the kingdom's demographic evolution. A series of good harvests in the first two decades of the century made possible, as we have already mentioned, growth in population which increased internal demand and so encouraged the bringing of new land into cultivation. In the agricultural sector, then, the revival was not due

[5] L. de Rosa, *Studi sugli Arredamenti del Regno di Napoli*, Naples, 1958.
[6] See E. Sereni, *Storia del paesaggio agrario italiano*, Bari, 1961.

entirely to natural causes, but formed part, even if indirectly, of a more general process of economic expansion. And this process, as the author stresses, was aided, in the very years when the Austrians' intervention was at its highest, by the inflation caused by the circulation of large numbers of fiduciary bills issued by the public banking houses (there was an increase of over 40% in these bills between 1720 and 1729, see p. 363, table 42).

The thirty years of Austrian rule in southern Italy were, then, of a very different character to the long, sleepy, years of the Spanish vice-royalty. The author's basic thesis, that it is possible to see in the economic policies carried out by the Vienna government an authentic plan of reform, is largely proved in this weighty and broad study. But in addition to its methodological originality and modernity, which we have already mentioned, it has many other merits. The documentary material on which it is based is vast and exhaustive, and this serves to illuminate a whole period in the history of the Mezzogiorno about which hitherto little has been known. The documentation itself is also the fruit of the painstaking research which the author has undertaken in archives in Austria, Czechoslovakia, Spain, France and Italy. But this in turn has made it possible for him to set the southern Italian economy in these years against the broader context of the Mediterranean world, and, in seeking the motives for the Austrian policy, allows him to direct our attention from the narrow setting of Neapolitan politics to the much vaster one which influenced the decisions made by the imperial government in Vienna. He succeeds then in providing a study which goes beyond a simply regional perspective to embrace comprehensively a stage in the economic development of 18th century Europe which has attracted little attention.

In terms of the economic history of the Italian Mezzogiorno, this study not only corrects the unduly negative appraisal of the Austrian period, still dear to those who approach economic history from an ethico-political, or Crocean, standpoint, but also fills an important gap by providing a detailed reconstruction of the structure of the southern economy in the decade prior to the advent of the Bourbon monarchy. The study in fact has few rivals even within the rich and well developed field of the economic history of the Italian Mezzogiorno.

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G. DUBY, *The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century*, translated by Howard B. Clarke, London, Weidenfeld and Nicolson, 1974, pp. x, 292, 3 maps and 2 figures.

I write this review with embarrassment largely because I have not been able to acquire and study the French original. Nowadays, Mr. Duby writes a very elegant, if somewhat breathless prose, and this quality has not come over into the English. Still, in spite of occasional infelicities, Mr. Clarke's translation in easy reading and wholly adequate, and Mr. Duby is to be congratulated at having escaped the assassins who struck down such historians as Fritz Rörig and Max Weber.

Mr. Duby divides the period he treats into three ages. The first is that of the Carolingians which he rightly sees as a period of progress, demographic, commercial, monetary and institutional. After the relatively brief setback of the renewed invasions, the following age is that of the seigniories, the "seigneurie banale" of his earlier work on the Maconnais. Stretching it a trifle thin, Mr. Duby here extends the idea of the banal seigniory to the lordship of the king in England. Beginning in the twelfth century, the last age is that of the money economy and urbanization. In regard to the earlier two ages, Mr. Duby likes to repeat the notion that historians have often described as "economic" or "commercial" activities that were really related to the gift-giving or potlach ethos of the warrior group or class and its proclivity for plunder and gain by force.

Because this book is essentially devoted to describing the situation and style of peasants and soldiers, the clergy play only a peripheral role. Conceptions about the economy spelled out by the clergy only come to the fore in the third age, that of the money economy. In that section, as is customary in France at the moment, the attack on usury and the conception of voluntary poverty as the highest moral good is attributed simply to the rise of the bourgeoisie and the growth of the dominance of money. Here, as in not a few places in this often thoughtful essay, I wish to demur, but am reminded that it is surely the right of an author who is writing general history to simplify in order to get his points across.

There are, it seems to me, occasional infelicities, although one such example may derive from the translation instead of from Mr. Duby's text. This is the anachronism of describing persons some of whose time was spent in long-range commerce as "professional merchants", and as "professional soldiers" those whose primary duties were administration, defence, and war. It is odd that the former phrase, one culled from Pirenne, should appear here because, if there is one thing lacking in this essay, it is any direct reflection of the ideological struggles among historians in the past. Insofar as Mr. Duby has an ideology, it is that of Bloch's post-ideological history with its desire to tell the story simply "as it was". The only express reference to an ideology of historical analysis occurs when the author refers to the conception of feudalism or a feudal age as being the peculiar possession of the Marxists. It is possible that the Marxists may be the only ones stuck with it nowadays, although I doubt it, but to attribute it wholly to them must set those eighteenth and nineteenth century English liberal economic philosophers — not to speak of others — spinning like tops in their graves. Anyway, reading between the lines, it is clear to me that, with his apparently open-eyed innocence, Mr. Duby is a thoroughly anti-Marxian Little Prince.

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D.K. Fieldhouse, *Economics and Empire 1830-1914*, Ithaca, New York, Cornell University Press, 1973, pp. xvi, 527.

There is order and growth in the work of imperial historian D.K. Fieldhouse, from his noteworthy article « "Imperialism": An Historiagraphical Revision » (*The Economic History Review*, 1961) to the present stimulating volume. The article was a well-argued critique of the theories of Hobson and Lenin; this book is an expansion and sophistication of that argument made in the context of general history of imperialism in the period 1830 to 1914. In the article Fieldhouse maintained that the new imperialism of 1870 to 1914 was mainly a « political phenomenon in origin », and was « the outcome of fear and rivalries within Europe » extended to the periphery. Now Fieldhouse gives greater weight to economic factors but tries to show just how they related to political decisions made by the imperial powers. And he has greatly extended his treatment of what was happening on the periphery" The periphery means to him the areas of interaction of Europeans and non-Europeans on the fringes of established empires or in areas not incorporated within formal empires by the nineteenth century.

Between the 1961 article and *Economics and Empire*, he produced two other works, *The Colonial Empires* (1966) and a book of readings, *The Theory of Capitalist Imperialism* (1967). The former is a general history of the major colonial empires, the latter extracts of works on the phenomenon of imperialism, running from Adam Smith to Gallagher and Robinson and Fieldhouse. It is useful to look at this collection of readings in connection with *Economics and Empire*. One can see which theories he means to criticize and some of the sources he uses to answer Hobson and Lenin, though there is little post-Lenin-Marxist writing in his collection.

Economics and Empire is roughly framed as an investigation: Part One offers many questions and some of the theoretical answers of earlier writers explaining imperial expansion in the late nineteenth century; Parts Two and Three present the evidence, or case studies of expansion from 1830 to 1880 and then from 1880 to 1914; and Part Four evaluates the theories lined up at the beginning on the basis of evidence. The major question of the book is how best to explain the great expansion of Europe's formal empires in the three and a half decades before World War I. Fieldhouse also wants to see whether economic factors were important in this expansion, and, if so, precisely in what ways did economic factors (banking, commercial, industrial interests) influence political events.

He finds that economic factors did play a greater role than he believed when he wrote his 1961 article. The role of these factors, he now holds, was often indirect and economic factors were related in complex ways to political decisions and the relationship varied with time, place, and circumstances. Fieldhouse also argues that economic and political interests were often in disagreement, and instances arose (China is an example) where economic self-interest worked against the establishment of formal spheres of control and the imperial powers were restrained because economic self-interest was primary. In many other cases (the South Pacific, East and West Africa) emotionalism, mutual fears of the European powers, and rash actions by Europeans on the periphery led to extensions of empire that were often economically valueless.

