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## REVIEWS OF BOOKS

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*Acta Historiae Neerlandica*, volumes III, IV, V, Netherlands Committee of Sciences, Martinus Nijhoff, The Hague, 1968, 1969, 1970.

The majority of European historians have had few opportunities to keep abreast of research in Dutch and Flemish history. There is an obvious language problem. It is therefore a great boon that the Netherlands Historical Association has undertaken to publish an annual selection of articles or thesis summaries translated into French, German and especially English. The contents of the three volumes reviewed here will be of particular interest to readers because of the large amount of space that has been devoted to the fields of economic, social, and demographic history.

There is only one article devoted entirely to demographic history, but it is an important one.<sup>1</sup> Professor Hofstee's long study (1962) of population increase in the Netherlands since 1859 is found here in its entirety. This article is an extension of another influential, but hard-to-find study published twenty years ago.<sup>2</sup> The changes in the crude birth rate are first analyzed in terms of their components: the age distribution of women, the proportion

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<sup>1</sup> E. W. HOFSTEE, *Population Increase in the Netherlands*, « *Acta Historiae Neerlandica* » (« AHN »), vol. III, 1968, pp. 43-125.

<sup>2</sup> E. W. HOFSTEE, *Regionale verscheidenheid in de ontwikkeling van het aantal geboorten in Nederland in de 2de helft van de 19de eeuw*, « *Akademie-dagen gehouden te Amsterdam op 14, 15 en 16 October 1954*, vol. VII, *Verslag en voordrachten* » (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 1954), pp. 59-106. Hofstee's preoccupations and regional approach are quite similar to that shown in the recent work in European historical demography coming out of Princeton's Office of Population Research. For Belgium and Holland, see ETIENNE VAN DE WALLE, *La nuptialité en Belgique de 1816 à 1830 et sa relation avec le déclin de la fécondité*, « *Population et famille* » (Brussels), vols. 6-7, pp. 37-56; VAN DE WALLE, *Marriage and Marital Fertility*, in D. V. GLASS and ROGER REVELLE (eds.), *Population and Social Change* (London: Arnold, 1972), pp. 138-151; and the forthcoming book in English by R. Lestaeghe on Belgium.

of women who are married, and marital fertility. The thrust of the article, however, consists in an interpretation of regional differences in the crude birth rate for the eleven provinces. Hofstee tries to explain the gradual diffusion of birth control from the North and West of the Netherlands to the East and South. He examines at length the hypothesis that religious differences can explain the differing degree of resistance to modernization, but rejects the hypothesis. He does not deny the role of the declining death rate in creating incentives to limit fertility, nor does he discount the importance of changes and differences in the regional economies. In the end, however, he adopts a model of cultural diffusion, where the geographic spread of birth control is related to the diffusion of what he calls the modern-dynamic pattern of culture, opposed to the « traditionalistic » one. The fall of mortality is related to the pace of dissemination of medical care and hygiene which depends on the same cultural factor, that is, on the population being prepared to accept their benefits.

Population growth is also what Christiaan Vanderbroeke attempts to explain in his excellent study of the cultivation and consumption of the potato in Europe from its origins.<sup>3</sup> The thesis is not new, of course, that the potato facilitated population growth during the eighteenth and nineteenth centuries by permitting more nutrition to be extracted from each acre under cultivation.<sup>4</sup> But the data which are introduced — mostly from detailed research on eighteenth-century Flanders — give some quantitative bite to this notion.

There are other studies dealing with the agricultural sector. Two of them deal with South Limburg, the area around Maastricht. J. Philips draws a static picture of this area as an economically backward region in the early nineteenth century.<sup>5</sup> J. Janssen, on the other hand, tries to explain such backwardness in an interesting article showing much change and growth from 1650 until 1800 but retardation thereafter.<sup>6</sup> Emphasis is placed on the changing distribution of landholdings at the expense of a farming middle class through the development of large holdings and the fragmentation of peasant land. It is argued that it is this kind of dualism that eventually leads to agricultural retardation.

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<sup>3</sup> CHR. VANDENBROEKE, *Cultivation and Consumption of the Potato in the 17th and 18th Centuries*, « AHN », vol. V, 1971, pp. 15-139. A long and interesting review of this article is MICHEL MORINEAU, *La pomme de terre au XVIII<sup>e</sup> siècle*, « Annales E. S. C. », vol. 25, no. 6, Nov.-Dec. 1970, pp. 1767-1785.

<sup>4</sup> WILLIAM L. LANGER, *Europe's Initial Population Explosion*, « American Historical Review », vol. 69, no. 1, Oct. 1963, pp. 1-17, is a convenient summary.

<sup>5</sup> J. F. R. PHILIPS, *Die Agrarstruktur Sudlimburgs in der ersten Hälfte des 19. Jahrhunderts im Vergleich zu den angrenzenden deutschen und belgischen Lossgebieten*, « AHN », vol. IV, 1970, pp. 84-104.

<sup>6</sup> J. C. G. M. JANSSEN, *Agrarian Development and Exploitation in South Limburg in the Years 1650-1850*, « AHN », vol. V, 1971, pp. 243-270.

A short summary of J. van der Poel's book surveys the history of certain agricultural tools and cultivation methods in the Netherlands since ancient times.<sup>7</sup> H. Roessingh's study of the Veluwe (Gelderland) in the eighteenth century deals with the hierarchy of hamlet, village, and town, by measuring the frequency with which non-agricultural occupations appear in censuses, depending on the size of the settlement.<sup>8</sup> Insights are derived from this technique about the extent of commercialization and specialization in the area.

On industrial history, we find a summary of J. A. de Jonge's thesis on industrial growth in the Netherlands from 1850 to 1914.<sup>9</sup> The analysis begins with a study of the principal manufacturing sectors of the Dutch economy. An estimate is made of employment in each one. Attempts are made to relate the findings to the Rostovian take-off and leading sectors schemes, and finally to assess the role of the East India Trade. R. van Uytven narrates the introduction and disappearance of the fulling mill in the cities of medieval Belgium, in contrast to its persistence and growth in the countryside.<sup>10</sup>

Wilfrid Brulez offers an interesting article on the balance of trade of the Netherlands in the sixteenth century by critically examining Guicciardini's figures and confronting them with other estimates made by contemporaries and by economic historians of particular sectors of the economy.<sup>11</sup> He finds a deficit incurred in trade with the Baltic countries which is only partially paid for by a trade surplus with Spain, Portugal, England, and perhaps France and Germany, and the rest by the « Spanish silver that since the end of Charles V's reign was for political and military reasons pumped in increasing quantities into the Netherlands ». In conclusion, the author presents a series of calculations which he calls « sometimes acrobatic » showing that Dutch per capita imports were about 4.5 times larger than those of France or England. It is further said that about a quarter of Dutch manufacturing production was for export.

Dealing with the same period, H. van der Wee writes on the economic background of the revolt of the Southern Netherlands, which started around 1566.<sup>12</sup> He argues that improving living conditions in the sixteenth century

7 J. M. G. VAN DER POEL, *A Hundred Years Agricultural Mechanization in the Netherlands*, *ibid.*, pp. 316-325.

8 H. K. ROESSINGH, *Village and Hamlet in a Sandy Region of the Netherlands in the Middle of the 18th Century*, « AHN », vol. IV, 1970, pp. 105-129.

9 J. A. DE JONGE, *Industrial Growth in the Netherlands (1850-1914)*, « AHN », vol. V, 1971, pp. 159-212.

10 R. VAN UYTVEN, *The Fulling Mill: Dynamic of the Revolution in Industrial Attitudes*, *ibid.*, pp. 1-14.

11 W. BRULEZ, *The Balance of Trade of the Netherlands in the Middle of the 16th Century*, « AHN », vol. IV, 1970, pp. 20-48.

12 H. VAN DER WEE, *The Economy as a Factor in the Start of the Revolt in the Southern Netherlands*, « AHN », vol. V, 1971, pp. 52-67.

stimulated individualism, rationalism and sharpened the critical sensibilities of the masses. Unfortunately his evidence for improving living standards seems to relate to the urban middle classes. He is more convincing when he argues that an expansion of trading opportunities created conflicts between the indigenous merchant class and some Spanish monopoly interests. Van der Wee finally examines the short-run economic fluctuations which preceded 1566. The winter of 1564-1565 triggered a serious crisis.

Jan Craeybeckx's superb article on the Brabant Revolution of 1789 starts from R. R. Palmer's view, inspired from conventional Belgian historiography (Henri Pirenne and Suzanne Tassier) that Belgium was a backward country in the eighteenth century, where therefore progressive forces were too weak to succeed against the reaction.<sup>13</sup> Hence the failure of the Brabant Revolution. Craeybeckx critically examines the sources that were used by Suzanne Tassier and brings in the result of his own research in Belgian economic history to show that by the very end of the eighteenth century at least, Belgium was far from backward in comparison to France. An entirely new model of revolution has to be used, therefore, one where revolution is linked to popular dissatisfaction, not bourgeois frustration. The Brabant Revolution failed because the intensity of the late eighteenth century crisis was smaller than in France and the masses more prosperous.

Finally, readers of this journal may want to read the summary of Theo de Jong's dissertation and series of articles on Dutch policy vis à vis Latin America between 1780 and 1830;<sup>14</sup> Th. van Tijn's thesis summary dealing with a « social history » of Amsterdam (1850-1876) that seems to be a year-by-year narrative of events;<sup>15</sup> and H. Hardenberg's short history and description of Dutch archives.<sup>16</sup>

The quality of the translations is between among these articles but I most strongly urge readers of this *Journal* to order a subscription to *Acta* for their institutional library.

FRANKLIN MENDELS

University of Maryland

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<sup>13</sup> J. CRAEYBECKX, *The Brabant Revolution: A Conservative Revolt in a Backward Country?*, « AHN », vol. IV, 1970, pp. 49-83.

<sup>14</sup> THEO P. M. DE JONG, *Atlantis, 1780-1830*, « AHN », vol. III, 1968, pp. 189-214.

<sup>15</sup> TH. VAN TIJN, *Twenty Years in the History of Amsterdam. The Social Development of the Dutch Capital from the Mid-Nineteenth Century till 1876*, *ibid.*, pp. 215-246.

<sup>16</sup> H. HARDENBERG, *Archives in the Netherlands*, *ibid.*, pp. 266-287.

M. M. ALEXANDRESCU-DERSCA BULGARU, *Nicolae Iorga, A Romanian Historian of the Ottoman Empire*, « Bibliotheca Historica Romaniae », editions of the Academy of the Socialist Republic of Romania, Bucarest 1972, pp. 190, 1 map.

This study will join the ranks of the texts of the Byzantine historians, from Procopius to Sphrantzes, which are often inaccessible. It is a work of biographical history which in many ways provides a valuable introduction to the history of the Eastern Mediterranean in the Middle Ages and in the modern and contemporary period.

