
REVIEWS OF BOOKS

E. ASHTOR, *A Social and Economic History of the Near East in the Middle Ages*, London: Collins, 1976, pp. 384.

The subject of this book is the classical Muslim Near East, the one which was shaped by the Arab conquests, and so both Byzantium and the Ottoman state are excluded. It is a history which is, in so many ways, quite new, and which provides a picture of the Near East which is different from the one we are accustomed to. It is a lively picture, pieced together from an abundance of mainly unedited sources, which have been utilized with a sharp awareness of the nature of the problems and with all the depth of analysis which only a historian who knows his craft perfectly is capable. In Eliyahu Ashtor excellent scholarship is accompanied by an openness of mind and an honesty which prevents any risk of an external approach which would play down certain motifs peculiar to Islamic society.

The study is in open polemic with the traditional literature which depicts an essentially static society in the area and period, a society resigned to its inequalities. Instead, its tensions and ferments are incisively revealed. Indeed, already during the Umayyad caliphate, the division between the variously privileged classes and a large number of "second class citizens" stands out so sharply that to the frequent, isolated explosions of malcontent, authentic revolutionary movements are added, which produced clearly defined programmes for a new social order. Social tensions and conflicts are a recurring feature of the following centuries, leading us to ask why they never succeeded in changing things. In the case of the rising of the Zindj, this was due to the lack of support from other social groups who

also remained hostile for religious and racial reasons. Later on, Ashtor argues, the spirit of orthodoxy often served to push aside such assaults, which tended to mix with conspiratorial and sectarian movements.

These conflicts are shown as a living texture of Arab-Muslim society, and the author does not allow himself to be deceived by the Arab chroniclers — whose works he has, however, used extensively — into writing a history of the aristocracy and great princes, which when it does not condemn them, practically ignores the aspirations of the lower classes. Keeping to his theme, Ashtor has written an economic and social history, and therefore emphasizes primarily the elements able to give life to it without neglecting the other phenomena which cut across this. His main analysis is concerned with socio-economic stratification factors, with demographic trends, prices and wages, monetary circulation and industrial and artisan productions. He succeeds in commanding the complex subject with ease and his eye for particulars never leads him to neglect the overall general picture, which is drawn comprehensively and coherently. Much of the data is conjectural, and often necessarily approximative, so that graphs, such as the one on page 317 illustrating the movement of bread prices in Iraq and Egypt between 700 and 1450, may give rise to some doubt. As an indication of trends, however, they are certainly valuable, and even if not comparable with contemporary statistics — which goes without saying — they can be used with confidence to avoid vague generalization and also as a starting point for further discussion and comparison.

The author's concern to establish accurate and precise information is very evident in his study of processes of population growth in the different regions, in which the scarcity of direct evidence is carefully compensated for, by the examination of the behaviour of other variables. Demographic expansion in the Abbasids period is thus suggested by the rise in grain prices, which were pushed up further by the increase in circulation of gold currency. On the other hand, the demographic contraction of the Seldjukid period is inseparable from a slow but continuous fall in cereal prices, which cannot be explained by the increase in production because agriculture was in full crisis. Ashtor also describes the sanitary conditions, the tragic cycles of dearth and epidemic, the malnutrition of the lower class due to the imbalance between the abundance of sugar and the lack of proteins, and the squalor of the urban centres. The turning point in the Egyptian demographic trend came after the middle of the XIth century, when the centuries-long expansion changed into headlong decline, as the numerous deserted villages go to show. Everything suggests that Syria may have experienced the same process.

Four of the book's eight chapters make explicit reference, in the titles, to feudalism, which was directly connected with military institutions and spread throughout the area after the collapse of the caliphate. They provide a sound understanding of the system, both in terms of its constituent elements,

its functioning and its interdependence with other branches of the economy. Ashtor examines the feudal system not only as a form of landownership, but also as a factor which provided historical continuity, and as a characteristic trait of the socio-economic structures of the Near East. But one of the bases of the book is the circulation of money, studied from the sources from which metal was obtained, the weight and alloy of the coin, inflationary facts and the relationship with price levels. Contrary to what has often been held, Ashtor argues that the linking of the dinar and the ducat in 1425 constituted a devaluation, enforced by the contraction of gold stocks resulting from the direct communication and contacts established between the Western Sudan and the Genoese and Portuguese. The excess of copper currency reflects the financial problems of the last Mamluk kings. Although he is well equipped technically, Ashtor's interests are not those of a numismatist. He deals confidently with the problems of a money economy, and for instance he shows the connections between the prosperity of the merchants and workers in the Ayyubid period and the return to a bimetallic system after the crisis of the kingdom of Saladin.

The money economy was based on the cities, which were numerous and often very large. It was here that the bourgeoisie operated, an autonomous class which Ashtor defines according to economic activities. He gives great importance to it, and its situation often provides one of the best indicators of the quality of the successive political regimes. He examines the feature which distinguished the bourgeoisie from the other social groups and he outlines its articulations, its ups and downs. Its golden age was under the Fatimids, because their economic policy was based on free enterprise, but the situation became more difficult as feudalism established itself. An important element of the bourgeoisie was the merchants involved in international trade, to whom Ashtor pays particular attention because of their connections with European economy. Pirenne's famous thesis has perhaps been pushed aside too hurriedly, for Ashtor is a firm supporter of his findings. There is considerable evidence that for at least two hundred and fifty years after the Arab conquest, contact between the Near East and southern Europe was virtually impossible.

It is not easy to convey the full wealth of this excellent book. Many problems are tackled and subjected to critical and penetrating analysis. There are many pages devoted to the problems of agriculture, as is fitting in the case of an economy which relied primarily on this sector. Feudalism, fiscal greed and neglect of water regulation systems, however, gradually got the better of the natural fertility of Egypt and Syria. This, together with the general insecurity and the virtually unresolvable antithesis between the sedentary population and the seminomadic Bedouins provides the key to the agricultural decline.

So as the comparison with European agriculture is very profitable, other comparisons are useful for the understanding of other phenomena,

namely those which are concerned with developments, sometimes running parallel sometimes being divergent, regarding the economic and social aspects of both the large areas, the European and the Near Eastern: the feud, urbanization, craftsmen, technological level, interest rates, merchants, *essence* of social revolutions. The balance which can be struck does give rise to some perplexity whether it is really opportune to apply the European label "medieval" to this period of Near Eastern history. There is no doubt that it is not completely suitable. Indeed, during the period in consideration, the reciprocal position of the two areas is marked by radical inversions of trend. In Iraq, in Syria and in Egypt, technology came to a standstill and industry declined, whereas in the West the level of industrial expansion rose to such an extent that a « dumping of its products in the Near East was well on the way ». It was at the moment when Europe embarked on the full phase of its expansion that the progressive decline of the Near East ended in the total collapse which took place under the last Mamluks. The area's subjection to the Ottomans in the early XVIth century was the inevitable consequence.

The wealth of erudition and the richness of thought make this a definitive work, destined to become a classic. We must be grateful to Ashtor, because the forms and mechanisms of the economic and social life of the Muslim Orient in this crucially important period could not have found a more enlightening interpreter.

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P. BAIROCH, *The Economic Development of the Third World Since 1900* (Translated by Lady Cynthia Postan), University of California Press, 1975.

This volume examines the broad spectrum of economic development of the Third World during the present century with a view to diagnosing the peculiar forces that condition this development. Accordingly, Bairoch provides not only a sweeping macro view of the global processes of development, but also an in-depth analysis of the underlying changes in such prominent sectors as population, agriculture, extractive industries, manufacturing, foreign trade, education, urbanization and employment. In each case, the analysis is characterised by historical objectivity and an abundance of insight which enables the author to put the main economic events in a proper relationship with each other.

Bairoch's study, following on the heels of his earlier works, fills a notable gap in the literature on the Third World by providing a quantitative study of its long-run growth, and thus isolating the fundamental threads

of development from the transitory and the ephemeral. In addition to being a rich source of empirical material, his study is distinguished from similar works in a number of ways. Firstly, a significant feature of Bairoch's analysis is the recognition paid to structural factors in the processes of growth—factors that are often neglected in the plethora of merely quantitative indices. The significance of structural factors is brought out perhaps most clearly in Bairoch's description of the complexity of the part played by the extractive industries in the general development of the Third World countries. The development of extractive industries particularly after 1948 was primarily intended to serve consumption in manufacturing industries of the advanced countries, and only secondarily as a vehicle of indigenous domestic development. The structural ramifications of this development are instructive. While manufacturing sectors have been too weak to provide an impetus for the domestic processing of the output of the extractive industries, the peculiar pattern of the latter's growth inhibited the growth of domestic manufacturing, since the major proportion of output was exported. Bairoch highlights the very real conflict between premature exploitation of extractive industries which clearly compromises the future chances of an "easier" industrialization and a delay which entails the risk of reducing the economic value of the deposits.