Fieldhouse's emphasis on the crucial role of Europeans in the periphery and on the significance of crises arising in the peripheral areas for the building of empires sets him apart from other writers offering political explanations of imperialism (e.g., Robinson and Gallagher, *Africa and the Victorians*). Fieldhouse concludes that, « Although European attitudes were often influenced by domestic forces, the evidence suggests that positive action normally began as a response to existing peripheral problems or opportunities rather than as the product of calculated imperialist policy . . . In the most general terms it must be concluded that Europe was pulled into imperialism by the magnetic force of the periphery » (p. 463).

One problem with this explanation and with Fieldhouse's general approach is that he makes it seem as if the Europeans acting on the periphery were not in most cases connected by class, interest, and outlook with Europeans in the metropolitan areas. I believe he is correct in giving weight to the actions of the Europeans on the periphery, but these men also have to be seen as Europeans, as part of the classes and national societies of Europe. In his effort to downgrade economic and domestic factors, he has dissolved the bands of personal and class connection between Europeans at home and Europeans in the non-European, prospective colonial areas.

Fieldhouse's passion is directed against the great simplifiers such as Hobson and Lenin and he argues well against their theories. But in concentrating on the decisions to build empires and the theories about these decisions, he has not told us anything about the actual working of empire. For this we must go elsewehere. Meanwhile, he has offered us an intelligent and provocative version of European expansion.

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J.M. Hayden, *France and the Estates General of 1614*, Cambridge, England, 1974, pp. XII-334.

In modern times the opinions of almost any social group or people are aired by the mass media to the point that they are rendered banal; in medieval and pre-modern eras historians are often confronted by true silent majorities, the inarticulate popular cultures, and a "press" more often than not in the pay of governmental officials and magnates. The problem of discovering the collective beliefs, political and social behavior patterns, and aspirations of pre-modern peoples can often best be approached through studying their representative institutions. To be sure, not all beliefs and attitudes would be aired in an estates general, diet, *cortes*, or parliament, but the political life of a pre-modern society would be more active, coherent, and more overt in the debates of a representative institution, and therefore more discernible by historians. And in the traditional societies of early-modern Europe politics recognized no boundaries of subject or bureaucratic organization. Channels of policy and delimitations of functions into private, local, and national politics were only in their infancy in the seventeenth century. Thus a French estates general bringing together 474 deputies in 1614 felt perfectly free to discuss everything from religion to dress codes, trade, justice, education, censorship, foreign policy, dueling, and royal marriages.

The Estates General of 1614 was the last held before the one which helped precipitate the Great Revolution of 1789. Constitutional historians of the nineteenth century and their heirs in the twentieth (not the Marxist historians represented by Mathiez and Soboul) have been fascinated by comparing the two meetings in order to discern the shifts of thought and political behavior in the ancien régime. And yet for all the interest in representative institutions, Hayden's is the first full length study of the Estates General of 1614. It is richly documented, well written, and enormously informative to any reader interested in the collective mentalities and political cultures of early modern Europeans. The approach is essentially that of the constitutional historians of the nineteenth century (A. Thierry) in its republican emphasis, brought up to date by the use of all the recent monographs by Roland Mousnier, J. Russel Major, and David Buisseret.

Largely because the royal ministers were preoccupied by financial and foreign policy, Hayden begins with chapters on these topics, and on the princely rebellion which provoked the calling of the estates. Did the ministers believe that finances and foreign policy were the issues on which they were most vulnerable? It is clear from the near disappearance of these subjects in the following chapter that these were only of particular interest to the commoners. Though quite unrelated to the general debates of the estates, Hayden offers the most authoritative discussion of French finances available for the early seventeenth century; their gyrations may have inspired anger and curiosity among some deputies, but the principle that these were the king's business had general acceptance. True, the third estate appealed for a reduction of the *taille*, but beyond that the more "constitutionalist" deputies who sought to limit royal control over finances failed to gain much support even within the third estate.

What did excite the deputies, however, was reform of the church, and of justice. Background chapters on these subjects would have been useful, but since Hayden takes the time to delineate the background to these issues in his narrative of the debates, the reader has little difficulty in grasping the overall significance of 1614 on religious and judicial reform. Hayden accepts the description of French society as divided by functional orders recently stressed by Mousnier. The divisions into church, nobility, and commons constituted the principal parameters of political and social action. The quarrels over precedence and insults to the dignity of various deputies reveal that despite agreement on many issues each estate remained an exclusive and functional social identity. The *amour-propre* of each could not be bent to the common interest of all, and nowhere is this more explicit than in the debates over judicial reform. Each order at the local level possessed its courts and clienteles of minor officials; the criticisms by one estate of the judicial procedures of the others' lay bare the boundaries between social identities and justice in a traditional society. As Y. Castan has brilliantly elucidated in *Honneté et relations sociales en Languedoc, 1715-1780* (Paris, 1974) the resort to judicial proceedings by members of the various social orders represented an escalation of a quarrel, not the intervention of a judicial system which was repressive or interested in social engineering for the benefit of any single social class or ideological tendency. Here the defense of judicial proceedings and jurisdictions by each order reveals the vitality of the orders themselves, and their almost collective *machismo* as they castigated the judical proceedings of the other orders. Venality was the fly in the ointment; it embarrassed members of the third estate, infuriated poorer nobles, and brought forth charges of corruption from churchmen. Just how it affected each order's ability to exclude or to recruit new members is not revealed by these debates — any more than by Mousnier's thesis on the subject. Each estate was affected in different ways, yet despite their agreement in principle to have venality abolished it was to remain a characteristic feature of French society down to the Revolution, and would survive down to the present (notarial charges are still venal in France).

Debated with even more intensity, however, was religious reform. Led by reformers who wanted to institute the decrees of the Council of Trent in France, the first estate collided head on with the third, which opposed these reforms on grounds that they would violate Gallican liberties. At some points this quarrel was but another phase in the battle over who was to render justice; at others it was the prolongation of a long feud over theories of regicide and the place of the Jesuits in the French church. Hayden observes, to his dismay, that despite the brilliant record-keeping habits of churchmen, the clergy did not see fit to preserve their *cahiers* with the same care lavished on account books and other registers. This tendency to throw away *cahiers* always dismays republican constitutionalist historians, because it implicitly denigrates the importance of the estates as representative institutions. Instead, it ought to be taken as a clue to the realities of power, and self-assurance of the clergy in its ability to defend its interests without estates generals. So powerful and rich, why should they preserve their *cahiers* when they were almost autonomous within the society? Indeed, their "reforms" like the "reforms" of the other orders, were self-interested expressions of their own political vitality—as P. Goubert has put it in his *l'Ancien Régime*, II (Paris, 1973), a *pouvoir* independent of any common civic identity.

To be sure, there were requests made for more frequent and regular meetings of the estates general. These requests have always been of interest to constitutional historians because royal officials' failure to comply with these appeals appears to be evidence to stifle the estates. Occasionally Hayden views the government as a *bête noire* because of the alleged hostility of royal officials to holding meetings of the estates. But where is the evidence of this overwhelming hostility to estates? The *pouvoirs* of a very elitist society could force a meeting to be held, but not relatively powerless individuals in the third estate. Indicative of the marginal importance of estates generals to the political life of the ancien régime is the loss of interest in the estates on the part of Parisian society after the first few weeks (p. 148) and the failure of the orders to place in their cahiers a request for further meetings of the estates general. More likely, the estates were doomed to a long oblivion as a result of the cost of attending such meetings and the remoteness to individual members of most of the issues discussed. Men seeking popular support and in need of an audience of locally influential persons would appeal for an estates general, as Condé did in 1613, his son, the Grand Condé did in 1649, and as power-hungry intellectuals such as Fénelon did at the end of the seventeenth century, but the general if grudging willingness to accept government by ministers in a society of conflicting *pouvoirs* more accurately explains the lack of further meetings of the estates general.

