REVIEWS OF BOOKS

W. O. Aydelotte, A. G. Bogue, and R. William Fogel (eds.), *The Dimensions of Quantitative Research in History*. Princeton, New Jersey: Princeton University Press, 1973. Pp. ix, 435.

The first in a series of books sponsored by the History Advisory Committee of the Mathematical Social Science Board (MSSB), this volume is intended to encourage the application of mathematical methods to historical analysis. Designed as a "teaching vehicle", the purpose of this volume is to demonstrate through example, "not by abstract discussion of methodology", how quantitative methods can be used in historical research. Covering a wide range of subject matter and analytical diversity, the nine essays in this volume are intended to reveal the current state of the art, a sample of the "freshest and technically most promising work being done at present in quantitative history". Preliminary versions of these commissioned essays were presented initially at a MSSB Conference at Harvard University in June, 1966 and the "penultimate versions" at a second Conference in June, 1969 in Chicago.

Each of the essays is introduced by the editors and placed quite competently within its proper historiographic context. Lawrence Stone and Jeanne C. Fawtier Stone, in an imaginatively constructed project, examine the transference in ownership of country houses in Hertfordshire between 1540 and 1879 in an attempt to trace the extent to which members of the British middle class penetrated the ruling elite. Social mobility is also the theme of Stephan Thernstrom's study of various ethnic groups in Boston from 1880 to 1963. While findings may be disputed, due to a limited sample and the difficulties of data linkage, the creative use of data and the conscious application of

social theory are praiseworthy. Treating social mobility as an independent, rather than dependent variable, Gilbert Shapiro and Philip Dawson analyze the relationship between middle-class radicalism and opportunities for advancement into the nobility in France in 1789. Charles Tilly examines the relationship between French industrialization and mass violence in the years 1845 to 1855. American political behaviour is the subject of two studies, as Gerald H. Kramer and Susan J. Lepper attempt to construct a multivariant linear model to ascertain the effects of certain measurable factors on electoral behaviour from 1896 to 1964, and Allan G. Bogue skilfully assays the difficult task of measuring the relative distribution of power in the Civil War Senate. William O. Aydelotte, in a judicious exploration of the difficulties of evaluating legislative behaviour, investigates party divisions in the British House of Commons in the 1840s. The final two essays deal with the relationship between economic and political factors. J. Rogers Hollingsworth and Ellen Jane Hollingsworth analyze public expenditures in nineteenth-century American cities, and William Fogel and Jack L. Rutner attempt to construct a theoretical model to measure the efficiency effects of the American government's federal land policy from 1850 to 1900.

None of the essays approaches substantive definitiveness; most, as the editors readily admit, represent preliminary reports on more extensive research projects. The authors' hypotheses, even their data in some instances, are presented in a tentative, exploratory manner, and the text throughout is permeated by careful, well-qualified understatement. These are by no means fatal, or even severe shortcomings. Those who use formal methods, as the editors point out, "do not, if they know their business, pretend to have achieved finality", because universality and finality are "illusory goals". The undergirding philosophy of this volume and a great many of the practitioners of quantitative analysis, rests on the propositions that, first, "What is attempted in quantitative research, as in other research, is not full knowledge of reality but an increasingly closer approximation to it..." and, second, the value of statistical techniques, where they can be applied, is that they provide systematic, efficient, and replicable procedures for the analysis and deployment of data and thus "an accurate means of seeing where we stand, how far the emerging generalizations require to be qualified and how significant are the exceptions to them".

It follows from these premises that no amount of statistical paraphernalia can camouflage a poorly conceived, loosely reasoned research design Indeed, the design is the key element. All of the essays in this volume are of a uniformly high quality in this regard, but the essays by Tilly, Thernstrom, and Shapiro and Dawson are particularly noteworthy because of the admirable degree to which they suceed in assimilating data theory, and hypotheses into historical models. Tilly, for example, begins with the general theories of modernization and then develops and tests a model of industria-

lization and organized violence. Each step of his research strategy is carefully delineated and qualified, yet the procedure is sufficiently flexible that, in the course of testing and rejecting one model, he is able to generate another. Having delimited the problem theory-wise, alternative solutions become more apparent, and these, in turn, may be tested systematically in terms of their adequateness for explaining the critical questions left unexplained by the original model or theory. This is the gist of the historical-scientific process, and, as demonstrated by some of the papers in this volume, it can function with great fluidity, if the proper theoretical framework is in place from the start. As a demonstraion of this process, then, some of the essays are excellent examples.