Nicolae Iorga was a man whose published works spanned half a century. He had an enormous knowledge of popular attitudes or mentalities and initiated research in France on the belated expansion of the West with his *Philip of Mézières*, which was published by the *Hautes Etudes* in 1898 and which is still an indispensable reference work. He also contributed to such highly regarded journals as the *Revue de l'Orient Latin*. His methods and the material he produced were directed to the attempt, which many researchers are still making, to establish a bridge of knowledge between East and West. His task was so great, and demanded so much knowledge of the civilizations that lay on the other side of the divide, that we shall not dwell on the possible defects and gaps in Iorga's work. However Mme. Alexandrescu-Dersca has set out to examine the origins of the undertaking and to show how vast a research apparatus was employed by this great historian. As a result, it seems to us, certain controversies can now be considered to be definitely closed.

The first of these concerns the origins of the Ottoman emirates (p. 81 *et seq.*) for example. It was not their legendary fanaticism that led the Turks to dominate the whole of Anatolia in the first half of the XVIth century. The social organization of the primitive tribes changed greatly in the direction of feudalism, and rapid population increase forced them to spread out. But, as the studies of P. Lemerle and C. Cahen have shown, this expansion towards the Mediterranean did not lead to the replacement of a Byzantine pseudo-feudalism, because it had never existed. The decline of Byzantium was caused by the economic and financial crisis in the Empire, despite the heroic attempts at restoration made by Michael Paleologus VIII and by the continuous encroachment of the Latins (Franks, Italians and Catalans). In fact it is correct (p. 96) that in the Balkans, the Ottoman conquerers of the XVth century under Murad I were establishing themselves in the remains of a world which was typically and classically feudal in the Western sense.

Secondly, regarding the years from 1416 to 1522, I am sceptical (and here the author seems to support Iorga's thesis without hesitation, pp. 110-111)

of the intransigence shown by Mehmed II and his successors towards the Latins and particularly the Venetians. Of course it is true that alum provided a good reason why they had to pay a high cost for their intrusions into the Eastern Mediterranean. But other trades, such as the slave trade, were conducted on the basis of reciprocal convenience in step with the Turkish advance. This is borne out by Aragonese documents, for example.

Thirdly, we can only agree with Iorga's thesis, on the other hand which the author takes up, of Suliman the Magnificent's attempt to establish a maritime empire (p. 117 *et seq.*). Once — this time in the Byzantine manner — an intransigent autocracy had been established over the inland towns and countryside, an intelligent attempt could be made to throw a bridge between East and West, which seemed almost to constitute a diplomatic policy of « peaceful co-existence ». At least the France of François I understood it in these terms. Even today we can still measure the importance of such an attempt.

Fourthly, the work closes with an examination of Iorga's studies of the situation at the beginning of the XXth century. This great historian's perception enabled him to assess the importance of the social innovations that were to contribute to the foundation of modern Turkey and the European Balkans and to study the decline of the traditional social groups in the Christian lands. (Janissaries - usurers of decadence p. 137) and the abandonment of the *shariat* in 1908 (p. 142) which was treated at the end of an excellent and detailed analysis.

May we venture to make some minor criticisms of such a stimulating work? There are, in fact, several slips; P. LEMERLE wrote *L'émirat d'Aydin...* in 1957 (cf. p. 170, but corrected p. 79). The name of C. Cahen, not Cahun, ought to be spelt correctly. We mention these details in a work that will be of great service to scholars and students. We should also like to draw attention to the detailed and brilliant studies by A. Rubió i LLuch, H. Ahrweiler and P. V. Laurent who knew this part of the world so well, and offer as our excuse for so doing the qualities of the book under review.

J. P. CUVILLIER  
University of Toulouse

E. D. BARBER, *The Bourgeoisie in 18th Century France*, Princeton (New Jersey, U.S.A.), Princeton University Press, 1973, pp. 165.

Despite twenty years since its original publication and a sociological approach, Elinor Barber's concise statement on the bourgeoisie in pre-Revolution France is worthy of review by a new journal concerned with the economic history of Europe. Economic historians must incorporate all analyses of

historical phenomena into their generalizations of economic historical change. Class structure, certainly an underlying determinant of economic establishment, requires consideration in so important a time period as that leading up to the French Revolution. Barber's work provides the non-sociologist economic historian with a succinct statement of sociological implications of the French class structure before the Revolution. The class structure serves to explain, or at least partially explain, the economic direction taken by France in the *ancien regime*.

The Barber thesis is simple, straightforward, plainly stated, frequently mentioned, strongly and systematically supported, and never out of sight. There is no question regarding the author's intention or direction in this easily read volume. France before the Revolution had a class system more aligned with caste requirements with those of an open society. In the ideal there were nobles and non-nobles constituting French society. Mobility between the two groups was not condoned generally though limited upward mobility did in fact exist. The system received support from both social groups, and pressure for change remained slight so long as upward mobility served to vent otherwise growing frustration in the *roturier* class. As the available avenues to nobility were closed off during the course of the 18th Century those citizens seeking higher social status were thwarted in their efforts. Although upward social mobility had been theoretically impossible, when it became so in fact the bourgeois aspiring to nobility no longer had reason to support the *status quo*.

In support of her thesis the author outlines the various levels in the bourgeoisie and discusses the paths from these levels to nobility. More important for the economic historian, however, is the explanation of the influence of possible ennoblement for the bourgeois class. Explanations abound for the laggardness of France in economic development while Britain and Holland excelled. The availability of ennoblement must be considered as important in any analysis of the retarded development of a strong and viable merchant class in France. Men successful in business financed the efforts of their sons to attain nobility through a career at law, in the military, or in the Church. A successful family business, then, lacked the continuity found in Holland or in England. Further, if successful and fortunate, a businessman might himself attain noble status, in which case he would immediately depart from the business world. In France a thriving business was a means to departure from business itself, by rising to a noble status.

Mrs. Barber provides an illuminating description of the bourgeois life styles, and the role the mode of life played in the mobility aspirations of the bourgeoisie. Further she discusses the procedures by which the bourgeois actually made the transition to nobility and the reaction of society in general, both nobility and the bourgeoisie, to such mobility. Finally, a depiction of the

18th Century reaction of the nobility to previous upward mobility is provided. An evaluation of the success of noble attempts to prevent further social elevation as well as the reaction of the bourgeoisie to that success is the central core of the presentation.

Twenty years have not dimmed the light shed by Elinor Barber's study. Although a sociological study the work is valuable to historians and economic historians as well.

DAVID O. WHITTEN  
Auburn University

J. BLUM, *Lord and Peasant in Russia from the Ninth to the Nineteenth Century*, University Press, Princeton, New Jersey, 1972, pp. X-656.

This new paperback edition of Jerome Blum's massive panorama of a millennium of Russian social and economic history should be greeted with great applause. Here again those qualities of erudition and composition shown by the author in previous works are to be found.<sup>1</sup> It is now over 40 years since Pierre Pascal wrote: « In the history of Russia there is one essential character and that is the peasant... The whole life of the country depends on him ».<sup>2</sup> But for the most part this vast rural mass remained inert except for those tumults of fury and despair which met the increasingly onerous exactions of the few tens of thousands of noble landowners who controlled all the administrative position, monopolising all the offices and exercising all the commands. It is the economic, social and psychological relations between these two, the peasant and the noble, which forms the basis of this impressive study. The whole development of serfdom is traced without neglecting the different opinions, the numerous controversies and all the various explanations of a topic which has been strongly subject to ideological pressures for some fifty years or so.

The author does not conceal his ambitions: « The reader will quickly discover, however, that this story, as it is related here, is not limited to agrarian matters alone. Much is said about political and economic affairs, too, for I have written the story of the lords and peasants against the background of Russian political and economic evolution... It took me longer to realize that there was still another external factor that was also of great significance in the evolution of agrarian institutions. The economic aspects of these institutions and the interaction between agriculture and other sector

<sup>1</sup> Cf. *Princes in Russia in the Sixteenth Century*, « Journal of Economic History », XVI, 1956; *The Rise of Serfdom in Eastern Europe*, « American Historical Review », LXIII, July 1957; *Russian Agriculture in the Last 150 Years of Serfdom*, « Agricultural History », XXXIV, 1, January 1960.

<sup>2</sup> P. PASCAL, *Le paysan dans l'histoire de Russie*, « Revue historique », 173, 1934, 1, pp. 32-79.

of the economy made it essential to study the general economic development of Russia. As these investigations continued it became clear that Russian economic life (like that of other lands) had gone through long swings of growth and decline, and that these secular trends had been of major consequence in shaping the patterns of agrarian relationships. And so the long-term fluctuations in trade, manufacturing, market demand, prices, money values and entrepreneurial activity, take their places alongside the history of political events as integral parts of the story » (pp. 3-4).

In his introduction the author stresses the importance of the environment and briefly describes the principal characteristics of the terrain, the soil and the climate. The study is then divided into four sections of uneven importance. Kievan Russia is covered in the first three chapters (p. 13-56). These concentrate on the origins of rural expansion in a largely empty country in which large seigniorial estates, then peasant communes and private property and finally the organization of the estates and of agricultural labour gradually took form and were established. The forms of this primitive rural exploitation have to be traced among traditions which were quite alien to those of the West. Even the structure of serfdom reveals original characteristics which were not to be found in the Central European countries in the same period. Blum argues that although the peasants remained free men they became more and more dependent on the landowners who became their creditors. The indebted peasant could only tie himself even more closely to his master and so little by little he lost his 'liberty'. At the same time both lay and ecclesiastical feudatories were able to free themselves from their dependence on the State by skilfully taking advantage of the exemptions which the princes were prepared gradually to concede.

The second part of the study consists of four chapters (pp. 57-116) which analyse the changes introduced during the Mongol Era, when certain specifically Asiatic customs replaced, or were superimposed on, the earlier economic, social and political structures and deeply influenced Russian legislation in later periods. The author describes the nature of the Tartar rule, which he takes as the beginning of the period of decline, the features of the large estates and the varying conditions of the groups that made up the gigantic mass of rural peasantry — a mass formed from innumerable heterogeneous groups ranging from free peasants to slaves. He describes the complex phenomenon of the spread of serfdom and attempt to relate it to economic developments for whatever economic problems there were, they always ended by favouring the richest and the strongest. From the XVth century — as in Europe as a whole — serfdom grew more burdensome with the rise of the minor and middle nobility.

Seven chapters (pp. 117-276), the third part of the study, are devoted to « The XVIth and XVIIth centuries »; in which the development of the noble-peasant relationship over this very eventful period is described. The new

conditions of Russian economic expansion are described and the author then analyses the often strained relations between absolutism and a frequently rebellious aristocracy. The period of expansion and commercialization (XVth to early XVIth centuries) was followed by an era of crisis and demographic recession culminating in the late XVIth century. The impoverished landowners began to increase their exactions from an already heavily exploited peasantry. The troubled political situation further added to this. The differences between the status of peasants, and in the seigneurs' power over the peasants in their neighbourhood, may be explained largely by the wars, the length of which varied from region to region. Slow structural changes occurred in the seigniorial economy, influenced by the import of new techniques and by the overriding necessity of defending the country against enemies in the East and West, all of which served to consolidate serfdom to such a degree that the whole life of the Russian Empire seemed to depend upon it. On the eve of the accession of Peter the Great (1689-1725) the condition of the peasantry had scarcely improved since the period of Tartar domination.