Secondly, a noteworthy feature of Bairoch's analysis is the exercise in comparative statics affording an analytical comparison between the contemporary development of the Third World and that of the developed countries at the time of their own "take-off". For example, the average level of agricultural productivity in the African and Asian countries in the recent past is about 50 per cent below what was achieved by the developed countries at the start of the Industrial Revolution. Similarly, there appears to be a very wide gap between the rate of demographic growth of the developed countries on the eve of this "take-off" and the rates currently experienced by a large number of Third World countries. Bairoch offers a refreshing insight into the relationship between population growth and economic development. The real danger of demographic growth lies in too rapid an increase, and not in the absolute level of population which that growth creates. It could be that for a number of Third World countries, population density is not sufficient to permit a higher level of development. On the other hand, Bairoch cautions that in seeking to achieve a higher density too quickly the possibilities of development may be adversely affected, as indeed is the case in many countries.

Thirdly, an absorbing feature of Bairoch's study is the comparison of economic growth in non-communist Third World countries with China's "unique" path of development. Bairoch's diagnosis for the non-communist underdeveloped world is generally negative. The early effects of industri-

alization in almost all countries are more than offset by stagnation in practically all spheres of economic activity, not to say regression in the agricultural sector. By contrast, Bairoch believes that, despite the clear difficulty in assessing the statistical indicators, China has achieved a successful "take-off" which appears unmistakable when compared with the meagre performance of the non-communist Third World. Bairoch credits this notable performance to the priority accorded to the agricultural sector which resulted in an increase in agricultural productivity by roughly 30 per cent between 1953-57 and 1968-72. This seems to demonstrate that rapid and successful agricultural development is not impossible within the framework of conditions prevailing in less developed Asia, *viz.*, high population growth coupled with a limited quantity of land on which fertility is mediocre and climatic conditions generally unfavourable. These observations fall in line with Bairoch's major stipulation that agricultural development is of prime importance for economic growth. Increases in agricultural productivity have formed the genesis of industrial revolution in western countries.

While Bairoch's view of agricultural development in China seems essentially correct, it probably overlooks the importance of parallel development of manufactures. The policy of "walking on two legs" instituted during the Great Leap Forward envisaged a parallel development of the urban-industrial and the rural-agricultural sectors.¹ The two sectors were subject to different sources of growth in terms of output-mix, factor proportions and incentives, but clearly provided mutual structural support to each other.

Bairoch's analysis, while historical in nature, provides remarkably contemporary insights into development policy. He correctly perceives the problem of industrialization in the Third World as being primarily dependent on domestic demand, with its attendant implications for protection and subsidies. He also underlines the need for increased trade among Third World countries as a means of providing an escape from their narrow domestic markets. Dependence on exports to advanced countries, Bairoch cautions, invites the risk of introducing a new division of labour which would effectively confine the emerging industrialization of the Third World to sectors characterized by moderate growth rates and relatively weaker inter-industry repercussions.

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¹ C. Mou, « Manpower, Employment and Unemployment in Communist China », in *Economic Trends in Communist China*, edited by A. ECKSTEIN, W. GALENSON and TA-CHUNG-LIU, Social Science Research Council, Chicago, 1968.

M. BLOCH, *Slavery and Serfdom in the Middle Ages: Selected Essays*, Translated by William R. Beer, Berkeley and Los Angeles, University of California Press, 1975, pp. ix, 276.

No one would deny that translations can often be of inestimable value to scholars and students. Certainly in this category are Marc Bloch's, *Feudal Society* and his *French Rural History*, both distillations of his brilliant and patient research in varied fields of medieval history. Without these translations numerous American and English students would remain ignorant of what Marc Bloch said about these difficult subjects in a complicated French interlaced with hesitations, doubts, and tantalizing queries. Also in this category is the volume *Land and Work in Mediaeval Europe* (University of California Press, 1967) where J. E. Anderson collected and translated certain of Bloch's articles broad in historical scope, speculative, interpretative, and concerned with methodology. Here, for example, are his famous articles on a comparative history of European societies, on technical change as a problem of collective psychology, on the problem of gold in the Middle Ages, and on natural economy or money economy: a pseudo-dilemma. These articles were intelligently selected from the definitive collection of Marc Bloch's articles, critical reviews, unpublished papers, and complete bibliography of his works issued in two volumes by the Ecole Pratique des Hautes Etudes in 1963 under the title *Mélanges historiques*. It is from this collection that Professor Beer has taken and translated the present articles on slavery and serfdom.

Because Professor Beer has done a generally excellent job of translating these incredibly difficult articles, I am loath to say that it was not a fine idea and yet, regretfully, I must. I do not deny that this translation will be of assistance to some students and a few scholars or that the theme of these articles is important, but their subjects are so extremely complicated and technical that they can be appreciated only by a select clientele, a limited number of scholars and advanced students with a special knowledge of the primary sources and historical literature who can just as easily turn to the *Mélanges historiques* and read the articles in French. Undoubtedly scholars will continue to do just this because they must see the complicated vocabulary of Marc Bloch in the original in order to ensure that they adequately understand his finely calibrated observations and conclusions. Even Professor Beer writes in his Preface that Marc Bloch is difficult to translate because his style is « simultaneously exact and speculative » (p. ix) and because of his « frequent use of words themselves as historical data, for want of clearer records » (p. ix). Without disparaging Professor Beer's sensitive and technically accurate translation of these articles, I would suggest that those who admire Marc Bloch and who still feel that more of his studies should be translated, desist from the laborious

effort. What remains of Marc Bloch in French requires no translation because it is of concern to but a very small group of specialists. To search beyond the *Mélanges historiques* for further unpublished papers and to edit and translate them would be a disservice to the memory of Marc Bloch. His admirers have now reached a limit in making available his ideas and scholarship without making their efforts appear to be a kind of cult, which indeed Marc Bloch would not have appreciated. His principal writings on social and economic history are so basic and seminal that there is no need to keep his name *au courant* by means of *mélanges* and translations that periodically appear. This smacks of hagiography.

Enough said about translations. What about the six articles in this volume? As already noted, only a limited number of scholars will be able to digest the complicated data and arguments. The articles here assembled were written between 1911 and 1933 and discuss a common theme but were not intended to be read ensemble. Some of their content is repetitive. The following sentences selected at random are typically Bloch but to read them repeatedly gives one a feeling of surfeit: « The fortunes of words themselves suffice to assure us that serfdom was something quite different from slavery » (p. 23). « For indeed the researcher has to make a selection, governed both by the very nature of the data and their availability » (p. 33). « I will attempt to place French evolution within the general curve of European development » (pp. 33-34). « In their own way the "charters of custom" are an episode in the intellectual history of the period » (p. 46). « Human institutions being realities of a psychological order, a class exists only through the idea we have of it. To write the history of servitude is above all to retrace, in its complex and changing development, the history of a collective notion, that of the deprivation of liberty » (p. 91). This is the stuff of *Annales* history but it is simply too large a dose.

In the essay « How and Why Ancient Slavery Came to an End », unpublished by Bloch but discovered and printed by the editors of *Annales d'Histoire Economique et Sociale*, Bloch makes the compelling argument that except for limited areas of western Europe, the large slave reservoir of the Roman Empire no longer existed in the Middle Ages, smaller agrarian units of exploitation did not require the human labour used by the *latifundia*, Christianity placed restrictions on slavery and the trade, and slaves (*servi*) were manumitted but not freed from essential economic obligations. Men gradually became free from personal servitude (slavery) but did not become socially, economically, or even juridically free. Though lacking a critical apparatus and with *lacunae* in the text, this paper presents a credible case that squares with what evidence exists between the fourth and thirteenth centuries.

The article most typically Bloch is « Personal Liberty and Servitude in the Middle Ages, particularly in France ». Here Bloch illustrates how difficult and sometimes impossible it is to ascertain the legal, social, and economic condition of those who laboured on the soil. It may have been that in the early thirteenth century *chevage*, *mainmorte*, and prohibition of *formariage* were the only burdens indicative of serfdom but there were large numbers of serfs without these burdens and some peasants with these burdens who were not considered serfs. Some peasants who paid the *taille* were considered unfree but payment of *taille* was ordinarily no indication of personal unfreedom. What, in fact, was the condition of a serf in the twelfth and thirteenth centuries? No longer the agrarian slave of the Roman Empire or the slave of the Carolingian period, he was sometimes unfree, that is, the personal dependent of a lord with burdens more onerous than those of other peasants, and sometimes free but responsible for those burdens imposed by the land held. Although some serfs could leave their land for other land controlled by other lords, economic realities generally blocked such horizontal mobility. Some peasants who were free and called *coloni* in the Carolingian period had descendants known as serfs. Some slaves from the Carolingian period with descendants who had been manumitted were still known as serfs. What Bloch so beautifully illustrates and proves is that a name such as serf connotes nothing exact in a social, economic, or legal sense. By the High Middle Ages serf simply connoted someone who worked the land and serfdom was a *mélange* of tillers of the soil with an amazing variation in burdens and personal legal conditions. Above all, Bloch shows how the meaning of a word can change in response to the change of social and economic evolution.

In a fine article on that class of peasants called *colliberti* in eleventh- and twelfth-century France, Bloch seems to demonstrate that originally such persons had been *servi* (unfree) and then had been partially enfranchised, that is, enfranchised as persons but still not freed from agrarian service and other impositions. They were enfranchised but *cum obsequio* (service). During the thirteenth century *colliberti* disappear from the records and this class seems to be considered serfs. Here again Bloch shows how informal and gradual the process of emancipation, how incomplete it could be, how imprecise the medieval Latin vocabulary, and how hopelessly confused the various classes of the peasantry.