Yet, if the overriding purpose of this volume was to illustrate by example the advantages and limitations of quantitative analysis of historical data, there are a few drawbacks. The statistical explanations in the text, seemingly important in a book of this type, are uneven at best, unclear or non-existent at worst. Only those readers with quantitative expertise and background in the fields of specialized subject matter can be expected to profit from some of these essays. In particular, the Kramer and Lepper and Fogel and Rutner studies assume a great deal of statistical sophistication on the part of the reader and, contrary to the announced purposes of the volume, ignore the general reader completely.

The value of the essays as teaching tools is diminished further when one considers the extraordinary nature of most of the research projects undertaken in this volume. Considering the obvious amount of individual effort, research assistance, and external funding required to complete these studies, it would impracticable to expect, say, students in a graduate seminar to attempt similar efforts. Despite advances in data processing and the generally more acquiescent attitude among historians, the quantitative analysis of historical data, leading toward the solution of substantive historical problems, appears destined to remain within the province of a few. Rather than seek to include quantitative analysis within the standard repertoire of the historian, this volume, taken as a whole, seems to ask that the newer techniques and their adherents continue to be judged by different standards. The succeeding volumes in the series, which focus on specific problem areas, doubtless will alleviate some of these misgivings.

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I. T. BEREND and G. RÁNKI, *Economic Development in East-Central Europe in the 19th & 20th Centuries*. Columbia University Press. New York and London, 1974, pp. xiii, 402.

It would be a bit too facile to say that this book is the best survey of east-central European economic development in western languages, because

it is the only such survey. Being the first of its kind, however, makes it a vital work, and although it has some limitations, it is an excellent beginning for familiarizing readers with the major features of east-central European economic history in the nineteenth and twentieth centuries. The work is actually a translation from the original Hungarian study, published in 1969. The period dealt with is 1800 to 1949, and the areas covered primarily comprise those of the present day states of Austria, Bulgaria, Czechoslovakia, Hungary, Poland, Yugoslavia and a touch of the USSR.

What is most useful in the work is, of course, the fact that it presents to readers not familiar with the many languages of the area a useful tableau, as the authors put it, of « the main tendencies, the common features, and the special peculiarities of economic growth » in east-central Europe. For this reviewer, the best and least survey-like sections are those devoted to the problems of transforming the agrarian sector and to the relationship with Germany, particularly during the inter-war and second world war periods.

With all due praise, there are still several shortcomings which should be pointed out. The first is a tendency at times to let political predisposition guide the interpretation of the data. To cite several instances, the discussion of the period of the sixteenth to the eighteenth centuries as an era of transition not « from feudalism to capitalism but rather a peculiar, belated feudalism » (p. 6) raises a number of questions, not the least of which is the difficulty of accepting the existence of "feudalism", an often amorphous term in both Marxist and non-Marxist writings, in this entire area. Further, one would like substantiation of the idea that foreign capital was motivated in investing in the region by "the endeavor to win power and influence" (p. 234), rather than, or together with, profit considerations. Again, there is a tendency to confuse social and economic developments. This is particularly true of discussions of land tenure and economic growth. On page 290, for example, the authors argue that the existence of both large and dwarf holdings retarded agricultural development. While it is clear that there was often an unjust distribution of land, which is the point that the authors wish to make, it is not so clear that large holdings, *per se,* would retard agricultural growth. In fact, from an economist's point of view, one might expect economies of scale which would have made the opposite to have been the case in some areas, as was true for parts of Russia.