It is the fourth and final part, however, which is far and away the most important. Entitled « *The Last 150 years of Serfdom* » (pp. 277-600) it is divided into 12 chapters which stand out because of the wealth of detail and originality. The author argues that commerce and industry were a decisive influence in extending servile obligations into a number of new sectors, and whatever the economic situation the entrepreneurs always tried to increase their profits by increasing the charges born by tenants, both those paid in cash (*obrok*) and those paid in kind (*bartchina*) or else they combined these two forms of tribute in a mixed system which exploited to the full the most flexible factor — human labour. The author then examines « *The Factory Labour Force* » and the conditions of labour in « *Agriculture* ». The circumstances in which the noble reaction (« *The Dvorianstvo Ascendancy* ») took place serve as a good introduction to the range of different positions which existed within the ranks of the nobility, where birth and wealth were distinguished and formed quite separate classes.

Finally, in the five weighty chapters « *The Seigniorial Economy* », « *Masters and Serfs* », « *The Silent Obedience* », « *The non-Seigniorial Peasantry* » and « *The Village Community* » Blum shows how a nobility based almost exclusively on the land rose gradually at the cost of those who worked the land. The work ends with a survey of abolitionist theories and of the movements designed to improve the lot of the peasantry, and Blum reviews the main polemics.

To conclude, then, Blum studies serfdom mainly in its institutional aspect. He does not consider it as an indispensable element within the feudal system but argues that once the peasant was removed from the direct authority of the State he found himself under the personal control of a private landow-

ner. We cannot discuss in any detail here the other interpretation of the gradual establishment of serfdom in Russia, but it is worth noting that for Soviet historians there can be no feudal system without serfdom, itself an economic fact based on the nobles' monopoly over the land and the forced labour of the peasants. The problems arising from this thorny issue are reviewed by George B. Carson in « Recent Works on the History of Russia in the Period from the Tartars to Catherine II », *Cahiers d'histoire mondiale*, VIII, 3, 1964, pp. 548-563.

Blum's work concludes with a very full bibliography (pp. 623-645) which will be extremely valuable for those who are unfamiliar with Russian, even though the majority of primary and secondary sources are given in Russian. The glossary, the general index, the numerous tables and three maps make this an indispensable source for the student of the Russian economy and society. One should also mention that this fine study goes beyond the purely Russian context in the frequent and interesting comparisons which the author draws with other European countries. For the most interesting critical appraisal of the work, however, the reader is directed to the article by Alexander Gerschenkron which first appeared in the March 1964 issues of the *Journal of Economic History* and has since been republished in *Continuity in History and other Essays*, Cambridge, Mass., 1968, pp. 428-436.

FRANÇOIS JEQUIER  
University of Lausanne

F. CROUZET (ed.), *Capital Formation in the Industrial Revolution*, London, Methuen & Co., 1972, pp. IX-261.

Capital plays a leading role in the various interpretations of the « Industrial Revolution » and its abundance in England in the 18th century often seems to provide the main explanation for its early industrialization. The aim of this very useful book is to bring together some of the main articles on this subject which appeared between 1935 and 1969 often in journals which are not easily accessible. The collection has a preface by Peter Mathias, editor of the series « Debates in Economic History », and is followed by an important introduction by the editor, sub-titled « An Essay in Historiography » (p. 1-69).

Professor Crouzet starts by reminding us that while capital has been a central preoccupation amongst economists since the time of Adam Smith, the first historians of the « industrial revolution » were not greatly concerned by the problem of capital formation. They were mainly interested in the institutional, technical and social aspects of the « Industrial Revolution » rather than in the economic problems it posed. With the aid of extensive footnotes covering the main contributions (generally monographs in periodicals) the

author provides a vivid picture of the historiographical development of the highly controversial problem of capital formation during the Industrial Revolution. He illustrates the common features of the main hypotheses, which he also discusses, stressing the influences to which respective authors were subject, and finally he summarizes the principal arguments advanced against them. Professor Crouzet has provided us, then, with a very sound survey of all these largely contradictory theories around which disagreements as to which factors in particular contributed to the genesis of capital have centred for years. The very detailed analysis covers the points at which historians and economists meet, and allows the author to provide a general picture which must be considered the finest general appraisal of the state of the debate. It is impossible to summarize all the different topics treated in this masterly introduction. Only by reading it can one appreciate the richness and subtlety of this great scholar of the financial and economic aspects of the « Industrial Revolution ».

The articles which Professor Crouzet has used are ordered chronologically as they were published. The first serious discussion of the problem of capital formation in an historical context employing economic concepts was the article by M. M. POSTAN, published in 1935 — *Recent Trends in the Accumulation of Capital* (p. 70-83, first published in the « Economic History Review » VI, No. 1, October 1935). In the passages concerning the Industrial Revolution, Postan stressed the imperfections of the capital market which prevented the recently created enterprises drawing on the financial assistance of rich landowners and wealthy merchants. Postan preferred the concept of a « multiplicity of small disjointed pools » to the notion of a « general reservoir of national savings ». He argued that « Industry and agriculture each had a capital market of its own, and within industry almost every industry was restricted to its own private supplies. The Industrial Revolution got under way while capital was not yet capable of moving between its alternative employments » (p. 74). Postan also stressed the individual character of capital in the 18th century and while he believed that industry suffered from a shortage of capital, he also noted: « But on the whole the insufficiency of capital was local rather than general, and social rather than material ». (p. 71).

Before looking for the origin of capital, Herbert Heaton set himself the following question in his article *Financing the Industrial Revolution* (p. 84-93) first published in the « Bulletin of the Business History Society » XI, No. 1, February 1937): « How much was needed by those who built up enterprises with the new machines and power generators housed in mills or factories? ». And he gave a detailed reply to this, using studies based on the archives of those new enterprises. Professor Crouzet summarizes Heaton's conclusions in the following words: « Herbert Heaton pointed out that the initial capital requirements for eighteenth-century would-be factory masters were very modest, especially as far as fixed capital was concerned, and that a variety of

sources were tapped to supply them, with family ties playing a predominant part » (p. 5).

The editor also analyses the work of Phyllis Deane, comparing it with the work of other economic historians, and emphasizes the importance of her article *Capital Formation in Britain before the Railway Age* (pp. 94-118, first published in « Economic Development and Cultural Change » IX, No. 3, 1961).

The study was « the first quantitative study of capital formation during the British industrial revolution. She did not attempt to make estimates of aggregate capital formation, but carefully pieced together, sector by sector, the available fragments of evidence, both qualitative and quantitative, about the additions which were made to the nation's capital during the eighteenth and the early nineteenth centuries, and compared them with the growth of population and national income. She stressed that most of her data, drawn from published material, were inadequate, and her conclusions necessarily tentative » (p. 14).

Two articles by Sidney Pollard tackle the problem of the composition of capital. In the first *Capital Accounting in the Industrial Revolution* (pp. 119-144, first published in the « Yorkshire Bulletin of Economic and Social Research » XV, No. 2, November 1963) he pointed out that, during the Industrial Revolution, there was no general « rationalization » of accounting, as postulated by the « classical » scheme of W. Sombart and Max Weber, in order to fit fixed capital into it. The analysis of textbooks of accountancy and of actual accounts of firms reveals little change from the methods which had crystallized in the sixteenth century. There was an enormous variety of practices in the counting houses, but no clean-cut attempt to adapt them to the notion of capital as generalized, de-personalized property, seeking the highest returns. In fact the concept of capital was little understood; it was treated as an auxiliary to entrepreneurship, instead of the central motive force behind the firm, and it was confused with revenue. The notion of capital as a permanent, autonomous factor was virtually unknown ».

In his second article, *Fixed Capital in the Industrial Revolution in Britain* (pp. 145-161, first published in the « Journal of Economic History » XXIV, No. 3, September 1964) Sidney Pollard shows clearly how circulating capital was of far greater importance than fixed capital, even in the case of the metallurgical industries. The cotton-spinning mills were the only concerns where fixed capital became the major component — just a little more than half the capital invested ». Professor Crouzet compares Pollard's calculation with those in more recent studies and provides the necessary modification of the figures in which the ratio of fixed to circulating capital is estimated, a relationship which has been at the heart of so many of the theories of economic inheritance.

In publishing his own contribution in English *Capital Formation in Great Britain during the Industrial Revolution* (pp. 162-222, first published in « Second International Conference of Economic History », Aix-en-Provence

1962, vol. II, Paris - The Hague 1965) Professor Crouzet has been able to bring his impressive array of footnotes up to date to take account of more recent studies. In this essay of general synthesis, the author stresses « the predominance of " internal " financing of industry. Most firms were started with a small initial capital, which had been accumulated through pre-factory system manufacturing or merchant-manufacturing activities, or in the trading of industrial raw materials or finished articles; and they were expanded thanks to the steady ploughing back of the large profits which were often possible. Altogether retained profits were seen as the greatest single source of long-term capital, and the role played by « external » capital, supplied by landowners, overseas traders, banks etc., was rather played down. The Industrial Revolution would have involved the creation of new savings and new outlets to absorb them, rather than the tapping of traditional types of savings. « Now some ten years later the author is ready to reconsider the problem of the financing of industry in the light of more recent studies on the structure of capital ».

B. L. ANDERSON, *The Attorney and the Early Capital Market in Lancashire* (pp. 223-255, first published as chapter 3 of J. R. HARRIS (ed.), « Liverpool and Merseyside: Essays in the Economic and Social History of the Port and its Hinterland », London 1969) concentrates on the « mortgage market » in a local context. « The practice of mortgaging was very widespread in Lancashire (being made easier by the large number of small landowners), and the habit of borrowing and lending had penetrated all classes of society quite early on, involving a complex pattern of capital movements between individuals and trades. Of course the specific uses to which funds raised by mortgages were put are not generally known and it is quite possible that they were mainly used for consumption, and also for house-building. None the less, the existence and activity of this rudimentary but flexible capital market, the mobilization of capital funds (especially rural savings) and the large facilities for borrowing which resulted from it, cannot have been unimportant for agricultural and industrial capital formation, and for the economic growth of Lancashire », Professor Crouzet states in his introduction to Anderson's article.

A wide range of problems related to capital formation during the industrial revolution still remains unresolved, and economic historians remain sceptical « about the unique role capital was formerly supposed to have played in the beginnings of modern economic growth ».

A selective bibliography ends this collection, which provides an outstanding guide to the subject. But the richness of the book is mainly due to its French editor, and to the English general editor of the series (« Debates in Economic History ») which well deserves its title.

FRANÇOIS JEQUIER  
University of Lausanne

G. DORIA, *Investimenti e sviluppo economico a Genova alla vigilia della prima guerra mondiale*. Vol. I: *Le premesse (1815-1882)* and Vol. II: *(1883-1914)*, Milan, Giuffrè, 1969 and 1973, pp. xii+400 and xii+776.

Four years elapsed between the publication of the first and second volumes of this work. The first volume was intended to show the historical antecedents of the main features of the Genoese economy in the thirty years before the First World War. The work is completed by the second volume, which is entirely devoted to the latter period and so offers the reader an overall picture, the product of an extremely demanding piece of research.