Most students of French seigneurial history have taken for granted the classic expression of nineteenth- and early twentieth-century French historians: *serf de la glèbe*. Supposedly this referred to a serf (unfree man) bound to a parcel of land. But did such a person exist and was there such a medieval expression? Bloch definitely shows that historians of the nineteenth century used this term to designate certain medieval serfs but that it never existed in the Middle Ages. It was a creation of the eighteenth

century popularized by Montesquieu who coined the phrase in 1748: « Servitude de la glèbe ». It became widely used and was defined as follows in the *Dictionnaire de l'Académie*: « Slaves attached to a domaine, to a land holding, were called *esclaves de la glèbe* » (p. 186). In the Middle Ages the term *servus glebae* can be found only in a twelfth-century gloss of the famous Irnerius of Bologna who used it exclusively to explain the condition of the Roman colonus during the Roman imperial period. The word glebe, moreover, never meant a domain or a parcel of land; it simply meant land (*terra*).

Despite the care Professor Beer has given to this translation, I question why he has translated *parlement* as parliament (a totally different institution) and I wish that more attention had been given to proof-reading. On pp. 170-173 there are an excessive number of errors. Occasionally notes are omitted, as on p. 179 after the title of the article. On p. 193 Irnerius appears « around the end of the ninth century » after he has already appeared correctly in the late eleventh century. Domesday Book is misdated by ten years. But these are minor flaws and certainly the non-specialist will welcome the glossary of difficult words. What I regret most, however, is the lack of an introduction where Professor Beer could have discussed the methodology, objectives, and results of the research in these articles. Such an introduction would have helped to sort out the principal themes and results and would have prepared those uninitiated with Marc Bloch's work not to expect well-organized articles with clear definitions and conclusive answers. One of Marc Bloch's achievements was to demonstrate the utter confusion of medieval social and economic history and to rescue it from the well-ordered categories given to it by the legal and institutional minds of the nineteenth century. Marc Bloch has taught us that there is nothing so uncertain and imprecise as the humble medieval classes of men who lived on and tilled the land.

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C. M. CIPOLLA, *Public Health and the Medical Profession in the Renaissance*.
Cambridge: Cambridge University Press, 1976, pp. viii, 136.

In this volume Professor Cipolla provides a readable and well documented account of the history of public health and the history of the medical profession, with particular reference to Renaissance Italy. By use of contemporary Italian archives he presents much information about both of these subjects in the states of Northern Italy during the sixteenth and seventeenth centuries. Through analysis of primary sources, the author

presents a scholarly exposition of the development of institutions recognizable to this day.

The book's introduction sets the tone for its survey of the statistics, prerogatives, and action of Public Health Boards, professional guilds and Colleges of Physicians in Italian cities during the Renaissance. In it Cipolla writes:

« Both health and disease are social as well as biological phenomena. Correspondingly, the doctor has a duty not only to his patient but also to the community to which he belongs, namely the duty to safeguard the health of that community. If medicine is properly viewed and understood in the light of this broader perspective, one must concede that the story of the Health Boards in Renaissance Italy, although generally overlooked, is in fact the most original and most exciting chapter in the history of medicine in the late Medieval and early modern times ».

The first part of the book, *The origin and development of the Health Boards*, comprises the Eleen McArthur Lectures delivered by the writer at the University of Cambridge in March 1975. The origin of these Health Boards in Italy goes back to the pandemic of plague which decimated Europe between the years 1347-51. After that interval the disease remained endemic during the following three centuries. The last great epidemic of plague swept across Europe between 1613 and 1666. Each of these holocausts left major population dislocations in their wake.

When the plague ravaged northern Italy late in the fourteenth century, health boards were established through state edicts in its various cities: Venice, Florence, Pisa, Pistoria, Pontremoli, Leghorn, and later Milan, Pavia, Como, Cremona, and elsewhere. From an initial, urgent concern with stemming the plague, these lay-administered Boards extended their sphere of influence to much wider areas such as general hygiene, registration of deaths, abatement of prostitution, control of movements of domestic and foreign merchandise and conditions under which foodstuffs were sold. Italy was ahead of other European countries in the field of public health and this section of the book emphasizes the relationship between the Boards and the social and economic environment in which they operated.

The second half of the volume, *The medical profession in Galileo's Tuscany*, is devoted to an analysis of a rare census of the medical profession made in Tuscany in 1630. The census reveals that university-trained physicians were more numerous than was generally believed, not only in the cities but also in the countryside. It also shows that most of these doctors came from the country, and that they were sometimes sponsored to study medicine by their home districts. The importance of public intervention in creating the position of community doctors (*condotti*) and the doctor's special role and varied status in society is also elucidated. Guilds of physicians and apothecaries, and of barber-surgeons, with their rules and regulations,

were organized in part to enhance training and reduce the impact of quackery. Six tables and two maps prepared for this book summarize relevant data culled from the archives of the Grand Duchy of Tuscany, encompassing its districts of Florence, Pisa and Siena.

The organization of the medical profession during the Renaissance period was destined to exert a substantial influence on subsequent social developments in Europe. For example, in sixteenth-and seventeenth century Italy, there appeared Colleges of Physicians which were not associated with universities. The early statutes of the Royal College of Physicians of London, England, compiled by Thomas Linacre in 1518, were inspired by similar institutions in Italy. Bills of mortality, quarantine regulations, and public health practices such as disinfection and fumigation also came from this epoch. The period from the beginning of the sixteenth century to the middle of the eighteenth century was a time of transition in public health. During this seminal period, as has been noted by Professor George Rosen in his *History of Public Health* (1958), the basis for change to a more modern model was being created.

Extending his thesis to more recent times, Cipolla cites the likely influence of northern Italy's tradition and organization in public health upon the concept of « medical police » promulgated by the German physician Johann Peter Frank (1745-1821). Frank, who directed public health activities in Lombardy during the Austrian occupation of that region, also taught medicine at the University of Pavia for ten years during the second half of the eighteenth century.

A concluding appendix in Cipolla's book summarizes the 1630 Tuscany Statistics by means of five tables which list the distribution of barber-surgeons and physicians by their geographic location and individual characteristics. A bibliography and index complete the work. Comprehensive in scope and enhanced by perceptive correlative studies, *Public Health and the Medical Profession in the Renaissance* is a significant contribution to our knowledge of the history of medicine and public health.

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A. E. ECKES, JR. *A Search for Solvency: Bretton Woods and the International Monetary System, 1941-1971*. Austin & London: University of Texas Press, 1975, pp. xiii, 355.

The breakdown in 1971-73 of the international monetary regime (the Bretton Woods system) that had prevailed with some modifications since the early postwar years, and its replacement in the spring of 1973 by a

system of generalized floating of exchange rates, have been the occasion for a number of scholarly studies relating to various aspects of the rise and fall of that system. Although the Bretton Woods arrangements, almost from the very beginning and especially since 1960, have been the subject of intensive and often penetrating examination by economists, an interesting feature of the literature since 1973 has been the burgeoning of full-scale studies in this area by political scientists, historians, and even sociologists — almost as if these professions had largely « discovered » the system only after it had collapsed.

The book under review is the work of a professional historian who, in his own words, has had an interest both in diplomatic history and in international economics. The book is a skilful blending of these two fields as applied to the problem at hand, but with pride of place going to diplomatic history. Indeed, despite the period 1941-1971 specified in the subtitle, the author's concern is predominantly with the origins of the Bretton Woods institutions and the diplomatic jockeying that led to the eventual compromise hammered out in New Hampshire in 1944, rather than with the actual functioning of the system after it began operations after the war. Over two-thirds of the study (including a brief introductory chapter on the international financial experience of the interwar period) is given over to the former and less than one-third to the latter. This distribution of the contents, however, is probably all to the good, for the story of the functioning and weaknesses of the international monetary system in the postwar period has been told many times. Indeed, the last third of the book, while competently and interestingly written, draws heavily on secondary sources and in fact adds little to what is already known. This part of the book, incidentally, is not as closely integrated as it might have been with what precedes it.

On the other hand, the author's treatment of the origins of the Bretton Woods system throws some new light in this area and provides a well-ordered chronicle and analysis of events leading up to and immediately following the agreement in New Hampshire. Drawing heavily on manuscript collections and other primary sources, including the papers of many of the American officials involved in the Bretton Woods conference (such as Henry Morgenthau, Harry Dexter White, and Emanuel Goldenweiser), he traces through the early drafts of the American plans for an international Monetary Fund and a World Bank; the Keynesian Clearing Union plan, the divergences between the American and British approaches, and the eventual compromise worked out between them; the alternative proposals offered by the American banking community and other opponents of a universalist approach; the delicate diplomatic manoeuvrings on all sides; the eventual conference itself at Bretton Woods; and the official campaign to sell the agreement to the American public and the Congress. In all of

this the author is less concerned with the mechanical details of the various plans and their economic merits and implications than with attitudes, bargaining stances, political and foreign-policy considerations, and underlying strategies. Interspersed are some fascinating sidelights on the personalities of some of the major participants involved.