A second type of problem arises in that there is at times a seeming loss of overall perspective and awareness of the macro-economic processes. While the authors say that they attempted to avoid writing a detailed economic history of the region, country by country, nevertheless the reader is at times in danger of being overwhelmed by micro-economic details and description which are not clearly related to a picture of the main processes. In brief, a clearer idea of the overall picture would have been most helpful. The authors state that they sought to "delineate an East-European economic model" (p. viii)

but it is difficult to determine from the text just what the model is. A bit more analysis mixed with the description would have helped a great deal, as would a concluding chapter with something of an overview. This lack of perspective and analysis leads at times to confusion, such as the discussion in Chapter 9 of the immense problems of each country in the nineteen-twenties, which is followed by a conclusion, not explained, of how great growth was in these areas during this period! (pp. 240-241). This same type of confusion arises particularly in the hazy picture of the role of foreign capital, mentioned in another context above. On page 211 we are told in a discussion of the twenties that "in the postwar years foreign capital was not forthcoming", and yet in subsequent pages we are told several times of the important role of foreign capital in the economy, and that, in fact, "in most countries of East-Central Europe foreign capital had a 50 to 70 per cent share in financing the economy during the postwar decades (p. 237).

The errors described here may arise from the problems of collaborative writing or oversight, but I think that more attention to the overall framework would have alleviated most of them. And most important of all, these few errors in no way detract from the conclusion that the work is of extreme worth and is an impressive landmark in the writing of the economic history of this important and much neglected area.

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G. BEST, *Mid-Victorian Britain, 1851-1857*. Schocken Books, New York: 1972. Pp. xi + 316.

In the introduction to this book the author states that he « simply wanted to know what it would have felt like to be alive in about 1850 and how different it would have felt about a generation later ». Whether one man can subsume the individual experiences of an entire society is a debatable point. If Geoffrey Best has succeeded in this daunting task only he can tell. As for translating this experience to the printed page perhaps we ought to accept that only the novelist will ever be in a position to cater for our needs in this respect. This is not to suggest that Professor Best's only competition consists of fiction. We should bear in mind his own suggestion that Dickens may tell us more about "what it was like" than any Royal Commission.

This quibble aside, the volume can readily be accepted for what it is; a stimulating and eminently approachable survey of the fabric and dynamics of British society in the third quarter of the nineteenth century. It will appeal to the undergraduate and his teacher without alienating the informed general reader, its style being open and yet convincing. Profes-

sor Best acknowledges that he has utilised less sociological theory than some might consider necessary but doubts its real value had he known any more. Thus in some ways this work escapes from a potential terminological straitjacket; yet this lack of sociological insight has led to some minor but irritating errors. In chapter two, for example, the concept of middle class appears to be directly equated with a certain income bracket. Generally speaking, however, the scarcity of semantic speculation will ensure the book's widespread acceptance not only by students of social history but by those of economic, political and cultural history. It will also be of value to the student of nineteenth-century English literature, filling in on background. All these readers will find the treatment of the abundant source material on the period easy to digest, the often racy narrative being frequently spiced by contemporary quotation, from novelist and Royal Commission, from social investigator and Times correspondent.

The book is divided into four sections which set out, in turn, to look at British society at home, at work, at rest and within the context of the social order. The underlying theme is of the prosperity and stability experienced in Britain between the Great Exhibition of 1851 and the onset of the "Great Depression" in the mid-1870s. This was a time when foreign observers like Hippolyte Taine could enthuse over "the British system", with its lack of revolutionary tumult and its apparent gradual solving of social evils through popular consensus. Leaving behind the uncertainties of industrial revolution, Luddism and Chartism, the mid-Victorians looked forward in a self-congratulating way to further advance and consolidation. In this rosy mood Best concludes that on the whole, living standards must have been improving through this period for the bulk of the population, a supposition attractive to most other writers on the period to date. However, there was still much room for poverty to linger in the darker corners of Britain's growing cities and towns, as the first chapter of the book ably shows. Here Best examines housing conditions and the urban environment to great effect. The feverish growth of urban life in Victorian times, with its attendant benefits and problems, is something hard to exaggerate in the context of social change. Professor Best provides a well balanced synthesis of much recent work that has been done on the Victorian city.