In these pages Genoa is seen as the subject of economic decisions or as a springboard for enterprises abroad, rather than as an autonomous unit, the features and structure of which might be described. The city, acting like the wings of a stage, delimited the boundaries of a certain area within which there was an economic force that dominated the interior of the city perimeters and which also spread outside, although with less marked effects and within different limits than a century earlier. This economic force, the subject of the study, is 'capital', that is a mass of wealth which moved from one economic sector to another, in the continuous search either for income or for security, dissolving traditional assets, encouraging new activity and always deeply affecting everything with which it came into contact.

What Doria was seeking, then, was a very elusive character which moved in unknown paths but which left traces of itself and its movements in a massive range of documents, many of which have scarcely or never been used. The author has followed his prey through the reports of the English, French and German legations and consulates, some of which were still unpublished, and the *Foglio annunzi legali* of the province of Genoa, which contains a mine of information on the official life of local companies. He also consulted numerous invaluable ministerial publications which have been neglected until now. They range from the « Bulletin » of the accounts of credit institutions to those of the joint stock company, from reports on the public debt to the proceedings of official inquiries into the conditions of industry and shipping, from the specialist newspapers to the economic and financial sections of the most important Italian journals.

In addition to the bibliography, against which only one complaint can be made — the lack of a systematic index of the works quoted which would be of use to anyone who wished to gain a deeper knowledge of individual problems — a number of manuscript sources are quoted which were tracked down with difficulty in local archives, in the company archives which were deposited with the Tribunals of Genoa, Milan and Rome (a source Pantaleoni consulted with profit for his study of the *Credito mobiliare italiano* and which are now, in some cities, in a state of extreme neglect) in the archive of the Banca d'Italia and in the state archives of Rome, Paris, London and Bonn.

Doria has turned this immense amount of material to good effect and succeeded in reconstructing the elusive career of capital in Genoa for over a century. At times this is seen in relation to the progress of foreign capital which from time to time poured into the city, or else rushed from it to other havens; at others it is seen in terms of the broader national and international framework within which capital operated. And since it was men who moved the wealth about, the entrepreneurial element, with its ambitions, capacities and deficiencies, always receives the greatest attention.

It is enough to glance at the lists of the companies and operators which close the two volumes to find there the names of all those who were most important in the economic life of Genoa in that century (and, indeed, in this). The shareholdings of the most important groups between 1883 and 1914 are traced in one of the valuable appendices to the second volume in order to outline the fields in which such men as Raggio, Bombrini, Tassara, Piaggio, Ravano, Bruzzo etc. participated.

However it is obvious that a study that centred on individual entrepreneurs would have made it impossible to assess the full range within which they selected their activity, had they not been seen against the background of more general movements that could be placed on a macro-economic level. This is what determined the particular structure of the work. The affairs of individuals are studied against the general transfer of capital from one sector to another, which is deduced from comparisons made at regular intervals of traffic in the port, the tonnage registered, industrial and building activity, the stock market, occupational distribution of the active population and the number of public companies.

In this way the author draws a detailed picture of the ebb and flow of capital which, like a rushing stream, at times spread out into shipbuilding and maritime trade, only to abandon them in difficult times and stagnate for a while in building or stocks and shares or securities. At one moment it moved out of state bonds to be engulfed in some industrial enterprise, and at another disappeared from Genoa only to re-emerge hundreds of kilometers away, in Sardinia, on the banks of the Plate, or along the routes for Russian grain.

The description of the transfer of capital in quantitative terms has meant solving a series of rather complex problems, ranging from the criteria by which to value ships, buildings, shareholdings and other forms of investment to the correct interpretation of available statistics. Therefore some of the author's conclusions regarding the intensity of these transfers are of course open to discussion. For example, the fall in sea trade with foreign countries after 1861 is, at least to some extent, only apparent, because trade with the territories of the ex-states, which were considered as part of Sardinian foreign trade before Unification, was excluded from official statistics. Besides, it appears doubtful to me that the loan of 644 million for the

abolition of the *corso forzoso* could have resuscitated the policy of increasing state expenditure, for it was almost entirely used to withdraw an equal amount of unconvertible banknotes. The operation thus brought about an increase in the interest payment budgeted against the Treasury. The standstill in dividends paid through the Genoese treasury agency appears illusory if we bear in mind the conversion of consols from 5% to 3.5% which was ordered in 1906.

If, then, the author's claims regarding the quality and intensity of transfers of capital in certain medium-term periods (or, in other words, in a Juglar perspective) need some correction in certain details, there can be no doubt that the picture drawn by Doria remains valid especially for long-term structural changes.

From this analysis there emerges a convincing picture of the gradual process by which capital was accumulated. Its strength after the Restoration lay in trade (but also in the 100 million public bonds which had survived the revolutionary storm, with the regular and secure flow of interest now restored). In the 1850's Cavour's enlightened policy, which aimed at reconciling the reluctant middle class of the ancient republic to Piedmont through merging their respective economic interests, facilitated the first explosion of Genoese capital into new fields (engineering industry, shipping lines, Sardinian mines) with investments finding their way into building, the textile industry and trade, thanks also to the free trade policy.

For more than thirty years Genoese investments developed in different economic sectors, enthusiasm for shipbuilding alternating with that for the manufacturing industry, for building speculation and for the stock market, whose respective and close relations are effectively described. At the end of the 1880's the essential equilibrium that had dominated the various alternatives was shattered, either because of the desperate crisis in the building of sailing ships which threatened one of the pillars in the process of accumulation, or because of the turn towards protectionism or else because of the new standards of bank management which emerged after the 1890-95 crisis and the invasion of German capital.

The last years of the XIXth century thus saw a vast pouring out of resources and a greater concentration of capital in the industrial sector. In the Giolittian period this strange situation facilitated the building up of holding companies based on engineering, iron and steel, sugar refining, cereal milling and building which eventually dominated the entire life of the city with cartels and monopolies.

The crisis that followed in 1907-8 brought about the disappearance of many of the minor companies which had formed part of the local economic network and had given it new stimulus. It left a clear field for the bigger companies which were incurring great debts in following the trend towards massive concentration and strengthened the role of French and English, to

the detriment of German capital. The growing dependence of Genoese industries on orders from the state, which was particularly marked in the shipbuilding sector after the Libyan war and the later impetuous development of engineering, derived from this as did the Genoese oligarchy's unveiled support for interventionism of which its own newspapers became the mouth-piece. This situation also leads the author to describe, in a way which links those years with our own, the limits of an entrepreneurial attitude which, while neglecting light industry and aiming at heavy industry above all, would have eventually tied the fate of the city to state intervention and condemned it to stagnation today.

Doria's study reveals new problems, then, like all stimulating and intelligent works; but it solves many others which are fundamental to the understanding of Genoese economic and social life. It would be enough to think of the successive pages devoted to Cavour's achievements, the description of the stratagems followed by the local middle class in making the most of the opportunities of the time and the description of the mixture of public action and private choices. In short, it is an original analysis based on an exemplary methodology which has produced many tangible results.

GIUSEPPE FELLONI  
University of Genoa

R. C. FRIED, *Planning the Eternal City*, Roman Politics and Planning Since World War II, Yale University Press, New Haven, 1973, pp. XVII, 346.

The modern planning of Rome can be seen in terms of the desperate plight of the great and beautiful city being destroyed by automobiles, smog and a spreading hinterland of inhuman, high-density sprawl. Or it can be seen in terms of the response of the planners, of their 15-year effort to establish a guiding plan for the city, responsive to problems and establishing modern standards for late 20th Century Rome. Or finally, it can be seen as the frustration and defeat of the plan, partly through the bankruptcy of the municipal government, partly through the partisan rivalries and selfishness of Roman politics, and partly through the very vitality of the city itself whose seething development during the past two decades has outstripped all efforts at guidance and control.

Robert Fried in *Planning the Eternal City* gives us an informed perspective on each of these views. His deep understanding, disciplined approach and readable style bring the incredibly complex picture into clear focus. It is a book that will be valued by the planner for its constructive criticism and discussion of Rome's excellent plan. It is a book, as well, that will be read with fascination, if often with despair, by anyone who knows and loves the great city of Rome.

Fried's purpose is to describe Roman city planning and the social, historic and political culture in which it operates, and to analyse and evaluate the performance of the Roman planners. The book opens with a chapter entitled « Judging and Evaluating Planning Performance ». Fried posits six criteria against which the achievement of the Roman planners is to be evaluated: (1) « planning style » or the adequacy and sophistication of methodology; (2) « planning power » or the ability of the planners to influence the real world in the direction of their goals and policies; (3) « planning commitment » or the financial and staff support provided to the planners by their government; (4) « environmental quality » or the amenity and liability of the environment which the planners produce; (5) « goal achievement » or the degree to which the actual city development conforms to the predictions and policies of the plan; and (6) « time costs » or the ability to produce and adopt a plan within a reasonable period of time. This model contains the essential values by which city planning performance should be judged and Fried finds Roman planning wanting on nearly every score.

The main body of Fried's book is a study and analysis of the historic, political, economic and social context within which Roman government and planning operate. It is a systematic, thorough and informed presentation of the Italian, and particularly the Roman situation. Fried has made copious use of original source in government documents, literature, and the press. To these basic data Fried brings his understanding of Italian culture and his skill in relating the Italian social and political ways to our own experience and understanding. We learn of Rome's location between the under-developed south and the affluent north, and of the political jealousies which have prevented the national government from providing either the financial means or the legal powers to enable Rome to manage its own affairs.

There is a lucid and thorough discussion of the Italian political party system with its rivalries and polarizations. Fried stresses the ideological differences among the parties as an obstacle to coalition and action, and points out that this national political characteristic is exacerbated in Rome, both because Rome is the seat of party organization, and also because the two polar parties, the Communists and the Neo-fascists, hold unusually large shares of the Roman electorate.

Rome's economic troubles are vividly presented. We see a city overburdened with poor and unskilled immigrants from the south, and lacking a strong economic base to provide jobs for its people and taxes for its operation. Fried stresses the fact that building and land development is the largest industry in Rome and the principal employer of the unskilled poor. The strategic role of the building industry in the city's economy results in virtually irresistible political opposition to effective control over land development and building standards. The weakness of the basic economy is matched by a weakness in municipal finances; as Fried says, « The city had to borrow not

only to pay for capital investment, but to pay the salaries of city employees. The cost of providing services and infrastructure for the city's booming population — even at very inadequate levels — had simply exhausted the public economy ».

Although he provides a good summary of the earlier history of planning efforts in Rome, Fried is essentially concerned with the contemporary city plan. He describes the arduous process by which the current (1962) plan was developed, beginning with the decision to produce a new general plan in 1951, and including two abortive plans along the way. This process formally ended with the approval of the plan by the national ministry of public works in 1965. This 15-year span for the creation and adoption of a new plan accounts for the low rating of the Roman planners in Fried's criterion of « time costs ».

The more important objectives of the 1962 plan are: to preserve the quality and vitality of the historic centre through careful control of the scale and nature of activity permitted there, while directing new growth to new development centres to the south and east; to provide adequate public transit including a subway, and to take other means to save historic centre from traffic strangulation; to establish modern standards for public services and public open space in the new quarters; and to encourage reasonably compact development and avoid «leapfrogging» in the «agro-Romano», the rural land surrounding the city. Each of these objectives has been achieved to some degree, but the accomplishment has been painfully small compared to the magnitude of the need.