Indeed, the estates could never do much more than to express a consensus about reform, in reality a kind of nostalgia or desire to return to the "good old days". Frenchmen from all estates agreed that the royal domain ought to be reconstituted so that taxes could be lowered, that evil tax collectors ought to be prosecuted, the king's life protected, and that venality should be abolished. Meetings of the estates were not needed to gain consensus about such reforms, but how could a return to the "good old days" be brought about when the real monetary and social interests of the various estates would have to be sacrificed in order to bring them about? A bit like prostitutes, royal ministers, tax collectors, and venal judges would carry out the functions of the state without directly infringing on the powers and privileges of the estates, and in this way the return to the past was avoided until a new era broke forth sometime after the mid-eighteenth century. When the estates met in 1789 the orders, like the meaning of reform as a return to the past, had sufficiently broken down to make a public consciousness appear strong enough to transcend private interests. Reform had a new and quite radical political significance because it proposed to assume all the powers of the estates, and of the state itself, a notion quite beyond the range of political thought and action in 1614.

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P. L. Landi, *La Leopolda: la strada ferrata Firenze-Livorno e le sue vicende (1825-60)*, Pisa, Pacini ed., 1974, pp. 256.

For ease of discussion this book can be divided into three parts. In the first of these the author examines the effects of the rapid spread of the new means of locomotion on the cultural and commercial world of Tuscany through the articles published in Vieusseux's journal "Antologia" and the "Giornale Agrario Toscano". After first describing a number of essays by Andreini, who from 1835 onwards strongly championed the Florence-Leghorn line, by Corsini, Corsi, and finally Serristori and Dini (a joint memorandum from these two addressed to the Grand-duke is republished in the appendix), the author devotes the remainder of the long first chapter (66 pages) to the Florence-Leghorn line. Although the title of the chapter refers to the preliminary financial arrangements, a wide number of problems are in fact treated here. He describes the inauguration of a service to provide statistical information on the commercial movement between Pisa and Florence, undertaken by the Administration of Roads and Waterways but in fact paid for by Serristori and Dini. He also deals with the attempts by both "certain merchants of Leghorn" and by the house of Pietro Senn and Emmanuel Fenzi to win the franchise, which in fact went to the Senn-Fenzi Company, with Dini's attempts to obtain an indemnity in return for losing the contract, and with the long negotiations required to draw up a register of responsibilities and with the legal problems of the company. In fact only fourteen pages of this long chapter deal with financial operations, and examine in particular the relations between the Senn-Fenzi Company and their shareholders, which were made difficult by the European wide-economic and financial crisis which developed after 1837.

Chapter II follows its title, « The building of the *Leopolda* », rather more closely. The author describes the problems surrounding the construction of the line, both those arising from the interventions of various individuals pressing for one deviation or another from the original plans, and from the actual building of the railway which was done in six sections. The chapter ends with a paragraph on investments and provides a comparison between the actual expenditure and that estimated by Stephenson, who was called in to draw up the plans for the project. The third chapter deals with the operation of the *Leopolda* between 1844 and 1860, and is mainly taken up with commentary on a number of tables (the sources of which the author does not cite, nor does he make it clear whether they have been devised from transcribed or from estimated information). This is partly descriptive and partly designed to explain unexpected falls or increases in revenues.

There are also six graphs in the sixth chapter. Three of these (n. 2, p. 142; n. 5, p. 148; n. 6, p. 150), however, are devised using techniques which almost totally destroy the advantage of graphical presentation. The author wishes to compare the development of two curves, but in choosing two different units of measurement in fact makes any comparison meaningless. The graph on p. 152 (n. 7) is also incorrect, not only because it uses the same technique, but also because the expenditure of the company is given in tens of thousands despite the fact that the author states that they are given in *hundreds* of thousands (the graph portrays the data given in the final column of Table 7, p. 156). The chapter ends with a paragraph on the attacks on he railway line by carters and "bargees" whose interests were damaged by competition from the new means of transport. The railway also suffered attacks from peasants who thought that the steam-engines were responsible for cholera and vine diseases.

In the third part the author turns to consider the famous polemic between Petitti, Cavour and the Lloyd Trieste Company, which began with Petitti's famous book on railways, as well as the debate over the choice of line to be used in Italy by the India Mails. Since the topic is not directly related to the theme dealt with in the book, and since it is not, in my opinion, adequately developed, it would have been better had the author simply referred his readers to the texts of the polemic between Cavour and Petitti, and to other studies such as those by F. Arese ("Cavour and the Railways") and Guderzo ("Roads and communications in Piedmont between 1831 and 1861"). The other debate, over the line to be adopted by the India Mails, in fact of course concerned every Tuscan railway, not simply the *Leopolda*, without mentioning that it dragged on for so long that in the end those taking part began to envisage a type of railway expansion which, in particular, in the case of Tuscany, bore absolutely no relation to the realities of the period.

Landi's book is based mainly on documents to be found in the cities touched by the railway, and on articles from the *Antologia*, the *Giornale Agrario Toscano*; as well as the *Giornale del Commercio* and the *Annali Universali di Statistica* and some others of less importance. However, the author uses the material he has found with such a lack of selective criteria or critical approach that he continually manages to disrupt the thread of his narrative, which quickly gets lost in a tangled maze. As a result, much of the book, especially the long first and second chapter, becomes a mere descriptive register of events. When, on the other hand, the author broadens his theme to discuss railways in Tuscany and Italy in general, the studies on which he relies are totally inadequate and lead him into statements which are either too general or else downright wrong. For example, it is true that between 1825 and 1830 « the views which were published in the reviews, pamphlets and newspapers were very small in number » and also that those published « showed a certain indifference towards the new means of transport » (p. 45), yet this does not constitute grounds for generalization. The first request for a railway franchise in Italy, for the Genoa-Po line, dates from 1826 only four months after the opening of the Darlington to Stockport line!

On the other hand, it is totally untrue that « the first line opened in October 1839 in the Kingdom of Naples (*sic!*) » was short and designed mainly to « satisfy the curiosity of the public and ... to facilitate the movements of the royal family » (p. 6). In fact, in October 1839 only the first stretch, from Naples to Portici, was opened of a line which by the end of the Bourbon period had reached Vietri, that is virtually to Salerno, after overcoming enormous difficulties due to the nature of the terrain. This is also not to mention the branch line to Castellammare, which had been planned from the start and by means of which the line reached a total length of seventy kilometers. Landi also fails to mention that this was not a state-owned line, or one subsidised by the state, but a line run entirely at the risk and expense of a private company, based almost entirely on French capital. The company did not receive a penny from the government. Landi must be alone in believing that a railway could operate for over twenty years without making any profits, relying solely on the curiosity of the Neapolitans.

Chapter III is certainly the most important, because it contains information which ought to provide a comprehensive picture of the economic state of a railway company in pre-unification Italy. Unfortunately the author becomes hopelessly lost in a welter of useless detail in which the central aspects of the problem are completely lost sight of, so that we are treated instead to matters of only secondary importance which appear disjointed and incomprehensible. The book makes no attempt to explain why it was that the railway, despite the fact that it ran through regions where the economy was essentially agrarian, should have been one of the most flourishing in Italy both before and after unification, until in 1865 it was amalgamated with the Romagna line, more as a result of government pressure than desire. In all, by failing to establish the central themes, the author has produced a book which is wordy and at most contains the making of an article, not a full study. This is a pity, because it is a particularly interesting topic and one which could provide interesting insights into the relations between transport and economic growth, which even today is still an immediate and fascinating issue.