The work continues with consideration of working conditions, wages and living standards. This part of the book is well illustrated with statistics, simple but understandable at a glance. The state of the poor receives mention, although the analysis of relief mechanism could have been extended. In chapter three we turn to the subject of leisure, although rather loosely defined. Education and religion are included under this heading. Would they have not been better included in that of social order? Apart from this, the chapter continues with a survey of reading habits, the rise of the

holiday and excursions, and the municipal park amongst other things. The final chapter turns to the core matter of social history, that of relating the individuals in society to the order. Here, some interesting ideas are raised about social mobility, class consciousness, and even, at a more mundane level, about the requirements needed to qualify as a "gentleman" in the eyes of respectable drawing room society. Generally, however, these ideas are not developed sufficiently for the specialist, although the undergraduate will find them a useful introduction to the subject.

This book has little new for the specialist. It has few glaring faults, but in conclusion one needs to be mentioned: the imbalance between town and country. In chapter one the urban environment, understandably impressive to the Victorians and their successors, has no less than 56 pages devoted to it. In contrast only 7 pages are allowed to cover the rural environment at a time when one in five worked on the land, and when village traditions and culture were fast being eroded. Nevertheless, the excellence of the rest of the work assures its place on the bookshelf of any serious social historian.

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G. FRECHE, *Toulouse et la région Midi-Pyrénées au siècle des Lumières (1670-1789)*. Paris, Ed. Cujas 1974, xviii, 982.

At a time when professionalism is becoming increasingly the rage in French universities, it is very good to find a book like this one by Georges Frêche which so clearly states the essential nature of our vocation: to make scholars think about the historical origins of the contemporary world.

A good part of the author's introduction is taken up with an explanation of the geographic limits adopted in the study, in which he shows the great contrasts between upper and lower Languedoc, and illustrates how dangerous it is for the historian to generalize from one part to another of this essentially artificial province. It is with upper Languedoc that he is concerned in this study. The work is, however, divided into two parts which follow on respectively from their titles: "An Agrarian World" (Part I)... "Which the towns control" (Part II). Following the practice and logic of contemporary regional studies, the first part of Frêche's book is concerned with demography. The two problems of central interest which rise from current research are, respectively, the extent of population increase in France in the 18th century and whether the means of subsistence expanded to keep pace with the rate of population increase.[1] The

[1] M. MORINEAU, *Révolution agricole, révolution alimentaire, révolution démographique*, in "Annales de Demographie Historique" (1974), pp. 335-374: see also the

two problems lie at the heart of French economic history in the 18th century and also are in many ways fundamental to the phenomonon of growth in the 19th.

Between 1730 and 1790 the population of upper Languedoc increased by about 45%, but although this was sligthly above the national average it is important to remember that the Toulouse region at this time was still in the more thinly inhabited part of France. Both the birth and death rates there were below the French national average (it is likely that the use of contraception had developed, but we feel more reserved on this after reading E. A. Wrigley, *Societé et Population* (1969) — especially as the female marriage age was not very delayed in the region). Demographic conditions in upper Languedoc were good in fact from 1720 to 1770, although a deterioration occurred very soon after this. But despite what is frequently claimed, even in these good years the effects of poor harvests were still very deeply felt: « In the mid-18th century the rhythms of death were still dictated by the movement of food prices », although it was maize that often cushioned this effect.

The author then turn to the masters of the land, for it was land that controlled all other activities. The tax registers provide a really exceptional source for such a study, but the author shows how they must be used with great care and precision to reveal their full economic and social significance. There was a clear increase in the number of large estates (over 60 hectares) in the Toulouse region in the 18th century, to the benefit of the nobility and the bourgeoisie, so that by the eve of the Revolution 84% of all estates in the Toulousain belonged to individuals not directly engaged in farming them. The peasants' holdings were relatively important in the fringe areas away from the main commercial routes — and this is easily explicable. The improvements in transport and communication (notably the construction of the Canal des Deux-Mers) drew the larger estates into commercial agriculture. The great landowners were both nobles and bourgeois, and began to give rise to the formation of a group of "notables" (for a contrast — admittedly referring to Provence and in mainly cultural terms — see: M. Vovelle, "L'Elite ou le mensonge des mots", in *Annales E.S.C.* 1974, pp. 49-72).