One reason why actual development in Rome bears so little relation to the plan is the weakness and complacency of the enforcement authorities, as against the strength and irresponsibility of the building industry. Fried suggests several times that the plan fails because it makes unrealistic and unfair demands on landowners and builders. No doubt this is the way the requirements are viewed, but I cannot agree with Fried that the plans are unrealistic or inequitable. It is a well-established principle in the United States and in many European countries that the developer who converts land from rural to urban uses should provide the necessary infrastructure and improvements to meet the requirements of the new use. The profits to be gained from such conversion are nearly always such that the investment in facilities, services and open space can be made without impairing an adequate and even ample profit. Nevertheless, many Roman developers simply ignore the plan's building standards, leaving the new neighbourhoods without sewers, drainage, schools, open space or other requirements. The city must then remedy the deficiencies at public expense, or more often, given the virtual bankruptcy of the public budget, the neighbourhood goes without such services and facilities. To reduce the plan's requirements might lead to a higher degree of compliance, but certainly would not result in an improved environment.

The alternative appears to be a change in public attitudes to give more support to public enforcement and less acceptance of anarchic action. Such a change, I must agree with Fried, will not come easily or quickly. There are, however, recent developments which give some promise of a new attitude. « Italia Nostra », an association devoted to conservation and environmental quality, is making the more educated and sophisticated Italians aware of the costs of inadequate planning and enforcement. Italia Nostra provides a focus for political pressure and action.

Among the lower classes, who are hardest hit by the over-crowding, poor transportation and lack of services, anger and frustration have recently found expression in activist organizations at the neighbourhood scale called « comitati di quartieri ». These committees employ the poor peoples' weapons of direct political and economic action such as barricading streets to provide play space, or withholding rents for defective and over-priced quarters. Such actions have brought home to the political leaders of Rome and other Italian cities the real meaning of their city plans and accompanying land development standards.

*Planning the Eternal City* concludes with a review of current developments such as the creation of a regional level of government, and the move to decentralize the municipal government of Rome in a populist direction with the establishment of twelve district councils. Reviewing these and other developments, Fried concludes his candid but rather gloomy study on a cautiously optimistic note: « Supported from above with regional financial and political support, and spurred from below by district expectations and demands, the city may find the impetus it needs to realize the ambitious goals set forth in its new master plan. And even partial achievement of these goals should allow a Roman to say once again with pride: « Civis Romanus sum ».

CORWIN R. MOCINE  
University of California, Los Angeles

E. S. HIGGS (ed.), *Papers in Economic Prehistory*, University Press, Cambridge, England, 1972.

This is a set of initial papers by members and associates of the British Academy Major Research Project in the Early History of Agriculture. At best, it is a step in the right direction, even if the step is rather faltering, the scope extremely limited, and the view myopic. I hope future papers will give more recognition to the fact that many plants and animals were domesticated in areas other than the Near East, that considerable investigation has been done in the early history of agriculture, particularly in the New World, and that there is a large body of data, some of it in print (and not in the bibliography of this volume) on theory, method and techniques, and

field and case studies that would greatly supplement the rather limited preliminary efforts presented in this volume.

The first section of this volume concerns theory, and the most useful of the three papers is that by Higgs and Vita-Finzi on the territorial approach or « catchment analysis » of artifactual and ecofactual materials from sites involved in early Palestine agriculture. Webb's article on soils and site locations in the final section of the book also pertains to much the same subject. As recognized by the authors, the methods outlined « are capable of considerable refinement and elaboration ». It is my opinion that one way that such refinement may come about is a recognition of the works of others who have used a territorial approach. Although they footnote the works of Flannery (1965) as well as Hole, Flannery and Neely (1969) on this subject for the Near East, I believe their type of study might benefit from a recognition of Coe and Flannery's 1964 article on Micro-environments and Meso-American Prehistory series including Volume I entitled Environment and Subsistence, edited by D. S. Byers, and published by the University of Texas Press in 1966; Flannery's article on Archaeological Systems theory and Early Meso-America in *Anthropological Archaeology in the Americas*, edited by Meggers, 1968, as well as his 1967 science article on Oaxaca written with N. V. and M. J. Kirkby and Aubrey Williams; Struever's and other works on Hopewellian Interaction Spheres, published in 1964 by the Illinois State Museum; Zubrow's studies on Carrying Capacity in the prehistoric Southwest; and papers by Patterson, myself, and others on Peru, as well as many, many more.

Higgs and Jarman's article in this section on the origins of animal and plant husbandry is again extremely limited in its theoretical scope although it is well written. However, the idea that agriculture is not essentially an invention or inventions, but a continuously developing natural process of great selective value in which adjustments to climatic and other ecological factors which are visible up to the present-day, and the recommendation that more archaeological resources should be devoted to the study of the economic mechanism rather than just a search for origins, are viable theoretical ideas. However, the lack of consideration of seasonality and the scheduling factor as important economic mechanisms involved in the origins of animal and plant husbandry makes their essay as well as Jarman's on the origin of wheat and barley cultivation seem rather unsophisticated.

I believe archaeologists will find the second section of the book more useful, particularly, those articles on froth flotation and sieving, for these are well-explained techniques that may prove useful in the field. Payne's essay « On Interpretation of Bone Samples from Archaeological Sites » also has some good technical advice, but I find his recommendations on sampling without recourse to statistical techniques and his plea for an integrated approach when many archaeologists working on early domestications are

using an interdisciplinary approach, both rather superficial. Jarman and Wilkinson's article on « Criteria of Animal Domestication » zeros in on a crucial aspects of origins. We, in highland Peru, with our many bones of camelids and guinea pigs, as well as other animals, are also much concerned with this problem. His recommendation that the zoological studies of animal domesticates could be integrated profitably with aspects of ethnology and ecology which are directly relevant to archaeological interests is apt. However, I would recommend that the authors read the works of, or get in touch with, Elizabeth Wing, Kent Flannery, Nathaniel Rutter, Barbara Pickersgill, and others who are doing just this sort of thing in Highland Peru. Legge's essay on cave climates, although interesting, is not quite so germane to problems of method and techniques. His well-documented conclusions that some cave climates may offer advantages as a focus of human settlement which compare well with other kinds of habitations is well taken. I remember well my few weeks of living in the dark, damp, cold inns of Scotland and England just after a season in the warm, dry, spacious caves of Mexico.

The final section on « Field and Case Studies » contains much useful information for archaeologists interested in animal domestications. Wilkinson's article about current experimental domestication and Sturdy's article on Greenland reindeer economy are good cases in point. Wilkinson's opinion that more emphasis should be given to objectives, success and failures by early-man economies « rather than the means by which these ends were achieved » seem reasonable. Legge's article on prehistoric gazelles in Palestine and Jarman's study of prehistoric European deer economies are well-documented and have information that should be useful to those studying prehistoric faunal remains. Dennel also has useful thoughts and criticisms concerning the interpretation of plant remains obtained from archaeological sites in Bulgaria and the Near East. Also of value are the techniques he used to obtain these plant remains from three sites in Bulgaria. The techniques utilized by French in his excavations at Can Hasan III, 1969-1970 for obtaining not only plant and animal remains but even artifacts are also valuable. Payne's article supplements French's by giving dates and more information on these plant remains from Turkey.

The conclusion of the book is an overly brief statement on the biology of domestication by I. B. Hutchinson. It has some sage comments on early studies of domestications, the early diffusions of some domesticates in the old world and « on experimental studies of the genetic systems through which the evolutions of crop plants and domesticated animals came about ». It is unfortunate that he did not elaborate further on all three of these very pertinent themes.

However, in spite of my criticisms of this volume, it does have much useful information and I recommend it for those interested in not only

the early history of agriculture but for archaeologists, anthropologists, economic botanists, and zoologists in general. In addition, it is a well-edited volume that comes in handsome format with good illustrations and well-constructed charts and tables.

RICHARD S. MACNEISH

R. S. Peabody Foundation for Archaeology,  
Amherst, Massachusetts

W. KALTENSTADLER, « *Experimentum sociale* » *Studien zur Wirtschaft und Gesellschaftsentwicklung Siziliens*, Munich, Verlag UNI-Druck, 1972, pp. 86, 12 tables; *Migration und Integration von Gastarbeitern. Auswirkungen auf die heimische Wirtschafts- und Sozialentwicklung*, Augsburg s. d., pp. 54, 3 maps.

This double sociographical study establishes, as it should, a diagnosis. The question of policy and of future prospects, however — the fundamental problem in Sicily of the nature of the activities of the *Cassa per il Mezzogiorno* which has directed and organized public investment since 1950 — is not considered. This basic problem might however be summarized as follows. If it is true that emigration really is only « a waiting room for integration into the indigenous labour market » or if it were true that the re-integration of the emigrants « costs » only 25 years' effort, then the evil would be less than if such praiseworthy initiatives had expanded in parallel with, and perhaps because of, a migratory trend which was looked on essentially as a safety valve. The solution to the problems created by emigration must depend on government action.

Central to these studies then is the figure of the emigrant worker — the *Wanderarbeitnehmer* or job prospector — in the same sense as that seen by such religious and welfare organizations as *Caritas*, *Unitas*, and *ONARMO*. This perspective is maintained by the in-depth local surveys and inquiries carried out in both Sicily and Germany. Dr Kaltenstadler's team of Bavarian researchers also use the work done by G. L. Monticelli, A. Perotti and F. Schinzingler whose findings are by and large reconfirmed here.

The distribution of the workers in the various Sicilian occupational sectors at once illustrates the whole fragility of an uncertain environment for which there is never compensation. The agrarian structure is typified by both large and small property which block progress equally. In short, in detail this is essentially Dolci's analysis of the south as a world of closed and unbreakable structures. It is also clear that the agricultural labourers, who exceed the national average by 8.7%, constitute an enormous weight on the island's economy and that the 11% *salarinati fissi* in the primary sector of Enna, or the 2% of the same category in Agira, offer no prospect of escape from a rigid and fragmented system.

The second study provides a picture of a quite different world — the Germany of the immigrant workers around 1960-65. The drama is revealed by the data on their distribution. 43.2% of all Italian workers stayed in Germany for less than one year, 28.8% for between 1 and 3 years, 17.2% for 3 to 5 years while only 10.8% stayed longer than 5 years. Behind these figures lies the lack of real qualifications, of status, of schooling and of « adaptation » (this being the most typical complaint of those questioned). Also underlying them is the fact of the remittances sent back to Italy, amounting to 3,892.5 million dollars between 1947 and 1963. Where they settled is even more important. In 1965 the majority were to be found in the periphery of the zones of heavy and engineering industry, or in the less concentrated industrial areas of Bavaria — that is in the unskilled or semi-skilled sectors.