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G. PENELEA, *Les foires de la Valachie pendant la période 1774-1848*. Bucharest, Editions of the Academy of the Socialist Republic of Romania, 1973, pp. 189 [Bibliotheca Historica Romaniae, Section of Economic History, n. 44 (41)].

The Wallachian fairs, those « rassemblements importants et organisés, à périodicité régulière et espacée, de marchands venant de régions éloignées » — as they were termed during the colloque organised in 1953 in Bruxelles by the Society "Jean Bodin" — had not been studied in Romania, in spite of the indisputable interest which might be raised by these early examples of trade and commerce.

This complex task has, however, recently been assumed — and, as one might expect, with great success — by dr. Georgeta Penelea. Her assiduous researches in the archives and her examination of the relevant literature has enabled her to reach some very interesting results about the organization of the fairs in the rural environment of the Wallachian Principality, during the period of the dissolution of feudal relations in agriculture. She also obtained very important information on the legal status of these fairs, about their participants, the wares which were brought for sale, the methods of taxation, and so forth.

The register of the fairs proves to be of an outstanding importance. An alphabetical list is given for each county, taken from statistical records dating from 1834, and records the periodicity of each fair, the names of the landlords who were the owners of those estates on which the fairs were allowed to be organised, the list of the products brought to each fair, and the taxes raised from them.

Equally useful is the statistical appendix consisting of five records covering the period from 1827 to 1842. This appendix illustrates certain peculiar aspects of the problem under survey.

In order to explain fully the specific features of the fairs in the Romanian Countries, the author uses a comparative analysis, and, in fact, she studies the evolution and the spread of the fairs not only in South-eastern and Eastern Europe, but also in Western Europe. She also attempts a very interesting excursus in the historiography and sociology of this important problem on a global level.

The terminology of the Wallachian fairs, includes three expressions which were used simultaneously, namely, *tîrg*, *zbor*, and *bîlci*. The former two are of Slavonic and the latter is of Turkish origin.

Dr. Georgeta Penelea first explains the juridical status of the fairs in the Wallachian Principality during the period from 1774 (the year in which the peace treaty of Kuciuc Kainargi, between the Russians and Turks, was concluded, which to some extent modified the hitherto exclusive economic monopoly exercised by the Porte in both the Romanian Countries, in order to ensure the food supplies required by Istanbul) to 1831 (the year in which the *Règlement Organique*, the legislation adopted in the Danubian Principalities during the period of the double protectorate of the Czar and of the Sultan, came into force). During this period, the author states that there were two categories of fairs, which varied according to the nature of the property on which the fairs were to be held: those held on ground owned by free peasants, and those held on the great estates belonging to the boyars of the monasteries. The prince of Wallachia enjoyed supreme authority over the fairs, through his sovereign right of *dominium eminens*, held *ab antiquo*. He permitted the holding of fairs only when they fulfilled the following conditions. They had (1) to obtain the consent of the inhabitants and of the neighbouring merchants; (2) to maintain a proper period of time between the fairs organized in different localities, so that they might not compete with each other; (3) to obtain an adequately sized ground for the fair; (4) to observe the so-called *nazim*, which ordered that fairs be held only beyond a distance of more than then hours, travelling time north of the Danube, to prevent the Turks settled in the so-called *serhats* (a *serha* meaning a Danubian fortress) visiting them. The main privilege of the prince consisted in the right to collect taxes, that is a direct tax on commercial transactions which involved importing or exporting wares. In very rare cases, the prince sometimes consented to cede this tax to the landlord on whose estates the fair took place. But the prince could also order the annulment of fairs held on the estates of a boyer whom he presumed to be unfaithful to him. Finally, the central power was also responsible for security arrangements.

Among the benefits allowed those who organised such fairs on their estates, the author lists the right to sell alcoolic beverages, and the right to raise taxes from the merchants who participated in them.

On the juridical status of the fairs from 1831-1848, when the *Règlement Organique* ruled both Romanian Principalities, dr. G. Penelea shows that the main innovation of the new legislation consisted in suppressing the internal taxes, in the reduction in certain of the conditions relating to the organization of fairs, due to the removal of some of the former privileges granted previously to certain landlords. In exchange, they were allowed to raise only certain duties which varied according to the nature of the goods which were sold during the fairs.

As far as the people who attended the fairs are concerned the author describes the overwhelming presence of peasants and inhabitants of neighbouring villages, as well as their capacity both as producers and as consumers. But the main participants were foreign merchants, coming chiefly from the neighbouring countries: Turkey, Austria and Russia. In contrast to the Western fairs, which used to specialize in certain branches of production and in selling certain goods the Wallachian fairs, even in the 19th century, were not specialized, and at such fairs, as well agricultural and food products were sold as well as textile wares, metal wares, etc.

The great mass of Wallachian consumers could satisfy their demands by purchasing products furnished by small local handicraft producers, such as the blacksmiths, farriers, joiners, tanners, leather dressers, curriers, tailors, potters, chandlers, etc. It was only the upper classes, and the rich, the merchants, boyars and high clergymen, who could satisfy their more refined tastes by purchasing foreign manufactured products, such as fine cloth, silk and velvet fabrics, luxury furs, or colonial goods. The main article of foreign trade, we learn, was cattle, and the data for 1834 fully attest this: 40,00 horned cattle were destined for the home market, and 38,957 for export.

The price of the products sold during the fairs is not very easy to calculate owing to the monetary chaos which reigned during that period in the Romanian Principalities. This makes it very difficult to interpret statistical data. Current exchange were effected in *"parale"*, *thalers* and *ducats* (Austrian coins), in Russian *roubles* and in Turkish *mahmudeles*. Prices were generally established by the central authority — through the so-called "*nart*", or mercurial, which began in 1783 — and varied according to demand and supply, and the circumstances of prosperity or economic recession.

The taxes raised by the authorities on the transactions during the fairs generally varied, according to the capitulations concluded by the Ottoman Empire with the European powers, around the minimal coefficient of 3% *ad valorem* on sales. To this sum has to be added the value of the rent to the proprietors of the ground for the right of putting up shops in order to sell the wares. From her researches the author claims that during the period from 1768 to 1829, the sum total of the revenues obtained by the Wallachian authorities from the fairs amounted to between 115,000 and 800,000 *thalers* yearly.

The last chapter of the book describes the location of the fairs in the Wallachian Principality district by district, from which it is possible to see the regions where the fairs used to be agglomerated, that is in the hilly region, where the best conditions for trade obtained. In the first half of the 19th century, 1450 fairs were held in 660 different localities.

Finally, dr. Georgeta Penelea shows that the disappearance of the fairs in Wallachia, towards the middle of the 19th century, was due to the fact that their economic function had ended. This was the final result of the capitalist exchange economy, for permanent trade became strong enough to replace the fairs, and the national market broadened itself enough to fulfil the function which the fairs had previously performed.

It is only to be regretted that this interesting work contains no map of the geographical location of the fairs mentioned; there is also no translation into French of the contents of the documents published, and no general toponymical index which would allow the reader more rapid orientation in the different chapters of the book.

But these deficiences are chiefly concernend with the conditions in which the book was printed, and by no means diminish its value. And this, we wish to emphasize, derives from the rich evidence it contains, and is due to the professional skill and erudition of the authoress, who consistently supports her statements with documentary evidence.

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TH. K. RABB and R. I. ROTHBERG, *The Family in History: Interdisciplinary Essays*, New York, Harper and Row, 1971.