The author, while broadly sympathetic with the principles and philosophy underlying the Bretton Woods agreement in the light of the conditions prevailing at the time, admits that in retrospect the agreement was vulnerable to some legitimate criticisms: its universalist approach was difficult to implement in the postwar transitional period; its provisions for exchange-rate adjustment were technically cumbersome and rigid; it made inadequate provision (I would prefer to say none at all) for the orderly expansion of international reserves; and it gave excessive attention to the possibility of postwar depression to the neglect of the possibility of world inflation. One can think of others that could be added to this list.

This study can be recommended for its detailed and scholarly analysis of American planning for the postwar international monetary order. It is likely to appeal more, however, to historians and political scientists than to international economists.

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R. GREY (ed.), *The Cambridge History of Africa*, Volume 4, c. 1600 - c. 190, Cambridge, Cambridge University Press, 1975.

The continent divided into eight regions is surveyed by ten specialists (two chapters being collaborative efforts) giving a general overview of the political, military, religious and economic conditions and events (in more or less that order of importance), plus a final chapter entitled « Africa in Europe and America ». Much of the economic activities were of regional or interregional significance only, but some articulations linked remote inland trade with European maritime commerce. It should be understood that the emphasis of this review slights a considerable part of this volume.

Several European nations competed to acquire the gold exported from the Gold Coast (p. 296 ff.), but the trans-Saharan gold trade continued (p. 151, 216) though Morocco's political ambitions in the Sudan were a failure. Elsewhere on the Atlantic coast, slaves were the principal commodity, sometimes to the extent that, as in the Kongo kingdom, the African polity « faltered and cracked » (p. 329). In East Africa, ivory (e.g., p. 379) outweighed slaves in value (for Europeans), but the Swahili resistance and Omani Arab intervention expelled Europeans from their toeholds on the land north of Mozambique (p. 550 ff.). Only in South Africa did Euro-

peans establish an effective presence and that was mainly as a « half-way house » to the Far East (p. 438 ff.).

Gum for European textile processing (p. 221), maize (p. 11) and rice (p. 292) to victual slaves on the voyage, cattle to victual sailors or settlers (p. 438), copper (p. 344-5), pepper (p. 294), beeswax (p. 295) were other exports; iron (p. 275, 345), wine (p. 326), tobacco and rum (p. 319) were imports of European or American provenance, while cloth was still apt to be Indian (p. 371). Some luxuries still arrived overland from the Middle East (p. 87).

European trade affected African societies by the introduction of new crops (p. 238 ff.), contributed wealth to local rulers (e.g., p. 218), extended firearms to new areas (p. 220, 302), increased the demand for slaves (passim), and exacerbated African political rivalries (p. 328). Disturbances in Europe affected trade and politics in Africa: the king of Bambuk's position was weakened when French traders, during the French Revolution, stopped sailing up the Senegal River (p. 221).

Black slaves were imported into Europe (p. 551 ff.) as well as the Americas, and white slaves, taken by corsairs in the Mediterranean (p. 88), were sometimes sent south of the Sahara — usually as slave soldiers — to Bornu or other states (p. 87). In Iberia, agricultural work was done by Africans (p. 582), elsewhere the nobility had African domestics (p. 589 ff.). Slavery in the New World produced plantation products such as sugar and cotton that were destined for Europe (p. 599). "Triangular trade" linked three continents (p. 593). Profits in Europe helped finance the Industrial Revolution (p. 596). A new attitude towards slavery, partly due to the changing economy (p. 598) and to slave rebellions (p. 597) in America led to the beginnings of the movement to abolish the slave trade and ultimately slavery.

The seventeenth and eighteenth centuries, in Euro-African relations, lie between the earlier period of Portuguese exploration of the coasts and attempted monopoly of the sea trade and the nineteenth century movement to suppress the slave trade, substitute "legitimate" trade, explore the interior and divide the territories in spheres of interest and colonies — it lacks, as a period, the innovative qualities of the preceding and succeeding periods: the coastal Africans, who already knew Europeans, merely saw more of them, but only doing the same things. Taking Africa, on the continental scale, Europeans were not important — though some of the indirect consequences of the coastal contacts were — and for most Africans in most societies the existence of the alien whites was still unknown.

This Africa-centric history is in the tradition, at least two decades old, that broke with the former focus on European activities in Africa and is a narration of dynastic conflicts, rise or fall of states, innumerable wars, the spread of a proselytizing religion in some regions, opening or blocking

trade routes (and, of course, asides on the evaluation of evidence or interpretations) in which Europe is largely irrelevant — in other words, the historians have accomplished what they set out to do: they have made the history of Africa (which some said, so recently, did not exist) exactly like the history of everywhere else. We are treated (if that is the word) to the spectacle of the activities of *alafin, brak, damel, etsu, farma, mai, mansa, mfumu, naba, negus, oni, reth, sarki, satigi, tunka*, and others — all rulers — and of *abuna, akomfo, mallam, nganga*, and others — all priests or clerics, in such states as Akwamu, Bondu, Dagomba, Funj, Kafu, Ndongo, Sakalava, and Wadai — so, while the themes are familiar the flavour is distinctive.

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D. HAY, P. LINEBAUGH, J. C. RULE, E. P. THOMPSON, and C. WINSLOW,
Albion's Fatal Tree, New York: Pantheon Books, 1975.

We all agree, I assume, that history and sociology have much to offer one another. History with its indefatigable attention to detail and sociology with its concern for nomothetic themes can enrich each other in a variety of ways, and the best work in both disciplines regularly demonstrates this fact. Immanuel Wallerstein's *The Modern World-System* and Richard Hofstadter's *The Age of Reform* are only two recent examples. To a certain extent *Albion's Fatal Tree* falls within this tradition in that it brings the historian's aptitude for uncovering the circumstances of history to a topic, crime, that is usually examined sociologically. But in so doing it underscores again the value of relating historical research to the fundamental social process at work, so as to give the research fuller meaning.

This volume is a collection of six essays by five authors dealing with criminological topics as diverse as the death penalty and threatening, anonymous letters. Each essay is thorough in describing its particular topic, but several fail to link the particular pattern of crime described to basic social processes and thereby leave the reader dangling.

Crime illuminates not only the condition of the lower classes but also the routines of the courts and their agents. To understand criminality we must know not only the life styles of the various social strata of the community but also the social adjustments that courts and their agents make to crime in both an intensive and extensive sense. Several of these essays attempt to draw these larger connections but only the first succeeds. The others inevitably prompt the question: of what significance is the research so painstakingly performed and reported? By way of illustrating this point, let us examine each of the essays in turn.

The first, « Property, Authority and the Criminal Law », by Douglas Hay is by far the best. He is concerned with the paradox that although there were more than 200 capital offences in eighteenth-century England, executions were roughly stable, especially from 1750 onward. The standard explanation, which Hay rejects, is that English jurors and judges were not willing to impose such fierce punishments upon minor offenders, who constituted a large portion of those liable to execution; hence, they dismissed or acquitted an increasing proportion of defendants charged with capital offences. Hay points out that although sanguinary laws (and convictions under them) continued to grow, even into the nineteenth century, executions after 1750 remained roughly stable. If the law and the courts were so at odds, it is unlikely that Parliament would have continued to rely so heavily upon the death sentence.

Hay's explanation is that the royal pardon which was used broadly in capital cases was actually the means whereby the allegiance of ordinary Englishmen was cemented to the crown. The gratitude of those hundreds who were reprieved from execution removed any resentment they may have felt in the first place for being sentenced to hang for a relatively minor offence. The mercy of the law was the impression that a liberal use of pardons conveyed, and Hays through careful research demonstrates that that was exactly the intent of the ruling class.

« The Tyburn Riot Against the Surgeons » by Peter Linebaugh takes us a little deeper into the problem of the death sentence by exploring the morbid circumstances of hangings: the struggles between prisoner's relatives and surgeons who wanted the corpse for medical school demonstrations; the infrequent revivals of prisoner's after execution; and the condemned's demeanour and attire during the long trip to the gallows. But Linebaugh summarizes the significance of it all rather limply by noting that although young people died in this manner regularly in Georgian England, eighteenth century Englishmen did not grow calloused to it. That may well be true, but of somewhat greater importance are such questions as, did such spectacles have any effect upon the gradual lessening of the severity of the law in later years, or did they have the planned effect of helping to contain serious crime in eighteenth century England? As interesting as this chapter is, it fails to justify itself by illuminating the place of public executions in English criminal justice, and it suffers for it.

Up to this point the essays have suggested that the English common man was relatively obedient and compliant and that he respected the law and submitted resignedly to its will. The remaining essays carry a different theme: that Englishmen in the countryside were defiant and contemptuous of the law. They smuggled vast quantities of goods past the King's customs officers (Cal Winslow, « Sussex Smugglers ») and dared them to enforce the law. They plundered coastal shipwrecks (John G. Rule, « Wrecking and Coastal Plunder ») in flagrant defiance of longstanding law. They

poached game on their lords' preserves (Douglas Hay, « Poaching and the Game Laws on Cannock Chase ») or conversely destroyed the preserves in open rebellion against the law and the hereditary rights of the aristocracy. And when all else failed, they resorted to anonymous, threatening letters against those who oppressed them (E.P. Thompson, « The Crime of Anonymity »).