It is here that the belief in the « waiting-room » principle begins to grow bleak. If, following F. Schnizinger, one considers that the delays in industrialization are due mainly to the absence of « pre-industrialization », then it becomes clear that emigration is anything but a solution. The lack of professional training and education abroad tend to ruin *a priori* the efforts made in the island itself. Here investments (which in the short term are important, necessary and rational) are designed to fill the gaps but not to overcome the decisive blockage against the re-integration of the emigrants who have now become, whether or not it is desired, completely Europeanized and often over-qualified for the type of employment open to them on their return. It is an ironical outcome, in view of the traditional preference of public organizations which invest either in whatever needs preserving or whatever is rentable. The emigrant Sicilians constitute between 40 and 60% of the Italians working abroad, and 15 to 30% of them were left in search of work (*Arbeitsuchende*) after the years 1958 and 1962. On returning to Sicily all too often they concluded that *la pioggia piove sul bagnato* ('the rain falls on those who are already soaked').

J. P. CUVILLIER

University of Toulouse

H. KARL, *Krisenmanager im Dritten Reich: 6 Jahre Frieden-6 Jahre Krieg. Erinnerungen*, Droste Verlag, Düsseldorf, 1973, pp. 532.

Hans Kehrl, the scion of a Brandenburg textile manufacturing family, was in his thirties when Hitler came to power in 1933. His sympathies for the Nazi cause quickly led to his becoming involved with the administration of the *Gau* Kurmark. From there he gravitated to the Ministry of Economics where he came into close association with those men who were shaping the economic policies of the Third Reich. Once war had begun, Kehrl was given a breathtaking array of assignments. In German-occupied Poland, he was

instrumental in setting up a huge textile factory at Lodz for the manufacture of synthetic fibres. The urgency with which the plant was built reflected his opinion that German acquisition of natural fibres, except for limited quantities of hemp from Italy, wool from Turkey, and possibly cotton from Egypt, and that by way of Turkey, would be very minimal, and, hence, the country would need to rely on synthetics. His success at Lodz brought new assignments as a trouble-shooter for the Ministry of Economics in the Netherlands, Belgium, and France in spring and summer, 1940.

Upon his return to Berlin in the autumn, however, Kehrl was taken aback by the wave of euphoria that had swept over the Ministry, which, despite the war, was planning a programme of both butter and guns. He was also shocked to learn that Walter Funk no longer had any real contact with Hitler and that economic planning, such as it was, seemed to be proceeding *in vacuo* (pp. 202-203). But, Kehrl was even more distressed to discover that in the Ministries of Finance and Interior the situation was pretty much the same. In consequence, he sought out Fritz Todt, whom Hitler had recently put in charge of armaments and munitions, and apprised him of his findings. Todt was non-committal, asserting that his own appointment dealt specifically with armaments, and, thus, he was not empowered to act as coordinator of the entire war economy (pp. 218-219).

The first winter following the invasion of Russia demonstrated amply the validity of Kehrl's criticisms. The procurement agency of the *Wehrmacht* had planned badly, and even worse, in some cases, had planned not at all. For example, no effort had been made to consult with the Finns about clothing and weapons. In consequence, German troops suffered cruelly and the incidence of frostbite was nearly disastrous. It was the pathetic condition of the troops that prompted Kehrl to perform yet another of his organizational miracles, the manufacture of winter garb made from synthetic cloth that had only recently been developed by the textile industry.

When Todt died in an airplane crash early in 1942, Hitler appointed Albert Speer to the post. Inasmuch as Speer, like Kehrl, was of the opinion that the war effort could only be properly directed from above, he established a central planning board in which Kehrl began increasingly to play a prominent role in the controlling of raw materials and the allocation of resources. Although Kehrl continued to perform feats of organizational legerdemain in his work with the Ministry of Economics and the Central Planning Board, the magnitude of the problem of keeping up production became ever greater with the start of the relentless Allied air raids. Indeed, by late autumn and early winter, 1944, the war effort was becoming unstuck. With the beginning of the new year, therefore, Kehrl and Speer were busy developing strategems whereby the *Führerbefehl* for a scorched earth policy was blunted. Both determined that the resources of the country, such as they were, must be preserved for postwar civilian needs.

Although these memoirs are urbanely written, indeed, one might even say stylishly written, they lack moral indignation about the outrages committed by the regime. Kehrl seemed to have been so caught up in his work that he did not pause to think of what was happening round him. The boycott of Jewish merchants and professional people had occurred in the spring of 1933, and thereafter the insidious process that led to the destruction of the *Rechtsstaat* had reached a highpoint in September, 1935 with the proclamation of the Nuremberg laws. Yet Kehrl attempts to justify his own attitude by explaining that at least in the economic sector official anti-Jewish measures had by 1938 still not reached « an intensive stage » (p. 140). In the Ministry of Economics there was a *Judenreferat*, but since his own dealings with it were infrequent he claims to have remained unaware of anti-Jewish activities, including the aryanization of property. Indeed, he was of the opinion that the situation could be resolved eventually by the emigration of German Jewry to Palestine and elsewhere in the world. But the pogrom associated with the *Reichskristallnacht* seemed to have shattered his optimistic estimate of the eventual fate of German Jewry, for he was « completely unprepared » (p. 141) for this act of frightfulness. Nevertheless, he persuaded himself that it had been implemented by lunatic fringe groups within the Nazi party; that Hitler had had a hand in it Kehrl could not believe.

In these remembrances of times past, too, the reader will not find answers to questions concerning the relationship to German industry and the overall war economy of SS enterprises located at concentration and death camps, nor will he find much information, beyond a few generalities, about the role of foreign workers who were carried off to Germany after 1940 to man the industrial apparatus. Somewhat puzzling as well is the fact that despite his self-appointed role as gadfly of the bureaucracy, Kehrl never did gravitate, not even to the periphery, of the resistance movement. Under the circumstances, what could have been more natural than the establishment of contacts with Carl Goerdeler, who after all did play an active role in economic affairs in the early years of the regime.

Finally, Kehrl leaves much unsaid about his association with Speer. At best, it must have been formally correct. Did Speer regard as a threat to his own position the *Verantwortungsfreudigkeit* of Kehrl? Although the index of this book contains more than sixty references to Speer, the reader of Speer's memoirs will find no reference made to Kehrl!

WERNER BRAATZ  
University of Wisconsin-Oshkosh

E. KING, *Peterborough Abbey, 1086-1310: A Study in the Land Market* ('Cambridge Studies in Economic History'), Cambridge, at The University Press, 1973.

Dr. King's book is one of several studies of medieval English monastic finances and administration that have appeared recently; mention of some others could include R. B. Dobson's, *Durham Priory*, and the study of *Bolton Priory* by Ian Kershaw. These volumes follow in, without necessarily ranking with, the distinguished tradition established by the works of such eminent scholars as Du Boulay, Finberg, Miller, Page, Raftis, and Smith, to offer only an incomplete list.

As the subtitle suggests, King's focus is upon the operation of the market in land, within and beyond the Abbey's territory, and upon the creation of large estates, primarily during the twelfth and thirteenth centuries. The approach is that of family history, detailing as far as the sources will allow the history of the acquisition and use of property by various families in the region. Carrying this even further, King asserts « The monastic community was itself a family, and it is not entirely fanciful to look at the abbey's history as that of a family » (p. 168). True, not entirely fanciful, but the analogy ought not to be pushed too far, for a monastery was in several important respects quite a different corporate body from an ordinary family. It had a special character and status and, in particular, it was relatively immune from many of the problems of alienation and of inter-generational transfers of landed property, of the type which so often afflicted secular families, and which King so ably delineates in the first part of his book.

The book possesses a number of virtues. Such, for example, is the author's well-placed emphasis upon the impact of geographical factors on the economic evolution of the estates (pp. 141-142). A similar relationship is elucidated by the author, between the chronological sequence in the Abbey's endowment and the articulation of its economy. Indeed, the reader is left tantalized about the possibility of some kind of discernible 'life-cycle' in the patterns of pious donations (pp. 9, 11). Again to his credit, King is appropriately cautious in handling the ambiguities inherent in the original documents, and in dealing with the difficulties of individual identification occasioned by the nomenclature applied in the records to holders and holdings. He is perceptive in emphasizing the importance of the Norman settlement, and of the phases in Norman feudalism for the developing land market (pp. 18f, 52-53). He also brings out, in the chapter on « The Administration of the Estate », the significance of the managerial revolution involved in the transition from « feudal » to « professional » land steward (p. 126f).

The book has the structure of a series of essays, of which some, inevitably, will be of greater general interest to economic historians than will others. Those which are especially likely to reward the attention of professionals are

the sixth, dealing with Peterborough's unique *Carte Nativorum*, first dilated upon in so stimulating a manner by M. M. Postan; and the eighth, treating « The Organisation of the Estate » (and Appendix B), representing the volume's most systematic use of quantitative data, and which is most informative on the workings of the agricultural economy.

King's book abounds in rich detail. It is equipped with a useful bibliography and index. In addition, some informative documents are printed as appendices (though some others seem to be accessible only in translation). It is, in short, a thoroughly scholarly volume. One should not come to it seeking much in the way of broad generality and generalizations. This is not a work of synthesis: it is rather the stuff of which syntheses are made.

RICHARD ROEHL  
Bowdoin College

M. KRANZBERG-W. M. DAVENPORT (eds.), *Technology and Culture: An Anthology*, New York, Schocken Books, 1972, pp. 364, \$ 10,00.

Despite technology's central role in modern history, historians have long hung back from it, inhibited by their own technological inexpertness and perhaps also by a feeling that its history lacks the intellectual dignity of political, legal, social, or cultural history. Technologists, for their part, have generally not been historians. And so it was as recently as 1958 that the Society for the History of Technology was at last organized and soon after began publishing its quarterly, *Technology and Culture*. Melvin Kranzberg, editor of that journal then and since, defined its mission at the start in an article that now serves admirably to introduce this anthology: the journal, like the society, was established « to promote the scholarly study of the history of technology, to show the relations between technology and other elements of culture, and to make those elements of knowledge available and comprehensible to the educated citizen » (p. 18). The present collection is evidence of substantial success thus far in that mission.

The twenty-one articles here reprinted were published in *Technology and Culture* from 1960 to 1968, twelve of them in the years 1960-1962. Fifteen were first given as papers, presumably benefiting from knowledgeable comment even before submission to the journal. The editors of this collection, Kranzberg and William M. Davenport, have divided it into four sections: technology and society, technology and the humanities, man and machines, and invention and innovation, each introduced by a commentary on the general theme and the individual pieces.

Categories so broad cannot avoid some cloudy intermingling — are « society », « the humanities », and « man » easily separable? In each of the first three groups, the first two or three essays, discuss one or another

major theme and are therefore more readily classifiable. The rest deal with particular cases and so could as plausibly fall under one heading as another. The editors' natural urge to categorize does not lessen the value of the selections, and the introductory comments are stimulating and perceptive in their own right. Still this reader, at least, tends in his own mind to reclassify the articles simply as thinkpieces and case studies.