But was the land able to feed and support this increase in population which occurred? First the Toulouse region was subject to the "maize revolution" which did away with the fallow, made a triennial crop rotation possible, was often used for direct consumption and so made it possible to reserve the corn for sale. This immediately created new specializations — in corn in upper Languedoc and in wine in lower Languedoc, because

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review of the thesis by H. Neveux (*Les grains du Cambésis - fin du XIVe siècle, début du XVIIIe siècle*), entitled *Vie et declin d'une structure économique*, in the "Revue d'Histoire economique et sociale", 1974, n. 2, pp. 234-238.

the profits to be made from the sale of corn quickly diminished the number of vines in the Toulouse region. But the question is whether the production of consumable cereals increased or not. In fact, neither land reclamation nor the adoption of the "New Agriculture" produced any significant results. Yields hardly changed, and as a result the increase in cereal production, which the author examines very closely and is rightly suspicious of the tithe figures,[2] can only have been slight. The progress that did take place served to make good the collapse in production which had occurred in the last years of the reign of Louis IV — but there was no agricultural revolution in the "Midi-Pyrénées". The increase in agricultural production was in fact less than the increase in population. But how then can one explain why it was that subsistence crises seem to have been much less serious than previously? This was large due to improvements in transport and in the organization of commerce itself, so that Malthus was held in check, to the profit of Vauban.

The author then undertakes a highly detailed study of village society in which he shows how the class of agricultural wage-labourers grew at the expense of the group of owner-farmers, and that the hold of the bourgeoisie over the countryside grew rapidly, especially in the last years of the 18th century. This was also encouraged by the activities of the Intendants, and one section of the chapter is entitled "village rivalries or Class Struggle" — although the title is very attractive it is deceptive as the author does not deal with it at any length, although one cannot help asking whether there might not be an important problem here worthy of more attention.

The author then closes the first part of the study with a well justified chapter on the "Premises of a lay spirit" in which he shows how the influence of the protestant minority was out of all proportion to its numbers. It also appears that the peasants' faith was in a sorry state, while that of the bourgeoisie was heavily undermined by scepticism.

The second part is concerned with the control exercised by the towns and which is sketched out in the earlier part. In this area the attraction of investment in land was traditional, was clearly evident in the golden days of successful pastel production and remained so thereafter. But this was a process in which the members of the Toulouse Parlement "led the field". The Parlement brought together the elite of the landed aristocracy of the region, while the traditional nobility were increasingly eclipsed.

One of the most important aspects of urban power was the taxation of land and the exactions of the Church. Taxation began to increase noticeably from the mid-18th century, and seigneurial rights were partly re-

[2] In this context Georges Frêche has sparked off a very useful controversy which is echoed in the collective study *Les Fluctuations du produit de la dime*, Mouton 1972 (Association des Historiens Economists Français).

sponsible for this (also while revenue from industry was invariably underestimated that from land was consistently over-estimated). The tithe also seems to have been more onerous than in the kingdom as a whole, and was a source of great profit to the great tithe-owners. There are striking examples of this: in 1789 only 5.5% of the tithes of the chapter of Saint-Sernin went to the parish priest. But hostility towards the tithe, which was connected with the "laicization" of public opinion, did create major difficulties after 1750.

This was not all, however. Between 1735 and 1765 the division between different forms of agricultural income was shifted very much in favour of land rents, with the result that by the eve of the Revolution: « the State, the Province, the Church, the Nobility and the Bourgeoisie possessed, in one way or another, 62.5% of the rents on the land, leaving the tenant farmers, the sharecroppers and the labourers little over one third, even though they constituted between two-thirds and three-quarters of the population ».

The growth in the power of the towns was, as the first part of the study suggests, closely connected with the development of a commercial agriculture which had itself been encouraged by the improvements in transportation and communications. It was not only roads which were improved, but in particular the building of the royal canal "of the Two Seas" which allowed upper Languedoc to export its surplus of corn to the rest of the province and to Provence. It is also evident that a large number of the notables understood this well, for either directly, or else indirectly through the Estates, they helped finance the canal. The Languedoc Estates also played an important part in improving the road network.