Each of these essays describes in full detail the particular crime in question, but I constantly found myself asking, how important was this particular form of crime in the context of eighteenth century English society? Was it of such importance that severe counter measures in the law and crime control were undertaken to stamp it out, or was it of irritant value only? There are several passing references to the Black Act which was enacted precisely to control these forms of crime, but there is no extended discussion of it or its connection to these kinds of crime. These kinds of questions would point the authors in a different direction, but it would help to lay the foundation for understanding the place of crime in eighteenth century English society.

A secondary theme underlying each of these essays is the priority of upper class privilege over lower class necessity. The English poor were hemmed in by a Common Law that respected the rights of property over the pitiful, often desperate needs of simple men and women. Although the authors do not highlight the Marxist implications of this theme, the conclusion is inescapable that early modern English law was an instrument of oppression, preserving the economic advantages of the gentry against the human claims of the poor.

But is it so remarkable that the law of the period (and, indeed, of any period) should protect the institutions defining the distribution of power and wealth? Has the law of any society — socialist or otherwise — ever attacked prevailing political or economic institutions? Political parties and movements regularly do — but the law by its nature tends to conserve existing arrangements. And the sooner we recognize that fact, the sooner we can move on to more urgent and revealing kinds of questions.

In sum, then, this collection of essays deals with a wide variety of eighteenth-century English criminological problems and in so doing demonstrates a common difficulty among historical studies of crime. It documents carefully a series of interesting, if loosely related, crimes, and only secondarily attempts to link them to a broader theme or pattern. Had it fully succeeded in this regard, it might have become a standard work for years to come. As it stands it will be used by criminologists and historians to extend their knowledge about selected forms of eighteenth century English crime, but it will not provoke many arguments regarding the nature of crime or criminal justice.

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W. O. HENDERSON, *The Rise of German Industrial Power 1834-1914*, University of California Press, Berkeley and Los Angeles, 1975.

Over a period of many years Dr Henderson has given us a number of books and papers on the economic development of Central Europe in general and of Germany in particular. To this already long list he has now added a masterly survey of German industrial growth in the nineteenth and early twentieth centuries. A man born in 1834 and dying eighty years later at the outbreak of the First World War would, he notes, have lived through the whole span of Germany's development from a backward, pre-industrial nation to the foremost industrial power in Europe and the world. This progress he chronicles with a wealth of detail, but also with balance and judgment.

The book, like the period which it surveys, divides itself naturally into three parts. The first is that from the dawn of the industrial era to the mid-nineteenth century. The second, from 1851 to 1873, saw the rise of the great German manufacturing industries, while the last, from 1873 to 1914, was marked by Germany's achievement of the status of a giant among the world's industrial powers. The first of these periods was marked by the achievement of economic unity, and the author leaves the reader in no doubt that this was a precondition of industrial growth even within the extensive lands of Prussia. During this period much of Germany's industry was still being carried on as a craft, obstructed by guild regulations and catering almost wholly for a domestic market. The Great Exhibition held in London in 1851 marked its termination and at the same time offered a prospect of future German industrial growth. No less than seventeen hundred and twenty German manufacturers exhibited. Most represented traditional German crafts, but among the miscellaneous products of the potter and the printer was a steel ingot weighing forty three hundred pounds, a triumph of the technical and organizational skill of Krupp of Essen and a precursor of the steel castings and forgings which were for good or ill to come from that and other works within the Ruhr industrial region.

In many aspects of the European economy the years about 1850 marked a transition from slow to rapid growth, but in no country was this phenomenon of "take-off" more sudden or more significant than in Germany. It provided the sinews with which Prussia was able to defeat both Austria and France and to weld the German Confederation into the German Empire. The last forty years of the period covered by this book is essentially that of Bismarck's German Empire, when German technology advanced more rapidly than that of any of its rivals, when a simple iron industry was transformed into a complex steel industry, and a gigantic, all-embracing chemical industry was created. These develop-

ments are described by Dr Henderson, together with the parallel developments in banking and commerce which made them possible. Indeed, his short discussion of the role of the major German banks throws much light on the rapid pace of industrial growth.

Bismarck's Germany was nonetheless a country of paradoxes. Why did agriculture remain relatively backward at a time when manufacturing industry was making such immense advances? Why did a régime which so loudly protested its loyalty to the capitalist system nevertheless take into its hands a tenth of all mines, factories and transport facilities, and make at one time up to a quarter of Germany's total capital investment? And why, despite its declared hostility to socialism and its ineffective proscription of social democracy, did Germany provide the earliest example of an effective if somewhat paternal welfare state? These questions are not really raised, and it is as a record of the industrialization process and of its political and economic constraints that this book is most successful.

It is, nonetheless, not without its blemishes. Its style is in places dense, and the authors's desire to include all relevant data leads occasionally to disconcerting juxtapositions and to lists of names which can have little significance to the reader. There are inevitably minor errors. There was, for example, no blackband ore in Luxembourg (p. 236). There is a number of maps, in general well drawn, but inaccurate in detail. The map of Silesian coal mines bears no date, while that of the Ruhr is dated but grossly inaccurate. But these are minor errors and easily corrected in a book which must become a standard work on German industrial growth.

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CH. HILL, *Change and Continuity in Seventeenth-Century England*, Harvard, University Press, 1975, pp. i-xiv, 370.

The matter may not be new in Mr. Hill's latest book — it is a collection of his papers and reviews published in various places over the past decade or so — but it is nevertheless stimulating. Working from the theme of change and continuity and drawing from both intellectual and material currents, he concludes that, as in all historical periods, much changed in the seventeenth century, but much yet remained the same. That change and continuity are both part of the historical landscape is hardly a startlingly new discovery but the specific content which Hill puts into it is very much worth considering. On scientific development, the book opens with a discussion of witches and cunning men in the «dark corners of England» and concludes with Isaac Newton, «the high priest of modern

science ». But, says Hill, it is wise to remember that Newton was an alchemist and a student of biblical prophecy. In the realm of philosophy the early seventeenth century was dominated by the thinking of Hooker — « Aquinas in sixteenth century dress » — and by the end of the period John Locke, the forerunner of modern utilitarianism had appeared. But once again, Hill reminds us, « Dr Locke believed that a patient could be cured of pain in the kidneys by burying her urine in a stone jug ». In a similar vein, other chapters discuss the relationship between protestantism and capitalism, the influence of social pressures on old ideas and an assessment of seventeenth-century radical critiques on the principal professions — law, divinity and medicine.

No one will be surprised by the interpretation given to any of these topics by Mr. Hill. Economic self interest, the link between protestant ideas and the spirit of capitalism, the respect paid to the ideas of ordinary people as they are found in contemporary commentary, the appreciation of the complexity of historical causation and in particular the importance of both ideas and material influences, the distrust of public administrators' views about the departments they manage and the evidence their offices generate — all of these are familiar enough, although to be sure they are worth repeating because they are so often forgotten.

It is the manner and the tone in which he carries his arguments along that is most impressive. In contrasting the « dark corners » of Wales with cosmopolitan London he quotes a preacher at Paul's cross: « Methinks I come into another world, even out of darkness into light . . . ». To hammer home his argument that protestantism was a liberating force, a necessary prerequisite to social change, he quotes Tyndale's truculent language, « To steal, rob and murder are holy, when God commandeth them ». But he is at his best in portraying class hostility and the contempt in which the governors of society held ordinary people, the « many-headed monster ». On class hostility, a Norfolk gentleman is quoted, « all have conceived a wonderful hate against gentlemen and taketh them all as their enemies ». On the other side, an MP says, if « the ruder sort were privy to their own strength and liberty allowed them by law they would be as unbridled as untamed beasts ». Summarizing the contempt in which the ordinary people were seen by their richer neighbours, a nobleman said, « the poorer and meaner people have no interest in the commonweal but the use of breath ».

Many will challenge Mr. Hill's conclusions on these subjects and some will, quite rightly, quarrel with the evidence on which some of them are based. It is, clearly, difficult to make quantitative judgments based on quotations, however many are gathered together and no matter how incisive their content may be. But only the most stubborn will be unimpressed by

the mass of contemporary comments he marshals to make his points. He is truly a master at capturing an ocean of generalization in a drop of specific detail. The matter, then may not be new and the manner may be open to question, but because both are stimulating and important, they are very much worth having.

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R. H. HILTON, *The English Peasantry in the Later Middle Ages*, Oxford, University Press, 1975, pp. 256.

This collection of essays, consisting of Professor Hilton's Ford Lectures of 1973, as well as five so-called "related studies", presents readers with a good summary of this scholar's views on the English peasantry in the later Middle Ages. As such, it offers one a concise statement of the Marxist approach to the subject, and in so doing reveals its many weaknesses.

In his first lecture, Professor Hilton notes the incontestable fact that while social scientists have expressed an increasing interest in the peasantry, their interest seems to stop short of historical work. In reading over Professor Hilton's studies, it is clear why so few anthropologists and sociologists have been able to find any relevance in the offerings of historians. The level of social, not to mention economic, analysis of the peasantry is simply too superficial to arouse the interests of many social scientists.