The interpretative approach is dominant in the section « Technology and Society ». Robert L. Heilbroner sees technology as shaping history within quite wide bounds, demanding as its price certain broad characteristics of labour force and organization, but susceptible itself to shaping, conscious or otherwise, by society. Thus the social environment of the eighteenth and nineteenth centuries, when capitalism was strong and relatively unfettered by government or social mores, especially encouraged the development of technology. Peter F. Drucker takes a similar view, using as his example the rise of « irrigation cities » — the « first technological revolution » — in ancient times, and pointing out that the social and political requirements of irrigation did not rule out significant variations in temper and values among different irrigation civilizations. Lewis Mumford warns of the modern threat to democracy from the hierarchical requirements of technology, and he urges, consistent with the analysis of Heilbroner and Drucker, that values be consciously reordered by society to liberate and enhance the individual, even at some sacrifice of homogenized affluence.

In that section also, Lord Ritchie-Calder, in a rather miscellaneous discourse, comments on the social and political consequences of early agriculture and irrigation, then goes on to emphasize the possible usefulness of past technologies to emerging nations, as opposed to spectacular but inappropriate modern technology. John B. Rae considers the influence of American general culture on American technology and finds it to have tended toward pragmatic « know-how » rather than toward knowing why. On the other hand, Rae points out, technological differences between industries led to differences in their organization — again, a two-way street. Finally, John G. Burke, in a resonant essay, discusses a precedent-setting case of federal shaping of technology through safety regulations for steam boilers, slowly developed in the teeth of *laissez-faire* and self-interested outcries between 1824 and 1852.

In the section « Technology and the Humanities », Aldous Huxley calls for saving humanism and the environment from the consequences of heedless technological change and charges literature with a responsibility to deal with technology as with other aspects of the human condition — an essay that could easily have been interchanged with Mumford's in the scheme of organization. Scott Buchanan bewails the dehumanization of technology, once ranked among the arts, and suggests that the process be studied for clues as to how rehumanizing might be accomplished. Two English researchers,

J. B. Poole and R. Reed, recount their painstaking and ingenious deductions, through the study of leather and parchment technology, of the identity and religious ideas of the group that produced the Dead Sea Scrolls. And Carl Condit, in an eloquent essay widely reprinted since its first appearance, gives an example of the rehumanization of technology, or at least its transmutation into art, in the architecture of Louis Sullivan.

In the section « Men and Machines », another article by Drucker recommends that technology be defined in terms of work patterns, organization, and attitudes, rather than tools, and speculates that such an approach might best bring coherence to technological history. Drucker, however, gives no concrete example of how that approach would significantly alter current writing. Mumford calls again for redressing the balance between technology and humanism: technology has been too much taken as the measure of progress; it has drifted too far from its earlier life-centred spirit, its brotherhood with art, to a materialistic obsession with work and power, beginning with the first « megamachine », the work-force of the pyramids; it has counter-balanced production with wartime destruction. The remaining articles in this section, excellent and interesting as they are, would fit as well into one or more of the other sections. C. St. C. Davison persuasively challenges, by technical and historical analysis, the notion that wheels evolved from rollers. Lynn White, Jr., gives credence to the story that an eleventh-century English monk, Eilmer of Malmesbury, flew more than two hundred yards in a glider. In an account of the breeding of tomatoes to suit a projected harvesting machine, Wayne D. Rasmussen shows how a multiform or package approach can solve a technological problem.

The final section, « Invention and Innovation », consists entirely of case studies, most of them appropriate in some degree to other section also. Lynn White, Jr., gives eleven brief examples in support of his suggestion that « innovation » may be no single process but a variety of processes quite different in different cases. F. M. Scherer traces the invention and introduction of Watt's steam engine as a test of the distinction between invention as analyzed by Abbott Payson Usher and innovation as analyzed by Joseph Schumpeter. He finds the two concepts distinguishable, argues that innovation has been undervalued as against invention, and makes a persuasive case for Matthew Boulton's importance in bringing the age of steam to birth and, more tentatively, for the British patent system as a stimulus. Robert S. Woodbury thoroughly undermines Eli Whitney's pedestal as creator of the « American system » of mass production through specialized, powered machinery and the principle of interchangeable parts. Norman B. Wilkinson shows how much the industries of Brandywine Creek in the early nineteenth century owed to French and British technology. And Reynold M. Wik concludes with an account of Henry Ford's energetic, diverse, but often premature efforts to revolutionize American agriculture through science and technology.

Some of the thinkpieces in this collection fall into the error that a couple of them warn against: the unconscious imputing to technology, which is after all only a variety of human activity, an entity and — worse still — a will of its own. Notwithstanding that, and quibbles about the logic of their arrangements, the selections are high in interest and quality. It is to be hoped that many « educated citizens » will discover from them how « available and comprehensible » the history of technology can be.

ROBERT V. BRUCE  
Boston University

E. LE ROY LADURIE, *Le territoire de l'historien*, Gallimard, Paris, 1973, pp. 542.

In the past historians followed the movements of armies and carefully weighed up the consequences of victories and defeats. They examined how diplomats drew and redrew state frontiers, how they seized economic privileges or colonies, how they developed wise balances of military or diplomatic forces. For such historians their « territory » was the battlefield or the council tables of the negotiators.

This is a path, however, which no serious historian would consider following today. In Pierre Norat's words history has « broken loose » and flowed over into preserves which formerly were either ignored or considered to be of only secondary importance — as for example economics and demography, themselves relatively privileged subjects, and also such fields as climate, diet and *mentalités*. This expansion should be seen in conjunction with a new conception of historical time, for such themes can only be studied against a considerable time scale, that is *la très longue durée* — and it has of course found in L. Febvre, F. Braudel and the *Annales* school in general both its theoreticians and its leaders. The multiplication of new interests could only have occurred through parallel interest in the other social sciences or, as the cases of dietetics and medicine illustrate, even of the sciences as a whole, as it was from these that both new methods of inquiry and a new vision of social systems was derived. Recently for example Nathan Watchel has applied the methods of structural anthropology to South American societies of the XVIth century with exceptional success (see *La vision des vaincus*, Paris 1971).

The articles which E. Le Roy Ladurie has collected together under the title « Le territoire de l'historien » take us into both the new and the traditional corners of this expanding history, from rural and demographic history to anthropological studies (the height of French conscripts) and to the history of climate. Each article represents a real discovery which the economic historian will find significant and interesting. There is no space here to mention

all the articles individually, but we shall attempt to isolate certain common methodological principles.

History's gaze is no longer restricted to the ruling classes, but is directed above all to the masses, to the « ordinary man » who, in the long term, really « makes » history. As a result, statistics become the historian's principal and most necessary tool (see *La révolution quantitative*). A fact becomes significant only when it is repeated, and its historical significance depends very much on the scale of repetition. Hence the computer (*L'Historien et l'ordinateur*) alone is capable of organizing the huge volume of material required for these statistical studies. The articles discussing the ways of using the military archives and the movement of rents in Paris provide conclusive examples of the way in which computers may be employed. Certainly, contemporary accounts (*La verdure du bocage*) and impressionistic descriptions may provide interesting information and even suggest new problems, and it is also true that the quantitative method contains certain dangers, in particular that of over-hasty extrapolation (see *Les comptes fantastiques de Gregory King*). But it is no less true that statistics still remain the essential basis for any sound and serious study. One might even consider whether the study of *mentalités*, of attitudes, which has today become so popular, should not be submitted to more quantitative rigours, following the examples set by M. Vovelle (*Chaunu, Lebrun, Vovelle: une nouvelle histoire de la mort*) who has succeeded in tracing the concept of death in Provence right through the XVIIIth century by transferring thousands of wills on to an index system. In this case, however, the danger lies in the possibility of the study of attitudes becoming simply the traditional « history of ideas » at the level of the masses, with all its vagueness and impressionism of method and its dependence on the subjective character of the writer rather than on factual reality.

To conclude, however, it must be stressed that expansion of interest has not meant dispersion. While the exploration of new historical fields requires the development of new techniques and brings new categories of conclusions, both techniques and conclusions must relate to a more general whole. The study of climate concerns the historian only because it may, finally, explain the sequence of good and bad harvests, with their respective demographic and social implications... were this not so, then the study of climate would have no place in the historian's preserve. To broaden the field of historical research does not mean to fragment it into a series of researches deprived of all common links. Each historical factor has both a specific weight and also influences all other factors in equal proportion and it must then be studied in relation to these other factors. The author of the « *paysans de Languedoc* » has been able to demonstrate this more clearly than anyone — and has also remained true to these principles.

GERARD DELILLE  
French School, Rome

R. PIKE, *Aristocrats and Traders: Sevillian Society in the Sixteenth Century*, Cornell University Press, Ithaca and London, 1972, pp. xiii+243.

Ruth Pike's description of Seville in the sixteenth century reveals a society full of conflicting norms and inter-group tensions. It was a traditionally hierarchical « aristocratically dominated society », in which manual labour was degrading and artisans « were fully conscious of their inferior position » (pp. 130-131). Into this city flowed suddenly the wealth of America; and, in pursuit of it, immigrants swelled the population from 55,000 in 1534 to over 100,000 in the 1580's. New riches and new people undermined traditional class barriers and produced an « acquisitive society », where « dissatisfaction with social and economic status became the common affliction of all Sevillians » (p. 21). The way to satisfy this « greed for money » was to stick a thumb in the transatlantic trade. Titled aristocrats and newly ennobled *hidalgos* found no stigma in engaging in commerce, while even artisans invested in goods for sale in the colonies. Pike, who earlier studied the role of Genoese merchants in this commerce, now finds that by mid-century enterprising Spaniards had replaced them. In her picture, trade with the Indies becomes almost a vast cooperative effort of Sevillian society, not unlike the more familiar Dutch colonial trade of later date. She directly « challenges the oft-repeated assumptions about the Spanish lack of aptitude for business and the total abandonment of trade by the descendants of wealthy and ennobled merchants for the aristocratic way of life » (p. 128).

It is a weakness of Pike's analysis that she does not appear aware of the structural conflict between the older hierarchical norms and the new acquisitive society. The result is that contradictions in the society appear at times as inconsistencies in her own account. She uses the traditional structure as the basis for her presentation: a series of descriptions of social classes descending from titled aristocrats and *hidalgos* through clergy, the professions, and merchants (all these in one chapter called « The Elite » that is more than half the book), to working classes, unassimilated classes (Moriscos and slaves), and criminals. There is not much imagination in this organization, and virtually no concern for the fact that information on the elite comes mostly from the first half of the century and on the lower classes from the second half. The strength of her research, however, compensates in part. She has used notarial and municipal archives in Seville, and national collections in Madrid and Simancas. She has also drawn with profit on much recent work by other historians. The section on Negro slaves is particularly impressive for the range of sources used.

Running through this hierarchy Pike finds people on the make. They were everywhere, except among the Moriscos, who were successfully segregated, and the titled aristocrats, who had made it. Even black slaves could become freedmen and respectable citizens. The people whom Pike finds most successfully breaking through the social barriers and gaining control of the new acqui-

sitive society were the *conversos*, or descendants of converted Jews. Based on the traditional strength of their forbears in certain crafts and professions, they moved into positions of wealth and power. Some were transatlantic merchants and others artisans who speculated in colonial commerce. They bought their way out of merchant status and became leading *hidalgo* families, with places on the city council. They were found in the upper levels of the clergy, especially in the cathedral chapter, and they took over control of the new university. In both the latter cases they circumvented legal statutes requiring purity of blood. Pike's study will force a reassessment of the accepted picture of Spanish *conversos* suffering from persecution and discrimination.