The author then tackles the problem of agricultural prices but without, in our opinion anyway, explaining sufficiently the reason for treating this in the context of the second part of the study. Despite this, however, one could hardly refuse to recognize that Georges Frêche is one of the leading price historians even if one feels it necessary to add that the study of prices is branch of economic history which has become rather dated even though there still remains a great deal to be done. But that is the way of the world, and one fashion quickly chases out another even though its objectives have never been fully achieved... Fortunately, Georges Frêche's thesis does constitute a major contribution in this area, and he succeeds in drawing up systematically the price-indice for the 18th century. In his methodological discussion, however, he seems to overlook the value, for the earlier periods at any rate, of the debt receipts which are to be found in notarial registers (although they do need to be very carefully extrapolated).

What happened to the price of corn in the period then? From the mid-18th century it increased more rapidly on the Toulouse market than

in the country as a whole (hence "the triumphal attraction of landed rents"), but the "transport revolution" meant that corn from upper Languedoc still remained competitive on the markets of lower Languedoc, Provence and even Marseilles. Working in the opposite direction, the wines of lower Languedoc were always cheaper than those of the rest of the province, but thanks to the Canal could easily be drunk by the inhabitants of the Toulouse region.

In fact it was the case that "the whole life of the Toulouse region in the 18th century was organized around the grain trade". Unfortunately, however, the trade itself was dependent. With only a few rare exceptions, Toulouse was in general a town of brokers and agents rather than merchants — their employers were large merchants from Bordeaux and, above all, from Marseilles.[3] By the end of the *Ancien Regime* 9/10 of the region's commerce was in fact controlled from outside, and the main reason for this seems to have been its geographical disadvantages. It would also seem that the future under-development of the Languedoc region should be explained in the same terms.

The notables, in fact, showed little interest in this problem. What concerned them, understandably enough, was to reduce tolls and taxes on goods in transit, in other words to establish the free trade in grain which was achieved in 1763-4. Exporting to Marseilles was vital, and after several diversions the "free-traders" finally won the day in 1786. The result was that the trade in cereals — at fixed prices — increased by a factor of between 5 and 6 between the time of Colbert and Necker. In fact grain, although in part an export commodity, determined everything. It made possible the rise of a local aristocracy, led by the magistracy, which dominated the ownership of landed property. The "transport revolution" made export easier in the same way that demographich expansion facilitated production. But at the same time that this local aristocracy was growing rich, economic power was being taken away from the province... As soon as northern grain began to reach Marseilles in the second half of the 19th century, the province would find itself exposed to total ruin and defenceless.

George Frêche's study is then one of considerable scope and depth, and will take its place in the growing list of major local studies which have so greatly increased our knowledge of French history in the 17th and 18th centuries. But although this is an exercise in quantative history, it succeeds in avoiding the excesses which typify much of the genre. Quantitative analysis is after all nothing more than a highly privileged instrument. It is a pity that the book should have appeared at a moment when fashion has,

[3] For a "Marseillais" perspective see: C. CARRIERE, *Negociants marseillais au XVIIIe siècle*, Institut Historique de Provence (1975), 2 vols.

under the auspices of the College de France, drawn us back to qualitative history once more — but this in no way detracts from its merits. We can still hope, however, that the author will perhaps one day provide us with a study in which "the flesh" and everyday life are accorded priority. He owes it to his readers to go beyond the barrier of rather icy quantification, and in fact in his conclusions he lays out his case very well.

To revert to the time-honoured format of the review, there are certain formal points to make in conclusion. First, the term "Midi-Pyrenées" used in the title is not very satisfactory historically, any more than "demographic historian" used in the text (p. 70). Some of the footnotes are inaccurate (p. 288, n 2; p. 313, n. 4 and 5). Some of the maps and tables need improving, and the list of contents is incomplete as it does not refer to sources, bibliography, index nor the list of illustrations. One would also have expected to find mention in the bibliography of a number of works including *Histoire économique et sociale de la France (1660-1789)* which was edited by F. Braudel and E. Labrousse and which contains valuable points of reference, and also the fine studies by M. Vovelle and M. Agulhon on Provençal society at the end of the Ancien Regime and which provide so many points of comparison with the Toulouse region.

One final reproach, although one that is directed primarily at the editor — when a thesis of such outstanding importance appears, one that is likely to remain a primary source of reference for many years, why not provide some information about the eminent author on the cover, as is commonly done in England and America? There could be no more laudable reproach than that.