For Professor Hilton, the peasantry itself is seldom the topic. The peasantry is merely treated as an oppressed class in feudal society and there is little attempt to understand the inner workings of peasant society, or the peasant economy. It is this essentially Marxist approach to history which allows Professor Hilton to dismiss out of hand the work of Professor Raftis and his « school » as an expression of « . . . medieval organic society theory in a modern social anthropological disguise » (p. 11).

Whether or not Raftis' theory of medieval society is « organic » can only be answered by Raftis himself. However, it is important to note here and now that this is not a very relevant issue. The significant aspect of Raftis' early work was his realization that a systematic analysis of village court rolls could offer profound insights into the social and economic realities of village life. Finally, Professor Hilton errs in referring to the Raftis « school » — it does not exist. While some young scholars have recognized the value of court roll analysis, it would be grossly misleading to insinuate that their work represents any kind of « school ». The approaches,

as well as the academic significance, of studies based upon court roll analysis vary greatly and each study must therefore be considered on its own terms.¹

In all six of Professor Hilton's lectures there is a general failure to come to grips with the realities of village life. This is particularly apparent with respect to the important relationship between the structure of peasant families and the economic organization of medieval villages. While seeming to recognize the existence of such a relationship, as noted with respect to the work of A. V. Chayanov (p. 6), Professor Hilton never probes the various aspects of family organization in his « thirty or forty villages » (p. 17) in the West Midlands. Indeed, he has not used his records, which include village court rolls, to reconstruct the families, and thereby much of the socio-economic structure, of even one of his villages.

As a result, his analysis of social structure in the second lecture is very weak. Despite the fact that scholars working with court rolls have been able to reconstruct families and offer a very detailed description of the social structure of medieval villages, Professor Hilton has turned to *Piers the Ploughman*, the *Towneley Plays*, and the *Canterbury Tales*,² among various other literary sources, in order to produce a mélange that can no longer pass for a serious discussion of the social organization of medieval villages.

Professor Hilton's failure to deal with village social structure in a meaningful way carries over into his discussion of the peasant economy. He offers no insights into how land was distributed within his villages, and although he mentions debt arrangements, he does not attempt to relate the debt structure to the socio-economic hierarchy of village families.

The lecture entitled « Conflict and Collaboration » gives one the impression that the only real point of conflict in rural society was between the peasantry and the lord, while collaboration was the hallmark of inter-peasant relations. However, even the most cursory examination of village court rolls reveals a host of violent crimes among the peasantry, and a forthcoming study³ shows conclusively that such violence occurred along

¹ Raftis' early articles, published during the 1960's, along with his volume *Tenure and Mobility*, did much to influence Edwin De Windt's 1972 study, entitled *Land and People in Holywell-Cum-Needlingworth*. None of these works bear much resemblance to Raftis' rather unfortunate 1974 volume, entitled *Warboys*. Finally, a forthcoming volume by this reviewer, entitled *The Community of the Vill*, presents a picture of village society which is quite different in content and emphasis than any of the previously mentioned works.

² Although Professor Hilton uses Chaucer as an historical source, he is forced to correct the author of the *Canterbury Tales* when he fails to comply with Marxist theory (p. 25).

³ EDWARD BRITTON, *The Community of the Vill* (Macmillan of Canada, 1977), Chapter 10.

class lines within the peasantry itself. Far from being a rather homogeneous group of country folk working in cooperation, the English peasantry was highly stratified and the upper levels of village society possessed a degree of self-awareness which even Marx would have had to describe as « class consciousness ».

The essay on market towns is generally more helpful than the first four lectures, but in view of the fact that Professor Hilton is anxious to show that such towns were quite distinct from rural villages, one might well ask if the chapter is relevant to the studies which precede and follow it. As to the discussion of women in the final lecture, it is based largely upon the findings of G. C. Homans, W. O. Ault, and J. A. Raftis — all of whom are given no credit.

The so-called « related studies », drawn from obscure journals and covering a much larger segment of the medieval period than the first six lectures, only serve to illustrate how little Professor Hilton's views have changed in 25 years. Indeed, they show that despite his espoused concern for the historical process, the historical significance of the peasantry is immutable in his view. From decade to decade and century to century, only the few examples of supposed feudal oppression are regarded as important. The peasantry is seldom examined for itself, either from an historical, or sociological, perspective.

One final point, it is rather ironic to note that an historian such as Professor Hilton, who tends to regard with thinly-veiled contempt any incursion onto Clio's sacred ground by social anthropologists, should attempt to place King Alfred of Wessex in the eighth century (p. 20). Surely, he meant to place Alfred in the ninth century — where he belongs.

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C. S. MAIER, *Recasting Bourgeois Europe. Stabilization in France, Germany, and Italy in the Decade After World War I*. Princeton: Princeton, University Press, 1975.

This is a significant book that offers new insights into the organization of politics and the organization of economic life in Europe after 1918. Its primary focus is on the efforts of European conservatives and liberals to restore what in their various visions was seen as the heart of the prewar international bourgeois order against the onslaughts both of radical democracy and socialism. But Maier is interested not so much in bourgeois objectives

per se as in the problems that bourgeois liberals and conservatives confronted, in the means which they used to resist their socialist and trade-union opponents, and in the shared characteristics of the regimes that emerged from their efforts.

These are of course large differences between the three societies that are explored here, in their postwar political systems, and in their economies. Yet in concentrating on the growth of corporatist approaches to resolving political and economic conflict, Maier throws light on the range of settings under which the direct negotiations of economic interest groups could become equal to or even displace the mediation of political parties and parliaments, in the political as well as in the economic sphere. Among the problems faced by all of Europe's combatant nations after the war were demobilization, unemployment, the settlement of national and international war debts, and the reduction of surplus capacity in such diverse areas of production as agriculture, iron, and steel. The immense difficulties involved in resolving these problems gave rise in turn to further problems, e.g., unstable parliamentary majorities and galloping inflation.

In Germany, where corporatist approaches to the containment of conflict developed precociously, the iron and steel barons of the Ruhr accepted a partnership with the German trade unions in the Stinnes-Legien agreement of 15 November 1918 that became for a time the country's primary basis for dealing with domestic economic conflict. German heavy industrialists were concerned with preserving as much as possible of their property and power during the revolution of 1918, and thereafter with finding ways to exploit the unstable financial and monetary conditions to restore their position both in the Reich and on international markets. Their alliance with workers represented a cartel of well-organized producers against fragmented consumer and saver interests—the latter composed largely of shopkeepers, craftsmen, free professionals, and other middle-class bondholders. The alliance weakened only when the heavy industrialists saw opportunities after the mid-1920s to utilize « rationalization » to regain some of the terrain lost to workers in the factory, and later during the depression to exploit the prostration of labour to recapture further ground.

The class systems of Italy and France were indeed different from that of Germany, but certain relationships become clearer as Maier compares the systems. The Italian bourgeoisie had been able to maintain its position in society since the Risorgimento through its control of the narrow base of the Italian political system. The demand for replacing the liberal-dominated prewar system with a genuinely democratic regime that was unleashed by Italian socialists after Italy's entry into the war seemed to the country's elites to threaten the linchpin of their power. On their own, Italian industrial interests would have been too weak to induce labour

into collaborating to lay a corporatist foundation for the restoration of the bourgeois order. But once the Italian elites had made their pact with Mussolini, the fascist regime in turn was able to force both industry and labour into corporatist agreements that stabilized the Italian economy and strengthened its international competitive position. In France there was no dominant industrial group such as iron and steel in Germany, nor an immediate threat to the political power of the bourgeoisie as in Italy. More than any other concern except perhaps the fear of socialism, it was the desire to preserve the value of savings that finally united the otherwise so diverse elements of France's bourgeoisie. Maier suggests that it was the strength of this broad interest in protecting savings and the relative weakness of those producers' interests which saw opportunities for expansion in run-away inflation that accounts for the reining in of France's inflation in 1926, before the value of the franc had been completely destroyed. Of course the low international exchange value at which the franc was stabilized suited the interests of French exporters. But a "competitive" franc was an essential part of the fragile system of international agreements on which the temporary return to international stability in the late 1920's rested. Without it, French industry would not have consented to the several direct agreements between producers regarding contingents, market shares, and other limits to production that were so conspicuous parts of the effort to restore international economic order.

In exploring the relationship between economic and political change, Maier's book addresses some of the most difficult problems of twentieth-century Europe. The support of the German *Mittelstand* for National Socialism, Maier notes, was in large measure a reaction to the corporatist collaboration of better organized and therefore more powerful interest groups in German society. In contrast, it was largely because of the weakness and the low level of corporatist organization of the Italian bourgeoisie that its members believed it necessary to turn to fascism for self-protection. But in France, Maier suggests, it was the very underdevelopment of corporatist organization that made the parliamentary regime tenable in the eyes of the bourgeoisie. Two underlying themes of Maier's analysis of the 1920's may be even more important for understanding the dilemmas of Western industrial nations today - the immense difficulty of reconciling conflict between those interest groups that are sufficiently well organized to enforce their demands on society and those groups who are forced to pay the costs of fulfilling these demands, and the inevitability of conflict between the effort to satisfy the competing demands of domestic economic interest groups and the effort to remain competitive in the international economy.