Yet these *conversos* were operating in an openly hostile society « dominated by the doctrine of *limpieza de sangre* » (p. 100). How did they manage this achievement? Pike's answer is partly by support of powerful aristocrats, but more by a combination of guile and clannishness. They hid their Jewish background, lived apart by their own choice, and practised intermarriage, nepotism, and business solidarity. In this way, she concludes, they took over Seville. « With the stigma of their origins carefully hidden under false genealogies and their lives and ideas patterned along the lines of the official ideology of religious orthodoxy and *limpieza de sangre*, these aristocrats and traders directed the destinies of their city, and it is to them that Seville owed its period of greatness and prosperity in the sixteenth century » (p. 214).

Here is almost a new conspiracy thesis in Spanish history, with the Seville *conversos* taking a place beside the Jesuits, freemasons, and Opus Dei. I am not convinced. There is no comparative look at the Old Christian elite, and hence no demonstration that intermarriage, nepotism, and group solidarity were uniquely *converso* practices. Pike successfully sniffs out their Jewish blood, mostly by tracing family names and marriage alliances — a task which engages her best research talent — and in apparent contradiction, she assumes that their contemporaries did too. Two Basque merchants who married *conversas* probably did so, she says, because as newcomers they were « not aware of the *converso* background of these families » (p. 97). Her attribution of so much power to the *conversos* also comes as a surprise. They were a minority of the *hidalgos*, upper clergy, and merchants; and titled aristocrats (who were apparently not *conversos*) controlled the city government and the transatlantic ships. But even if there was no *converso* plot or take over, they were upwardly mobile in a caste society, the leaders of the new acquisitive culture. To comprehend them better one needs some reference to Weber, or Sombart, or more recent writers on the economic role of religious and ethnic minorities. Because of her careful research Pike tells us much that is new about sixteenth-century Spain, but she also demonstrates the weakness of social history written without concern for the theories and methods of the social sciences.

RICHARD HERR

University of California, Berkeley

J. THIRSK-J. P. COOPER (Eds.), *Seventeenth-Century Economic Documents*, Clarendon Press, Oxford, 1972, pp. XXI, 849.

In a historical work it is not perhaps prudent explicitly to adopt as a model a format and a *modus operandi* that were successful in another historical work a half-century ago. To do so is likely to suggest a certain deficiency in the spirit of adventure and to invite invidious comparison with one's predecessors. The New Cambridge Modern History, for example, incurred both liabilities, and its unhappy passage through the critical gauntlet is too recent to have been forgotten — especially by its sub-editors.

J. P. Cooper was editor of a volume in the new CMH, but that has not deterred him from joining Joan Thirsk in editing *Seventeenth-Century Economic Documents*. Like *Tudor Economic Documents*, edited by R.H. Tawney and Eileen Power fifty years ago, the recent volume is the result of the collaboration of editors, one of each sex. That likeness, to be sure, is mere chance. The remaining similarities of the two works are not. They are as much the result of the explicit intention of Thirsk and Cooper as similarity between the new CMH and the old CMH is the result of the intention of G. N. Clark, the chief editor of the former.

We may be glad that the editors of *Seventeenth-Century Economic Documents* took the risk of imitation and comparison; their work does not have the drawbacks and is not subject to strictures suffered by the new CMH. *Tudor Economic Documents* is a fine collection to which, as Thirsk says in her preface to the new volume, « the remarkable growth of studies in the economic history of Tudor England owes much of its stimulus (p. v.) ». In almost every respect *Seventeenth-Century Economic Documents* is as good as the work on which it was modelled; in some respects it is better.

First, the Thirsk and Cooper single volume is physically more convenient and easier to handle than the three-volume Tawney and Power set. Getting all the documents into one binding has not required the editors to cut back the quantity of material printed, if my rough calculations are near the mark, to the contrary. Although Thirsk and Cooper is almost 400 pages shorter than Tawney and Power, a somewhat wider and longer page may have given it 50,000 to 100,000 more words. The organization of the documents is also more convenient by virtue of being simpler and more coherent. In the older work each volume has a title (Agriculture and Industry; Commerce, Finance, and the Poor Law; Pamphlets, Memoranda, and Literary Extracts). Under these rubrics are 18 subheadings, and under these subheadings the documents are arranged in eighteen separate chronological runs. Moreover, since the contents of the third volume are distinguished from those of the other two not by subject matter but by form (pamphlets, memoranda and literary extracts), documents on the same subject matter (e.g. enclosure) appear in two different volumes and therefore two separate chrono-

logical runs. Indeed by employing the subheading « Industry and Trade » in Volume III, Tawney and Power collapsed into a single intermingled run of literary sources two subjects dealt with separately in the first two volumes. Finally, a considerable number of memoranda, supposed to be in Volume III, actually appear in the first two volumes.

Thirsk and Cooper have eliminated these varieties of awkwardness. They have put all the documents, regardless of kind, under eight subject headings (Economic Crises; Agriculture; Industries; Inland and Coastal Trades and Communications; Overseas Trade; Finance and Coinage; Aliens; Wealth, Population, and Land), arranging them chronologically under each heading. They have thus made it possible to follow the flow over time of both action and opinion on their chosen subjects without requiring the reader to look in two places for it. Their procedure thwarts only the reader who wants to examine the concurrent views and actions of Englishmen in a number of economic areas; but of course there is no way simultaneously to maximize the utility of both a topical and a purely chronological arrangement of documents. Finally, the editors have modernized spelling, punctuation, and capitalization thus sparing the reader the gratuitous freakishness of seventeenth-century « orthography ». So much for the form and arrangement of *Seventeenth-Century Economic Documents*.

Substantially the selection of document is excellent. Thirsk and Cooper have sought and found a solid mass of documentation in all sorts of places: central government archives, county and town record offices, library manuscript collections, and seventeenth-century writings and records, both those published at the time and those published since by individual editors and by national, regional, and local societies for the publication of historical records. In general, *Seventeenth-Century Economic Documents* seems to contain an even richer variety of material than did *Tudor Economic Documents*. This is probably the result of several different circumstances that are exemplified by three sections of the new work — one, on agriculture, paralleling sections in its precursor; two, on economic crises and wealth and population, new to the Thirsk and Cooper book.

The most marked contrast between the sections on agriculture (T & P 1:1-90; 3:12-81; T & C 106-187) results from the consistent divergence in the concerns to which the respective documents draw attention. Of forty-one documents in Tawney and Power all but three of four bear on the terms of tenancy, their main concern resting squarely on the issues of enclosure and depopulation. Only one, an extract from Tusser's rhyming *Five Hundred Pointes of Good Husbandry*, attends to the question of agricultural productivity. In the collection on the seventeenth century, however, less than a third of the relevant documents are concerned at all with the terms of tenancy, and of those dealing with enclosure, more favour it than oppose it. About five-sixths of the remaining pieces, more than half of the total, deal

with « improvement », improvement, that is, not in the sixteenth-century sense of « methods of raising rents » but in the eighteenth-century sense of « methods of raising better crops » or improving productivity. And the sources are not only general treatises on farming but local observations on current practices in agriculture and animal husbandry. Unless the selection of documents in both volumes is extraordinarily biased, it suggests, as Eric Kerridge has it, that high farming in England did not wait on the « Agricultural Revolution » of the eighteenth century.

In Thirsk and Cooper's eighth section on « Wealth, Population and Land: Some Contemporary Statistics », we come upon an invention of the seventeenth century, an obsession of the twentieth — « Political Arithmetick », as William Petty called it, statistical demography, as we might say. There is none of this in *Tudor Economic Documents*; there were no documents of this kind for Tawney and Power to gather. Thirsk and Cooper print a selection from the earliest precursor of statistical demography, Wilson's *The State of England, 1600*, and follow with selections from the later seventeenth-century galaxy of quantifiers — Petty, Davenant, and King — omitting the founding father, John Graunt.

Why a section of Economic Crises is absent from *Tudor Economic Documents* and present in *Seventeenth-Century Economic Documents* is not clear. A change in the habits of thought of economic historians in the past fifty years may have something to do with it. In the nineteen-twenties most economic historians, certainly Tawney, saw the movement of economic history over the very, very long run: from a natural economy and « feudal » society to and through individualist capitalism to whatever form of collectivism the particular historian currently fancied. The short-term and intermediate-term ups and downs of the economy did not capture their attention. For better or worse that situation had changed by the time Thirsk and Cooper made their document collection. Economists had become aware that economic dips of less than secular duration could have violent impact on an economy where many lived at the margin of subsistence. For the first seventy-five years of the seventeenth century, documents on economic crises in Thirsk and Cooper fall into six unevenly spaced chronological clusters, two to six years long — economic history seen, as it were, from the roller-coaster.

Tawney and Power, on the other hand, scattered under many different headings the documents suggestive of economic dislocation in the Tudor period, so one gets no sense of their chronological density or rarity over time. Whether *Tudor Economic Documents* actually includes evidence of short-run economic crises one could only find out by reshuffling the documents themselves. Whether regardless of the outcome of such shuffling there were indeed such crises in Tudor England, one could only decide by searching out evidence that Tawney and Power were not looking for.

Summarily then the documentation of Thirsk and Cooper may be richer and more varied than that of Tawney and Power partly because people in the seventeenth century were conscious of and self-conscious about economic matters that people in the Tudor period were not, partly because economic historians in the 1970's are aware of kinds of economic change that economic historians in the 1920's were unaware of or indifferent to.

The two excellent document collections we have been examining share a common defect. They draw on an extraordinary range of sources, but they seem to overlook entirely one most significant source. Using reports of the prerogative courts from Star Chamber to the Duchy of Lancaster, of quarter sessions and assizes, they both disregard the reports of the central common law courts. Yet these sources are easily accessible in the 166 volumes of the *English Reports*, and the centuries covered by the two books of documents are precisely those in which the common law established its permanent dominance over all its possible rivals. They are also the centuries in which, for example, the common law undertook the protection of copyhold, assigned the right to subsoil resources to the owners of the land under which they lay, and provided remedy at law to holders of endorsed commercial paper — all matters of considerable economic moment. Perhaps the best illustration of my point here is the case of *Darcy vs. Alleyn*. It is the leading case at common law on the vexing economic problems created by patents of monopoly. Both economic history source books concern themselves with monopolies. There are no less than three different law reports of *Darcy vs. Alleyn*. The case was decided in 1600, so by odd chance is appropriate for inclusion both in *Tudor Economic Documents* and in *Seventeenth-Century Economic Documents*. Yet neither Tawney and Power nor Thirsk and Cooper include it.

There are other minor matters at which one could carp, for example, the general idea that it is particularly desirable « to view » the seventeenth century not merely as a time span that involved continuities as well as disjunctions but « as a unified whole ». Whatever that may be. But why carp in the face of such over-all excellence? Tawney and Power was a tough act to follow. In *Seventeenth-Century Economic Documents* Thirsk and Cooper have followed it with success and éclat.

J. H. HEXTER  
Yale University