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J. F. GUILMARTIN, *Gunpowder and Galleys: Changing Technology and Mediterranean Warfare at Sea in the Sixteenth Century*. ("Cambridge Studies in Early Modern History"), New York, Cambridge University Press, 1974, pp. xiv, 321, $ 25 in USA.

The ghost of Admiral Mahan rose to haunt John Guilmartin when his interest in ballistics widened into a study of sixteenth-century Mediterranean warfare. Not Admiral Mahan himself, but naval historians dependent on him for ideas are responsible, Guilmartin says, for misunderstanding and misrepresenting naval strategy before the seventeenth century. Destruction of an enemy's main fleet did not then give a "command of the sea" in a "Mahanian" sense. Such command was impossible in earlier centuries when new warships could be built quickly and effective blockades could not be maintained either by round ship or by galleys. Since only oared vessels could take the offensive tactically, galleys dominated

Mediterranean war fleets. Quite correctly Guilmartin characterizes their naval actions as normally and essentially amphibious warfare, a felicitous formula. It consisted of raids and sieges, or lifting of sieges, in which the same men and weapons were employed on sea and land. His central problem is: What was the "impact of gunpowder weapons" on this "Mediterranean system of maritime conflict" (p. 5 and 67).

The most original part of the resulting study is the tecnological analysis of early naval artillery. John Guilmartin is not a product of the Naval Academy, although he is very conscious of the traditions of naval history at Annapolis and the technical studies made there of car-propelled vessels. He is an airman, a helicopter pilot. His backing and background are in the U. S. Air Force Academy, Colorado, supplemented by historical training at Princeton University and by highly technical advice and collaboration from experts at the Army Ballistics Research Laboratory at the Aberdeen Proving Grounds, Maryland. His explanations of the capabilities of the different kinds of guns, explosives, and projectiles are clearly and attractively presented and quite convincing. How the various types were actually used, when, and with what practical consequences is another problem, as he fully recognizes. He treats it less convincingly partly because of the obscurities inherent in the evidence, partly because of his failure to use directly Italian sources.

His answer to his central question is briefly as follows: gunpowder was added to the weaponry of warships without making any basic change in the Mediterranean system of maritime warfare. Until nearly the end of the sixteenth century the galley continued to be the dominant warship, just as it had been. But the increasing quantity of good bronze artillery in the sixteenth century called for larger and larger galleys which required more and more oarsmen, who had to be fed, even if not always paid. While social and economic developments kept raising the cost of propulsion by manpower, these developments, as well as technical improvements, powered the costs of propulsion through sails and of artillery. Finally the cheapness of cast-iron cannon gave a decisive advantage to the broadside-firing sailing ship.

Guilmartin's demonstration and elaboration of this thesis suffers from several weaknesses. He focusses narrowly on 1517-1571 although events of three centuries, roughly 1400-1700, need to be considered. He only hints at the first significant effect of gunpowder on the Mediterranean's amphibious naval warfare: shore batteries made fleets less important than they had been earlier in attacking or defending port-cities. No one so far as I know has yet analyzed naval actions during the fifteenth century to describe consequent changes in strategy. Guilmartin touches on it only in connection with the Turkish conquest of Constantinople and then (p. 80) so casually as to confuse the blockade of the Bosphorus with blockade of the Darda-

nelles. He takes up the story only in the sixteenth century. And he stops his narrative with Lepanto before cast-iron cannon were important in the Mediterranean, giving no illustrative actions to show how and when (or to what extent) the broadside-firing sailing ship proved its superiority within the Mediterranean's system of amphibious warfare.

The organization by chapter and sub-chapters also leaves much to be desired. He explains in a separate Preface the odd combination of subdivisions by which he has attempted to harmonize topical and chronological treatments, but his solution does not harness his material forcefully or avoid repetitions.

His decision to concentrate on a very brief "sixteenth century", 1517-1571, probably resulted from his lack of first-hand command of Italian materials, of either sources or the products of research or naval thinking. On the other hand, he has used Spanish archives to good effect. He sees strategic problems mainly from the Spanish point of view and illuminates Mediterranean naval history during the decades when Spain's role therein was most important.