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F. MELIS, *Origini e sviluppi delle Assicurazioni in Italia, secoli XIV-XVI*, vol. I, *Le Fonti*, Roma, 1975.

The first volume in the great series on the study of insurance in Italy which Federigo Melis was organizing, has now been published in very lavish form and with a rich documentary appendix which includes black and white and coloured reproductions, has now been published by the *Istituto Nazionale delle Assicurazioni*.¹ The publication itself represents an act of intelligent cultural patronage, since only an institute of such stature is nowadays in a position to undertake a project of this type, which is now beyond the means of the commercial publishing houses. It would be possible to discuss the issue of patronage at great length, and draw contrasts between Maecenas and the noble patrons of the XVIth or XVIIIth centuries, or with contemporary institutions. However, it is sufficient to say that, were it not for the awareness and foresight of the directors of the INA, this work would have remained in manuscript, only to be discovered perhaps by scholars in the distant future.

Although unfinished, Melis' work will now be available for scholars and they will be able to profit from the enormous industry, intelligence, and technical skill which the author devoted for many years to the project. But it represents, on the part of the INA, a gift which will be valuable to all economic historians and not solely to those concerned with the study of insurance, for Melis's work constantly flows beyond the boundaries suggested by the title. Bruno Dini, the successor to Melis' university chair, Elena Cecchi, who supervised the revision of the text and the author's younger assistants, Letizia Baldinotti Rio, Luciana Frangioni and Anna Affortunati Parrini, have all also contributed to the volume. Bruno Dini was responsible for editing the posthumous work, and he had the difficult task of introducing the first chapter, the only one completely written up by the author, and of providing a guide to the work as a whole. This was particularly difficult, as Melis did not generally write up his notes. His great historical intuition meant that he was always able to find something of importance in both factual and conceptual terms from every document he studied. He would dissect a particular document and explore its every inference and significance, relating both the essence, and every secondary aspect, to hundreds of other documents, relying throughout on his truly remarkable memory and exceptional ability to recognize the general themes hidden behind a mass of discrete information. He would then dictate a textual explanation which was always definitive from the first draft, and subsequently required only the addition of notes before printing.

¹ This is evident from a study of mercantile accounting currently being undertaken by Giovanna Anastasi Motta at the suggestion of Melis himself.

In this way Melis did not make written notes or rough drafts. The whole work had already been formed in his mind, and as he did not commit to paper more than the first chapter there must have been good reason for this. In my view, this lay in the fact that he was not satisfied. He was aware that he had not found solutions to certain problems. In particular, I believe, he was still dwelling on that tried question in the history of insurance, the premiums paid by the insured to the insurer. These were rarely mentioned in the contracts, and at present there is no very clear reason for this. The other problem that he had not fully resolved was that of the real incidence of the various maritime risks.

Melis was concerned to demonstrate the historical and economic aspects of insurance procedures, and held that the legal aspects were secondary. Insurance was the creation of commerce; it developed to meet the needs of commerce, and hence varied from place to place. Gradually, general procedures began to emerge (in Pisa, Florence, Barcelona etc.). For this reason insurance has little connection with common law, and it was at first camouflaged, either in order to avoid certain prohibitions, or else because the notaries did not know how to devise a formula for the contracts in question. How much has been written about the skill of these notaries, who were in fact a great mass of bureaucrats who were often unable to understand the formulae which they copied out! The best example of this is provided by the Palermitan notaries, typified by a certain Comito, of the early XVth century, who never took part in drawing up insurance contracts but simply restricted himself to transcribing the policies which were presented to him, already witnessed and signed, in order to keep record of them in case the originals were lost.

This inflexible but evolving system lasted until the merchants succeeded in preventing the lawyers from participating in the commercial and consular tribunals, in which equity procedure was followed, *sola facti veritate inspecta*. But when it came, the contact with law petrified insurance procedures as they stood, and prevented any further development. In fact, it was not the law that created insurance, but rather the latter which eventually led to the development of insurance laws which sought to organize the best of the traditional practices, or at least those that were most widespread.

Melis was concerned to study the development of insurance practices before they became petrified by the law, to attempt to capture the dynamic, the vitality and the spontaneity of their development. He was also determined to analyse them in terms of the development of commercial activities as a whole, as an element in their own right of national product, if one can use such a contemporary term for this period. In this sense, premium and risk became fundamental elements of historical interpretation.

Starting from the first chapter written by Melis, which deals with the research generally and with the interpretation of the sources, together with the documents which he had chosen to illustrate the chapter, Professor Dini

has succeeded, with great modesty and considerable intelligence, in reconstructing the nature of the overall plan which the original study would have followed had the author been able to complete it. The first chapter on the sources would, he suggests, have been followed by a second dealing with the economic background from which insurance procedures were born, « paying particular attention to the economic operators and their culture, to trade, the movement of goods, shipping and the function of the city ». In another chapter the precedents of insurance procedures would have been discussed, and finally the birth and development of the insurance contract itself. The material found amongst Melis' papers indicates that this broad panorama would have included references to the insurers, the insured, the brokers, the type of risks, the compensation of losses, and the judicial practices, relating to insurance, of the *Consolati del Mare*, the *Consolati delle Nazioni* and the commercial tribunals. The latter was the subject of a particularly long study. There was also a paragraph, or perhaps even a full chapter, on the origins of life insurance, which began as a form of gamble, but was soon transformed into a business. In this way the history of insurance would have become a central feature of the development of commerce, especially sea trade, of which it would have illustrated many human and technical features.

Professor Dini has also himself derived certain quantitative estimates from the deeds of two XVth century Genoese notaries and a XVIth century Florentine broker. Between 1427 and 1431, 2,471 insurance contracts were drawn up in Genoa for a gross value of 986,985 Genoese florins, while an analysis of the individuals providing insurance reveals a clear distinction between a mass of very small speculators, and a much smaller number of large-scale operators who held a dominant share of the business, and were capitalists who specialized in providing insurance. In the case of Florence, there is information for only one of the two brokers who were operating, from 13 Feb. 1524 to the 28 July 1526. After careful analysis, Professor Dini has succeeded in showing that the directors of commercial companies formed a special group of insurers. They employed the capital of their respective companies and speculated on their own account, with the result that the bulk of the premiums finished in the "private" accounts of the businessmen and were not paid back into the companies themselves. Due to his scholarly care and extensive knowledge of the problems in question which he possesses, Professor Dini's introduction is extremely valuable. It also has the further merit of providing a comprehensive bibliography which is now immense.

The first chapter and the documents which it contains, however, suggest that there are still a number of open problems. On that of the relationship between premiums and real risks, I think that the answer will be found through the study of the random references which exist in the vast archival documentation. This does not require a major monograph, but

rather a series of individual contributions which can then be brought together for more detailed examination, and the ideal place for this would seem to be the Institute of Economic History at Florence University, which already possesses the huge files collected by Melis. I would also take the opportunity to draw attention to the existence of authentic insurance archives in the Archivio di Stato di Palermo, which were kept by notaries who enjoyed the confidence of the Genoese at the time in the XVIth century when the latter exercised a virtual monopoly over maritime insurance. There are other sources as well, and, from a number of examples, it is worth referring to a strange form of calculation which appeared in Palermo at the end of the XVth century. The percentage ratio of the premium was apparently set by the major operators, while the small merchant charged the insured the fixed premium of one ducat, and from this calculated the capital insured (e.g. at 11% this would be 9 and 1/11 ducats, a sum which is not translatable in current money). In this fashion a small merchant was able to participate in insurance operations in Flanders or in Venice, on goods he had never seen, in favour of unknown persons, and on ships of which he was entirely ignorant. This was clearly a corruption of the principle, a kind of prudent but popular lottery. But it is also something worth noting, because it typifies the fashion for insurance operations in the XVth century.

Melis himself never overlooked the authentic gamble (for example, he published the gambles undertaken by Tuccio Fioravanti, a Pisan living in Messina, who recorded his bets in the master-ledger at Antwerp). Life assurance derived directly from betting in the first instances, because although the insured was sometimes directly interested in the life of the person on whose life the bet had been made (a pregnant slave, for example), in most cases there was no direct interest and frequently the insured life was not even related to the individual who took out the insurance. This topic would certainly have been expanded by Melis, in the way of which only he was capable, so I shall simply refer to Document. 30. (p. 217), dated 1549, in which the invalidity of an insurance contract is invoked, on the grounds that the insurers were unaware of the illness of the individual on whose life the insurance had been taken. The individuals concerned are well known, and the life of the Marchese di Terranova, which had been insured, was worth many thousands of *onze*, rather than the 160 *onze* stipulated, indicating that it was essentially a form of gambling which merchants in Palermo indulged in. Amongst these, the merchants originating from Lucca showed a particular interest; the holder of the policy, Gaspare l'Apostolo, was a banker, as was Accascina the insurer. The premiums referred to in the document reveal that the life of a man in good health could be insured over a short period at 6%, while for someone suffering from a disease the percentage rose to 50%.

Such evaluation of risks indicate that there was a degree of expertise in such business, and since we are dealing with Lucca merchants in Palermo this provides grounds for speculating on the spread of insurance techniques. The fact that the insurers were meditating legal action by taking their case to the Viceroy, also shows that there was already a legal machinery for dealing with insurance questions. In all, then, we must probably assume that the practice of life insurance was more widespread than the documents alone indicate.