Despite the resistance of disciplines to serious interdisciplinary modification, it is always refreshing to find new efforts at bringing together the substance and method of different fields of inquiry. Certainly such efforts help to clarify the degree to which and the ways in which such multi-dimensional approaches may engender more fruitful intellectual discovery. *The Family in History* is a valiant effort to develop such an integrative approach to the study of the family across many centuries. The different essays focus either on using family data to bring new insights to historical processes or to develop a sens of process and change in family organization and childhood. The usefulness of data on the family for new hypotheses about historical epochs is immediate in its potential; the validity of its contribution is subject to conventional tests. Further work on changing patterns of childhood and the family through time should be of great value in stimulating a deeper appreciation of variations in psychological functioning under those different social conditions that historical knowledge can so effectively elucidate.

Two major integrative essays appear only at the end of the volume. Tamara Hareven's comprehensive and incisively critical review of social science contributions to understanding the family is eminently worth careful attention. While one cannot cavil at the overall thrust of her argument or the more detailed criticism, it is too harsh a view. She ignores the contributions of classical psychoanalytic theory, including a number of explicitly historical analyses, which are often stimulating if no more conclusive than the other psychological and anthropological approaches she reviews. Even more questionable is her rejection of sociological contributions. Her critique is apt, but it ignores studies with particular potential for historical application: class differences in family behavior, cross-cultural studies of child-rearing, and family-relevant analyses in the work of several contemporary urban historians strongly influenced by sociological approaches. C. John Somerville's bibliographic note on childhood history is a useful if very brief essay with extensive references that should help encourage further investigation.

The collection includes a wide variety of types of essays from the broad integrative survey to book reviews to specific contributions. Few are entirely negligible in significance. But some of the best essays in the volume are reports of original research. One of the finest of these is the first essay by Emily R. Coleman on the significance of medieval marriage characteristics for clarifying complex problems in understanding serfdom. This is a superb example of the use of demographic data along with careful hypothesis-testing for evaluating a major, large-scale issue in medieval history: a gradual "mobility" process with a long-term decline in the lowest ranks of servile workers and an increase in the higher ranks. The data from a ninth century French seigneurie are great support. More significantly, Coleman traces the dynamic to the fact that with disproportionate frequency women married down to gain control of property and men married up for psychological reasons. She proposes that, in view of the fact that the child carried the mother's status, these factors account for the long-term shift in the servile class structure. Peter Laslett, whose published analyses of the implications of demographic patterns for understanding social structure have already had considerable impact on our appreciation of family and community in the 17th century, also contributes a valuable essay. His essay, however, is largely methodological in pointing up sources, hypotheses, and potential errors in estimating age at menarche. It is enlivened by constant reference to the social structural significance of differences in the average age at which child-bearing is possible. Edward Shorter also contributes a fascinating and provocative but less than fully satisfying speculative essay suggesting that a change in psychological orientation and a simultaneous change in the social structure of industrializing Europe led to a vast increase in premarital sexual behavior and a marked elevation of illegitimacy. His paper raises numerous questions of theory and data that are beyond the scope of a review other than to indicate (a) since the connection between illegitimacy rates and increase in premarital intercourse is so central to his argument, a fuller consideration of the range of alternative explanations of changes in illegitimate birth rates is essential, and (b) the neat paradigmatic approach to social explanation in the light of gross shifts in two (or more) independent variables is often initially helpful but may obscure the real dynamic by moving too rapidly from fragmentary concrete evidence to abstract categories.

Robert V. Wells' analysis of life cycle changes in American families between the early 19th and the early to mid-20th centuries reveals both the potentials and limitations of demographic history. Using a relatively lean statistical analysis, Wells determines that the period of commitment to child-rearing was very considerably longer in the early nineteenth than in the mid-twentieth century. It started earlier, there were more children, and it lasted until the parents were considerably older. The greater longevity during the twentieth century, signifying the likelihood that both parents are more likely to be alive and living together (in spite of higher divorce rates), means that a considerably larger proportion of husbands and wives are able to be together after their last child has left home. The analysis is neat and the historical vignette it presents interesting. But demographic analysis, concentrating on an important set of intervening processes, needs to be linked either with its determinants or with its social consequences. Wells starts on that path but it requires more than demographic variables to clarify the effects of this change on the later years of marriage in any century. At almost the opposite extreme, of greater richness of hypothesis than the data will bear, John Demos presents psychodynamic hypotheses to link Puritan child-rearing with Puritan adulthood and social process. Demos' major theme traces the traumatic weaning of the Puritan children (after a period of indulgent infancy) to pervasive ambivalence in adult character structure manifest in quarreling and slander, behaviors antithetical to Puritan values. The hypothesis, as Demos argues, is plausible. But the inaccessibility of many facts necessary for proper documentation, difficulties that beset clinical reconstruction with a patient available to confirm or deny, poses serious problems for science and requires rigorous and critical evaluation. Demos rejects various economic and social structural explanations which might account for the ambivalence that initially led to the particular (and not unusual) shift from nurturance to deprivation. Assuming the intervening adult psychological process is correctly described, that there was a sense of vanquishing the wilfulness of the child, why was this method chosen among the many possible? An adequate psychological explanation would have to go further and begin to demonstrate some degree of inevitability, rather than mere plausibility, in the argument.

Other critical observations about this approach to colonial America are given in a review of several books by James A. Henretta. But this essay by Henretta is far more than a review and presents an integrative if brief analysis of relationships betwen the economics of frontier farm settlements, demographic change, and family and community life during the colonial era. Even in this essay, however, and seemingly in the books it reviews, there is insufficient consideration of the complex interplay of variables necessary for tracing causal sequences. Contemporary sociological theory or the theory of economic development cannot come to the aid of such historical analyses, uncertain as it is in its formulation of models of social change. In one sense, one must be grateful that some of the interactions between environmental ecology, population, the economy, and family and community life are considered. Only the quantification and accumulation of data sets for more comprehensive historical analysis will allow fuller multivariate analysis. Joseph Kett's brief intellectual history of the concept of adolescence in the 19th century reminds us how grateful we should be for some of the other essays despite their limitations. It reveals the arid nature of an intellectual history which charts changes with no effort at understanding the social processes in which they are embedded.

Virginia Yans McLaughlin's study of work and family life among first generation Italians proposes a larger objective than her methods justify. She objects to the tendency among sociologists to emphasize the significance of work as a determinant of family paterns. She tries to counteract this by showing that, despite the assumption of new economic functions by Italian women in the United States, the family power arrangement was not altered. Even if she succeded in such a demonstration, it would only indicate that the organization of work is not *sufficient* to account for family patterns, a straw man. But demographic methods are inadequate for such a purpose and the analysis limps along, unsupported by adequate data concerning the stability of the family power structure. Systematic interviews with older Italian men or women would have revealed *their* sense of the drastic change in the structure of family power over time and in the American-Italian contrast. Lois W. Banner also deals with questions of female emancipation in reviewing a compendium of books on women. These range from a collection of essays on sex equality by John Stuart Mill and Harriet Haylor Mill and an early 20th century study by Emily James Putnam to several recent volumes. The return of repressed issues concerning the position of women in society becomes startlingly evident. It is a good review and clarifies both the recurrent themes and the many uncertainties about the role of women in past societies. But Banner's assumption (treated as a conclusion) that men have difficulty in understanding their female subjects would suggest that only a representative of a population can understand that population, posing a *reductio ad absurdem* in Banner's effort to understand the writings of men on women!