In addition to these problems which require further research to fill the gaps that Melis had not written up (always bearing in mind the remark of a friend: Melis is irreplaceable), a reading of these sources indicates another important issue: what happened to maritime insurance in the great Mediterranean crisis of the XVIth century? In Sicily it disappears, and in its place cargoes were split up and shipped on more than one vessel, often of very small size. Why was this? Was it because the risks became so high that they exceeded any possible increase in premiums? Or was it because insurance procedures became entrapped in legal technicalities and suffocated, so that they ceased to provide an attractive business proposition? Or was it because smaller capital was unable to face the risks involved after the withdrawal of the major operators, who found investments in government loans more attractive? Whatever the answer, the Sicilian case is of interest again, because Sicily was one of the leading centres of insurance operations, as Melis demonstrated by frequently referring to cases there and as the activities of the Genoese in the XVIth century confirm.

It is also important to establish when it was that control over the insurance market, as over so many other things, was lost by the Italian and Mediterranean merchants. In this respect, it is important to remember that at a certain point the Mediterranean became dominated by shipping from Northern Europe, especially the English and Dutch. At the end of the XVIth century there was also Elisabeth I's vague project for occupying Malta. From the evidence of the Sicilian and North African ports, as well as from the Dutch archives, it would seem that the larger insurance operations were by this time already being undertaken outside the Mediterranean. If this is correct, it provides another feature of the general assessment of the crisis.

In my opinion, then, this work by Melis provides a lesson in methodology of fundamental importance, and it is to be hoped that many will take up the incentive to further and complete these studies, so that the labours of this truly masterly scholar and friend do not remain infertile, and also in order to persuade the INA to continue its interests in the field of economic history.

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J. SCHNEIDERMAN, *Sergei Zubatov and Revolutionary Marxism*, Ithaca, New York, Cornell University Press, pp. 401.

Professor Schneiderman has written a comprehensive account of the *Zubatovshchina*, a police-sponsored trade union movement that emerged from the efforts of Sergei Zubatov, the chief of the Moscow *Okhrana*, at the turn of the century. The latter, impressed by the reformist orientation of Social Democratic trade unions in Western Europe, sought to develop with police aid a similar purely economically-oriented labour movement in Tsarist Russia. Somehow or other the fine line between economic action and political action was to be drawn by these police-sponsored unions.

Surprisingly enough Zubatov received support from higher officials in the Ministry of the Interior for his scheme. As a result, in 1901, police unions were organized in Moscow, predominantly among textile workers. The high point of this effort occurred on 19 February 1902, when 50,000 workers connected with the Zubatov movement paraded before the Kremlin in a loyal outpouring of support commemorating the emancipation proclamation. Soon after this, the Zubatov movement spread to the south to ensnare Jewish workers in Minsk and Odessa.

Professor Schneiderman is of the opinion that the Zubatov movement had real prospects of success before 1905, were it not for the opposition of Witte and others in the Ministry of Finance, as well as that of their short-sighted industrialist supporters. He thus takes issue with Lenin, among others, who saw the *Zubatovshchina* as being merely a clumsy police provocation which would only raise the revolutionary consciousness of the working class by revealing how hopeless were the perspectives for bread and butter trade unionism in Tsarist Russia. Lenin seems to have been proved right, however, for the various political and economic groups associated with the Zubatov movement did in fact radicalize the workers. The most outstanding example of such radicalization was the Odessa General Strike of July 1903 which was instigated by a paid agent of the *Okhrana*, Shaevic, and supported by the Jewish Independent Labour Party—an *Okhrana* financed organization designed to draw away Jewish workers and artisans from the Marxist Bund.

Whatever attraction police-sponsored trade unionism may have had for the Jewish proletariat in the Pale was rapidly ended by the Kishney Pogrom of April 1903, sponsored by the head of the Ministry of the Interior, Plehve. So what then did the *Zubatovshchina* accomplish? In the eyes of this reviewer, nothing but the opposite of what its founder intended.

These views are not shared by Professor Schneiderman, however. But much support for a negative impression of the Zubatov movement is supplied by his excellent and thorough study of the subject. A vast

mass of details concerning the pre-1905 labour scene in Tsarist Russia has been assembled with painstaking care, to the point where we can finally grasp the intricate nature of labour relations existing at that time. But there are some deficiencies in this book on both the expository and theoretical levels. The role of the industrialists in opposing the Zubatov movement is not developed in sufficient detail. This neglect may stem from the author's failure to appreciate the fact that creating any kind of a labour movement in a backward, semi-colonial state like Tsarist Russia may have had revolutionary repercussions. In the West, trade unionism could win significant concessions from employers, particularly where accompanied by democratic political initiatives through parliament. Also surplus profits, gleaned by industrialists from colonial investments, could be used to alleviate the demands of the metropolitan working class. But none of these factors were operative in pre-1905 Russia. Hence the sponsorship of trade unions by the *Okhrana* only pointed up the incompatibility of open trade union activity in an autocratic society, and thus hastened the final confrontation.

Nonetheless, despite these shortcomings, Professor Schneiderman's book is the best work on Russia police socialism that has been written in recent times.

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- J. W. SCOTT, *The Glassworkers of Carmaux. French Craftsmen and Political Action in a Nineteenth-Century City*, Cambridge, Mass., Harvard University Press, 1974, pp. xiv + 239.
- R. G. GEIGER, *The Anzin Coal Company, 1800-1833. Big Business in the Early Stages of the French Industrial Revolution*, Newark, University of Delaware Press, 1974, pp. xiii + 345.

Upon receiving these two books I had intended to write a review chiefly comparing one to the other, but as it turns out, the possibilities of comparison are limited. They represent two different approaches to historical knowledge. Geiger's work is primarily economic history with ample attention given to its technological facet and to increases of productivity and profits. Scott's book, on the other hand, is mainly social history, with the result that managerial decisions, along with changes in techniques of production, are important only to the extent that they influenced social structure and the conditions of labour. Yet there is some overlap and it is on this that I shall concentrate.

Geiger's research strikes me as particularly important. For one thing it supports the argument that there were eighteenth-century noble land-owners who actively engaged in industry — coal mining in this case — and he found in the North not one but several types of enterprisers, from traditionalistic to dynamic. Scott found an equally active noble entrepreneurship in the southern glass-making industry. It is significant that both the northerners and the southerners managed to combine progress with paternalistic labour policies until distinctly bourgeois managers appeared who were more aggressive in technological innovation and pursued *laissez faire* capitalistic policies vis-à-vis their workers. For Geiger, however, this was an economic matter; for Scott a social matter. In the Anzin basin, this shift resulted from decisions made in Paris by the banking concern of Casimir-Perier; in Carmaux it came about through the local owner's efforts to restore economic, social and political control in local affairs.

Both books are innovative within the confines of their topics, that is, particular enterprises. As regards the Anzin coal company, Geiger found a managerial team sufficiently dynamic for him to modify somewhat the long-held view that French industrialists were too complacent and conservative. Hostility to innovation came rather from the workers. He also reveals how difficult it was for one industry to introduce machines (the steam engine) when the machine-building industry was underdeveloped before mid-century, and a skilled body of engineers and technicians was not readily available. Technological innovation was not lacking, but neither was it a major concern of the company. Anzin was hardly a pioneer; in fact, it lagged behind mining enterprises in England, Belgium and Germany, introducing new techniques chiefly when they were required by government regulations or the threat of losing markets to competitors. On the whole, Geiger believes that technical change took place only as required to preserve productivity and prevent costs from rising. Anzin management, protected by high tariffs on foreign coal, was not exceptionally active in expanding sales either. In this respect he supports the widely held view that French industrialists preferred high prices and profits to expanding their volume of production and sales; a stable market was their goal, not a rapidly expanding one. The Anzin mining firm, then, was not too far removed from the traditional views on French business. Labour saving devices were applied for limited economic goals, at least until the 1830s. Geiger does not indicate the effects of these policies on the coal miners who appear as cogs in the total productive process.

On a different level of generalization, Scott argues that mechanical innovation in glass making in the 1880s both expanded production and had a marked effect on the labour force. Her volume is devoted to proving her thesis that highly skilled glass workers reacted adversely to new machines. When using traditional methods of glass blowing they were

the highest paid of all French workers, and enjoyed exceptional autonomy regarding procedures, hiring and training, even in sizable enterprises. When new devices threatened to change all this, they organized a trade union and resorted to strike activity. Their aim, however, was not that usually associated with strikes by factory workers or miners; rather it was to preserve the traditional method of glass blowing and their skills, as well as their high status and image. When they failed, they set up a cooperative glass works in Albi, but then discovered that their new venture, to compete successfully, had to adopt the new technology. The result was their gradual retirement from the trade and their educating their children for other jobs, often in the bureaucracy or liberal professions. They did not choose to become proletarians in factories, as did other artisan workers whose skills were made obsolete by machines.

I consider both books to be first-rate monographs, containing much significant information about their respective industries and regions, innovative, and indispensable to scholars working in economic and social history. Both reveal that several widely held generalizations do not cover all cases—that there is need for some rethinking about the dynamics of management and the effects of mechanization on skilled workers.

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