In another useful review, Rothman discusses several studies of childhood and the family in American society. He points to the excellent documentary presentations in Bremner's first volume on childhood and youth in America, but quite properly criticizes the underlying assumption of linear progress in encouraging independence and autonomy among children. The recent Supreme Court ruling supporting teachers' use of physical punishment despite parental prohibitions may be a retrogressive move from the more glorious days of that body, but almost certainly represents the temper of our times. Like several other reviews in this volume, Rothman's discussion of books achieves the status of an original essay and is one of the few papers in which the integral relationship between family and community life is explicitly recognized. In an effort to formulate general propositions, Kenneth Kenniston is quite properly critical of the unilateral emphasis on various mechanisms of "internalization" in accounting for child development, a process that can insure conformity but not change. But the perspective Kenniston provides reinforces the conceptualization of stability: « members of any given society . . . tend to share a highly similar developmental matrix . . . » and to show « important constancies in the modal developmental profile of adults » (p. 153). His discussion of the difference in developmental issues, sequences, and patterns of emergence would have benefited from documentation with either historical, cross-cultural, or cross-class examples. And there is no effort to document the view that psychological change may be causally significant for social change. Indeed, the impact of recent history on the changing characteristics of young people today, which he mentions, represents the causal influence of social change despite the hope (and it is merely hope) that many of us share that it may also impel further social change.

Pierre Goubert tries to cover a great amount of material on the French school of demographic history in short compass. The discussion of the origins of the "movement" as a convergence between demographers and historians and the rapid proliferation of many findings is interesting as a bird's-eye-view but is frustrating in the brevity with which each study is presented. Little of the material concerns the family despite the general relevance of family studies for demographic history and demographic historical studies for analysis of the family. There are glimmerings of recognition that demographic analysis cannot be dissociated from social structural, political, economic, and psychological analyses but too often the studies cited seem to bask in the discovery mainly of the new opportunities revealed by methods of population study. Etienne van de Walle's concise review of two historical studies, one on modern French family life and one on children in English society, concerns itself with the opposite danger, of the loose application of psychological theory to somewhat slippery data.

The volume as a whole presents a rich and varied diet, varied in style, in method, and in quality. Interestingly enough, the variations in quality are relatively unrelated to differences in method or style. As important as the presentation of these materials in a single volume, however, are some of the implications that emerge as by-products. (1) The quantitative study of history is essential to clear historical understanding and a fuller examination of potentials appears to be finding more adequate sources and bases for quantitative analysis than initially seemed to be the case. (2) Comparative methods have regularly been used in historical thought but far less frequently in the historical analysis of primary sources. Yet each of these studies and discussions reveals (often by indirection) the importance of direct comparisons in unraveling historically unfamiliar institutions like the family or childhood. (3) At the present time, historical research has more to offer substantively than to receive from the other social sciences. While there is considerable information concerning psychological functioning and social structure and dynamics, the theoretical bases of psychology and sociology are meager and their shortcomings become more apparent in application to history than do their merits. (4) From a methodological point of view, even from the vantage point of the process of theoretical development, the reverse may be true. The discussion of social units like the family, work organization, and community, units that have mainly indirect effects on the large-scale political and economic processes most familiar to historians, reveal the need for greater subtlety of analysis of multiple determinants and complex, direct and indirect, consequences.

While psychosocial and historical analysis may have to function much like the deaf leading the blind (or is it the lame leading the halt?), they can contribute substantially to more linear social understanding. The promise of this collection of essays lies in revealing this more clearly, as well as in its clarification of the importance of "microunits" like the family in understanding history.

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E. Shorter and Ch. Tilly, *Strikes in France, 1830-1968*, London and New York, Cambridge University Press, 1974, pp. xxiii+428.

Shorter and Tilly (ST) have written an ambitious and vigorous book of social history on a quantitative base. They have pulled together the available data on labor conflicts over the 140 years of France's industrial history, along with a limited amount of comparative statistical material from other countries. Much of the book is descriptive in the wider sense, ST using a variety of analytical techniques, from maps to multivariate analysis, to ring the changes on their mass of information. However, the book has a definite point of view. ST argue that the strike was in essence the principal operational component in the political struggle of French workers for power. In doing so, they reject two other types of explanation, not counting "national character": one, not seriously advanced for France, that views strikes as the last, non-market resort in a system of industrial relations akin to a two-person economic game; the other, commonly argued, interpreting conflicts as near-random explosions of anger and despair by workers uprooted and trampled by the juggernaut that is industrial capitalism.

As nearly always when one deals with French history, the numerical data are a mixed bag. Consistent data for strikes, their characteristics, and their socio-economic correlates only cover the period from 1890 to 1935. Lacunae in earlier series, which ST owe to other scholars, are understandable, and the level of union and strike activity was in any case not great before the 1884 legitimation. More disconcerting are the gaps caused by the very flood of strikes in 1936 but not filled at the outbreak of war. And the postwar statistics, though as a rule very good, are unsuited to a treatment based on the individual conflict as the unit of analysis. ST have worked long and well, but the reader cannot expect full and consistent series for the long term.

From the start, ST focus on change: the growth and transformation of strike activity among French labor. And indeed, strikes have increased in frequency and size while becoming shorter. Yet one should not exaggerate the growth. Comparisons of strike rates with the 19th century using the non-agricultural labor force as the denominator are biased because of the flood of wheelwrigths, carters, shopkeepers, pedlars and servants who shared this statistical category with the real industrial population in the earlier period. For the 20th century, a simple calculation using ST's own data (p. 333) actually shows a drop in strike activity from the early decades (1900-1929) to the postwar (1946-1967) when duration is taken into account: about 35,000 strike-days/100,000 workers *vs.* about 33,000.

Given the time span covered, the durable trademarks of French strikes appear more interesting as well as more novel. The regular lack of success in achieving the stated, usually economic, strike aims forms an important plank in ST's argument that strikes can hardly have been waged for direct economic gain. From the very start to the 1968 climax, the effect, indeed the object, of striking has been to involve the government; like businessmen in the fiscal field, workers looked for help to the very state on which they heaped abuse. Finally, a look at the distribution of strikes in space and over time brings out the general and continuing meagreness of strike activity: if one removes from the total eleven strike waves and fifteen towns (admittedly including most of the large centers, though not all) very little indeed is left for three fourths of the population and a century and a quarter of history.

French strikes were political because French unions, whose "presence" in strikes was the rule, were political. As is well known the anarcho-syndicalist French workers shunned political parties, even socialistic ones. Not unlike the AFL-CIO, though for opposite reasons, they refused to commit their organization and their action to the service of bourgeois political combinations. Instead, sustained by the myth of the general strike as the essential revolutionary act, they used the strike to generate solidarity rather than to exploit it. Their motto might well have been: in strength there is union! This history may go a long way to explain the fidelity of labor in the postwar period to a party hermetically isolated from formal participation in government.

As to the determinants of strikes, the more suitable problem to address with the data, ST argue convincingly that urban life, but *not* urban growth *via* migration, encouraged strikes and widened their scope. They also make an interesting case for the effect of technology in determining the nature and workings, if not always the amount, of industrial conflict. The process passes through three stages: first, skilled artisans with a rich associational life and concern for local, job-control issues experience bitter conflicts; then, the poorly integrated workers of proletarian mass industry prove fitfully loyal to their centralized unions; finally, the educated specialists in the recent, science-based industries and utilities show many of the associational strengths and shop-level concerns found in the artisan tradition.

On the other hand, the argument that organization rather than economic interest or deprivation was the crucial determinant of strike activity seems less than convincing. For a start, it squares poorly with the interesting model ST propose for mass-production industry, that of spark-plug unionism in which a few militants can mount strikes despite the lack of union strength at the plant level. And elsewhere ST argue that strikes promoted unionization rather than the other way around, as we have already noted. Finally, the alternative economic argument is given somewhat short shrift. Failing the crude frontal assault represented by correlating annual strike rates with various aggregate economic indicators, it is largely dropped and without apparent reluctance. The discussion of strike waves is revealing: their political nature is stressed by simply ignoring relevant economic events such as the depression of 1893, the Laval stabilization policies (akin to Churchill's decade earlier) of 1934-36 and likewise those of Pinay in 1952-53, both using deflation as a means of restoring economic equilibrium. In a sense, of course, any massive strike movement is political whatever its causes; French strikes were largely political if only because such a high proportion of them was concentrated in a few waves.

The authors' preoccupation with the political dimension leads to frequent mild overinterpretation of results, both in the sense that the discussion becomes hard to follow and in that the numbers are made to speak more clearly than they willingly do. The argument also does some disservice to another stated objective of their impressive book, that of providing other interpreters with an ordered statistical picture. Intriguing and rewarding findings in the tables and figures get little or no mention in the text. To this reader, the dominant impression left by the evidence is not so much that theories of the "industrialism" type fail as that France very nearly "failed" to develop such a system. The real vigor and staying power in the labor movement was passed on by pre- and proto-industrial artisans to post-industrial technicians. It was not the "dynamic" France of the North and East that struck, but the cities of Edward Fox's "other" France along with a few unlikely places like Brittany, and of course Paris. No real system of industrial relations ever developed: employers could no more treat with mutineers than workers, on the bloody road to the millennium, could pause to bargain with the enemy.

A final note on method. Though the book bristles with technique, the critical senses should neither become paralyzed nor aroused. This is not a work of cliometrics as economic historians have come to know it, but a set of attempts to get full mileage from hard-gathered data. In general, the simpler games work best for ST, the non-mathematical or hurried reader may rest assured.

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M. Spallanzani (ed.), *La lana come materia prima. I fenomeni della sua produzione e circolazione nei secoli XIII-XVIII*, Proceedings of the «Settimane di Studio dell'Istituto Internazionale di Storia Economica "F. Datini" di Prato». Vol. 1, Florence, Leo. S. Olschki, 1974, pp. 395.

Even in an industrial age when the technicalities of mechanization and standardized production have forced us into using a similarly directed and inelegant form of language, men still turn to figurative images when they want to express briefly highly complex concepts. "Black gold" has been used to describe oil and all that is connected with it, and Bernabò-Brea has even used the phrase « the black gold of the prehistoric age » to describe obsidian. We may then be allowed to describe wool, the primary product which provided the basis for extremely long distance commerce, as the « white gold of the Middle Ages ».

Gold, silver, copper, iron, tin, timber, silk, together with foodstuffs like grain, wine, salted fish, dried fruit and sugar, were all goods that were traded often over very great distances, but during the Middle Ages none of them were ever in a position to rival wool. Not only was it a primary material universally in demand, but the various operations required to transform it into a finished product gave it an unrivalled capacity not only to create labour but also, to use a modern term, to create "added value".

Such are one's first reactions on reading this first volume of the proceedings of the "Settimana di Studio di Prato" (18-24 April 1968), which contains an introduction by Fernand Braudel and papers on wool production in Europe: in the Low Countries (A.E. Verhulst), in England (G.D. Ramsay), in the German lands (K. Blascke, H. Kellenbenz, H.C. Peyer, H. Pohl, R. Sprandel, W. von Stromer, F. Tremel), in Italy (G. Barbieri, G. de Gennaro, C. Manca, P. Racine, E. Rossini, M. Fennel Mazzaoui) in the Iberian peninsula (C. Carrere, H. Lapeyre, F. Melis, R. Pastor de Togneri, F. Ruiz Martin), in Jugoslavia and the Balkans (D. Kovacevic-Kocjic, J. Tadic), in France (E. Baratier, R. Gascon, J. Richard) and in Eastern Europe (W. Endrei, A. Maczak, A. Nahlik). The volume ends with a general essay by J.A. van Houtte.

It is a great pity that the essay by Melis (*The wools of Mediterranean Spain and Western Barbary in the 14th and 15th centuries*), which covers two extremely important sectors and was based on research in the Datini Archive, has had to be left in the form in which the late author delivered it, due to his death in December 1973, with none of the critical apparatus which he would have added had he had time to revise it.

The original contribution made by each author make this a real encyclopaedia of the production of and trade in wool, but space does not permit us to summarize each of them. I shall mention only the essay by W. Endrei which deals with the way in which from the late 17th century fine merino and Paduan sheep were reared and acclimatized in Hungary with the result that Hungary became Europe's leading supplier of raw wool and was overtaken by Australia only after 1850. I take this example because it helps relate our studies of the remote past to modern techniques of breeding and biological improvement, and, with the reference to Australian wool, reminds us that one of the outlets for wool manufacture was the English industrial revolution.

The concluding essay by van Houtte concentrates on what is now known of the production of raw wool and its international trade. There is now sufficient quantitative and qualitative information available for us to assess the size of the flocks in Europe and also to quantify the volume of raw wool produced for the manufacture of textiles. The results suggest that the international wool trade developed a certain cycle. On the other hand we know very little indeed about the wool which was turned into poor quality textiles for local consumption, although the quantity was clearly very great, given that the coarse woollen fabrics made from only partially de-greased wool were considered to be waterproof and ideal for use in winter. Quantitative gaps of this sort are all the more annoying in view of historians' current attempts to assess the volume of national gross product.

The volume as a whole gives rise to a number of general reflections, and we shall consider four which give rise to two separate questions. First, the woollen industry provides certain examples of autarchy, in the sense that certain of the textile manufacturing countries attempted to free themselves from the necessity of importing raw wool — this is the conclusion of Verhulst's article on wool in the Netherlands, for example. Secondly, throughout Europe textiles for local use were made from home-produced wool, in order to provide the coarser and poorer quality fabrics suitable for less demanding customers. Thirdly, there was not necessarily any direct connection between the production of raw wool and the manufacture of good or high quality textiles destined for export. Many countries which produced fine wool did not manufacture textiles for export, and on the other hand many countries which did not produce high quality wool imported it and manufactured cloth which was famed throughout Europe for its excellence. It was often the case then that the production of raw wool and the production of textiles occurred in different and often distant places. Fourthly, the relationship between wool and silk is generally neglected by the writers in this volume, and van Houtte alone makes fleeting reference to it along with linen and cotton (pp. 385 and 395).

The final point requires some comment. It is well known that certain woollen cloths, the Florentine cloths and certain Flemish and Perpignan cloths were handed down in wills, and in dowry inventories were listed amongst valuable as *focalia.* It is worth asking whether it was not the case that after the mid-15th century, as velvet and silken cloths and costumes became increasingly fashionable, silk began to replace wool to some extent, and to satisfy part of the demand which had previously been met by high quality woollen fabrics. While it is true that the 15th and 16th centuries wree periods of expansion so that it might be argued that silk could cater for a new source of demand without necessarily reducing the traditional demand for woollen cloths, it seems equally clear that silk met a certain portion of the luxury market thereby reducing the number of customers for high quality woollens.

This leads to a second question. Could it have been that middling quality cloths, such as certain English woollens which were of good quality but inexpensive, were successful mainly because their use spread amongst the middle and lower classes, or because the wealthier classes also used them for everyday wear, keeping silk for their finer display clothes? In the second case the democratization of woollen cloths would have been more apparent than real. It would probably be very difficult to reach a conclusion, because we are very poorly informed about the ways in which cloth was consumed. and in particular we know very little about a practice which is easier to suggest than prove, that is the extent to which second-hand, and even third-hand, clothes were used.

The exchange of English and Flemish woollen cloths for raw Sicilian and Calabrian silk, through the hands of the English and Messina merchants in both Messina and Flanders for example, is well attested. In my opinion every piece of silk cloth which came on to the market in Antwerp or London meant one less woollen cloth in demand on the London market, and so freed an extra piece for export. I admit, however, that this is no more than a hypothesis, but one which needs some study.

From the second and third points there arises another question. If it was the case that textiles, even if extremely primitive ones, were produced everywhere, how was it that certain countries which did not produce good quality wool became quality manufacturers, even though they had to import the raw wool, while others made no headway even after experimenting with imported foreign wool, and even using foreign craftsmen? This is a problem which can be seen in relation to southern Italy and Sicily in particular. There is ample evidence, for example, that up until the 1530s Messina imported not only large quantities of English, Flemish, and Spanish cloth, as well as some from Tuscany, but also "small cloths" from Naples, from Piedmont, so-called "coastal cloth" which came from the Amalfi coast, and above all certain "Neapolitan Perpignans" which were very dear and so of very good quality, probably made in imitation of the famous Perpignan cloth. In 1511 a merchant from Cosenza sent a consignment of cloths to the fairs, 251 *canne* of which were foreign (*grifoni, bergamaschi, saie*) and 348 *canne* of local production, from Amalfi or Piedimonte, and of varying quality. In 1580, however, in a Cosenza wool shop there were 372 *canne* of foreign cloths, including Flemish and English, whereas there were only about 200 *canne* of cloth produced in the Kingdom.[1] There was high quality textile production in Naples, and the question is then why was it not improved by importing finer quality wools, and why was it that it in fact declined,

[1] C. Trasselli, *Il Mercato dei panni a Messina nel '500*, Annali Fac. Economia e Commercio, Messina 1973; and the same author's "Introduction" to the new edition of Antonio Serra, *Breve Trattato*, Reggio Calabria, 1974, pp. 22-3.

so that after the end of the 16th century exports from the mainland to Sicily ceased altogether?

In Sicily coarse cloths were woven from local wools, but by the .early 16th century "monks" habits were being imported, as were the rough cloths used by the galley crews. In fact, at least four attempts were made to establish the manufacture of quality textiles (which have been studied by Pipitone, Giuffrida and myself). In the 14th century there was an attempt backed by a Genoese merchant and the city authorities of Palermo; another attempt was made in the 16th century with backing and capital from Lucca, as well as support from the authorities in Palermo (this involved importing Spanish wools, recruiting foreign craftsmen, and manufacturing "Palermo Barcellonas" — the Palazo della Panneria still survives); in the 19th century there were at least two more attempts backed by Prince Leonforte (importing wool, from Apulia) and by Baron Malvica. All had the same fate, and after a few years came to nothing.

If then there was throughout the Italian south the possibility and the capacity for manufacturing textiles, why was it that, as the importing of raw wool, from Apulia) and by Baron Malvica. All net the same fate, and finished products rather than the primary materials which might have allowed the manufacture of at least medium quality cloths to become established? Again, if we except Apulian wools which were adequate for medium quality cloths, there do not seem to have been any attempts in southern Italy to improve the breeds of sheep in order to increase their yields. In Sicily, at least, this can perhaps be explained on the grounds that the yield in meat and milk and other derivatives was already sufficient to sustain a considerable export market. It remains true, however, that in the field of woollen production one is once again confronted by a case of "failed industrialization" in the Italian south which goes back to the remote past.

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N. Steensgaard, *The Asian Trade Revolution of the Seventeenth Century* Chicago, University of Chicago Press, 1974, pp. 441, 20 tables, 2 maps, 1 graph.

The news in 1498 of Vasco da Gama at Calicut and the entry of Portugal into the Asian market fell like a stone in the mill-pond of trade. Merchants were quick to voice their alarms, and these prophecies of gloom have since left their mark on the historiography of the early maritime empires. Venice above all so it seemed, was lost. The enterprise of the Portuguese broke with the overland trade of pedlars, Arab dhows and Levantine bazaars. However correct this may be in the long run, more recent research has produced ample evidence to reject the immediate conclusion that the opening of the Cape route closed a chapter in the history of trade. Historians of note, and especially Fernand Braudel and Frederic Lane, have shown clearly that the trade in spices and silks through the Muslim world revived in the second half of the sixteenth century, and Venice, far from being depressed, enjoyed a prosperous, even prestigious phase of commercial activity.

The new and impressive study by Niels Steensgaard has taken this investigation a stage further, and merits close attention for a number of reasons. In the first place, he extends the work of Van Leur on the peddling trade, that mass of dealings by small cohorts of general traders, travelling together in caravans, coping with bandits, settling the customs and other "protection" costs. Inevitably, territorial control brought the profits of power, and trade by the land routes was subjected to levies. The Portuguese in some respects offered an alternative system, different in route, but not necessarily in character. They too hastened to claim the fruits of monopoly; but after an initial, spectacular success, failed to dominate the scene and soon succumbed to the merchandizing skills accumulated over the centuries. Towards the end of the sixteenth century, it now appears that the value of trade overland to the Mediterranean was greater than the deliveries around the Cape of Good Hope. On the side of production and supply, it is clear that the spice trade was highly diverse. While almost complete monopolies could be established in cloves and nutmegs, pepper was grown in "gardens" all over the tropics. The best came from the Malabar coast, and in many ways the Portuguese committed themselves to a structure of empire based on Goa, to the north of this area. However, monopoly in pepper apparently demanded resources far beyond those the Portuguese were either willing or able to commit. Even in the early twentieth century, attempts to "corner" this commodity failed, and it is unlikely that conditions in the sixteenth century were greatly different.

Secondly, at the turn of the century, the Portuguese enterprise was relayed successfully by the Dutch and English. Niels Steensgaard sees this achievement as the result of superior institutions, that they internalized protection costs in the sense that sea power released them from the constraints suffered by the peddling trade. From the powerful capital base of joint-stock organizations in Europe, they were able to invest in a more complex, more effective system of trade and control. Among the dedicated empire-builders, few saw the situation more clearly than the Dutch Governor-General, Jan Pietersz Coen: ample funds, more extensive than his mentors in Europe seemed willing to release, were needed to realize an inter-Asian trade, which in time came to stretch from Japan to Persia. Costs were high but it gave access to a new structure of profits: towards the end of the seventeenth century some key commodities could exchange for as much as 800 or even occasionally 1000 per cent gross profit.

The second part of the book focuses the analyses on an event, a moment of truth: the fall of the fortress and market of Hormuz in 1622. The narrative leading up to this is given in great detail, especially the intrigues of the Sherley brothers, who aimed to re-direct the silk trade of Persia. In the end, the Persians and English combined to bring about the fall of Hormuz, and with it one of the focal points of the peddling trade. But the fact remains, however, that although the mainstream of this trade was damaged, it was not entirely eliminated, for a new market developed a short distance away at Bandar Abbas/Gombroon. In the huge human mass of Asia, the companies could seize only certain strategic heights of commerce and power. As the years passed, the roots of their influence spread into territorial control, but the indigenous, multifarious trade continued. Even in the eighteenth century, the seaway to Batavia was crowded with native, Muslim and Chinese boats. It was part of an immense history which was not easily dismantled.

The study casts into relief then a double problem. On the one hand, an event, such as the fall of Hormuz, in the end confirmed the critique of *l'histoire événementielle*: it was not an isolated, independent moment of time but belonged rather to a "family" of developments. The structure was such that the companies turned from the old to re-create a new market at Gombroon. And, on the other hand, institutional history can be written into the scope of technical change. The Cape route was a significant innovation, an epoch, but it required a century and a quarter of development before reaching a stable place in the hierarchy of international transfers. The fall of Hormuz was a mile-stone; so too was the founding of Batavia, the capture of Ceylon and Malacca, and the privileged position of the Dutch in Japan, so admirably explained by Kristof Glamann. In a long and complex history, this new book opens many new perspectives and adds much much pleasure to studying the subject. It can be recommended without hesitation.

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