Most welcome to an economic historian are his perceptions of the economic determinants of the techniques used in warfare and their intimate connection with the whole culture of the contestants. His many enlightening insights into these connections range far beyond the narrow period on which he centres his analysis. They make Guilmartin's book rewarding reading for every student of maritime history. For supporting evidence, as well as for stimulus in such perceptions, he owes much to the works of Fernand Braudel, and he relies on him and on one particularly rich footnote in Carlo Cipolla's *Guns and Sails* to support one crucial element in his main thesis (the cost of cast-iron cannon, cited on p. 175).

Such references makes the reader greedy for more evidence and Guilmartin does indeed supply considerably more regarding the costs of operating Spanish galleys. It is indicative of his confusing organization that this material (pp. 222-225) is not in one of his « topical chapters » but in one of his "seven chronological segments," namely, that entitled "Lepanto."

The graphs in which Guilmartin summarizes figures on costs found in archival sources are attractive and effective. Someone wishing to build on this data by fitting it together with evidence from other sources may well wish that tables had been presented with precise figures for precise dates on the costs of biscuit, gunpowder, etc. and one wonders what limitations or ambiguities in the material such a presentation would have revealed. The graphs suffice, however, to drive home his main point: When the ship became a gun carriage, propulsion by manpower became prohibitively expensive.

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P. J. JONES, *The Malatesta of Rimini and the Papal State. A political history*. Cambridge University Press, 1974. V, pp. 372, maps, bibliography, index.

Although this study is sub-titled "a political history", it draws from the very first pages on the sources compiled in the Romagna and its neighbouring provinces from the 15th century onwards and subsequently glossed by provincial scholars in the 18th century, so providing a mass of detail and in fact a micro-history. This is a study which is concerned with geopolitics, with society and with both public and private finances, rather than with "political history". One could even say that it is the history of a "scandal": how was it that a *signoria* came to be established with the alienation of a portion of the *Patrimonium Sancti Petri* in favour of a secular prince, an Angevin who was not only a Neapolitan but a Frenchman as well?

The issues which are here at stake are those of the political foundations of economic power in the late Middle Ages and early Renaissance, to which J. Delumeau devoted much attention. In fact from the end of the 13th century fiscal control over the Cernia salt deposits played a decisive part in the formation of the Malatesta proto-*signoria* (p. 15). The *contado* provided the second base for their power, and the early leaders of the family were also leaders of the new and recently urbanised nobility which quickly showed its opposition to the tradition medieval "captains". Because they were clan leaders, they provided a new style of government which contrasted with the anarchical power of the baronage. They were despots or tyrants in the fullest sense of the word, but they understood that the economic development of their territory and the spread of urbanisation, thanks to both of which the new city of Rimini was eclipsing the traditional centre of Ravenna, constituted the strongest imaginable pillars of their power, which was based on Rimini (or more exactly the Savio valley), Montefeltro, Urbino and the northern tip of the Marches.

The Malatesta first became known as chatelains of Verruchio in 1197 and as citizens of Rimini in 1206. They were *vaguely* Ghibelline — although this is not a real problem — and their first step towards the *signoria* was the office of *podestà* in 1248. P. J. Jones points out (pp. 31-33) that it was the very neutral colour which they gave this office which made it possible for them to stay in power through the reconquest of the Romagna by the Papacy (1263-78) and the Angevin invasion. This was the moment to choose between the Guelphs and Ghibellines, and this Malatesta da Verruchio did against Guido of Montefeltro while acting as defender of order (1282-8). But as a result of the remoteness of Papal power and of the constant mistrust shown towards the lay Neapolitan officers, the function of the *podestà* was expanded very considerably around 1334 by Malatesta Guastafamiglia. In addition to protector, he now became the

defender and *signore*. Henceforth the Malatesta were tyrants in the full sense of the word, "leaders of a territory" which stretched as far as Iesi, Ancona and Ascoli. Their title well expressed the compromise by which the bounds of their traditional office were superseded — Galeotto Malatesta was appointed "*gonfaloniere* of the Church", giving him temporal fiscal, administrative and feudal mastery over the lands of the Church lying east of Florence and Rome. This title was confirmed in 1356 and acknowledged the rise of the Malatesta.

P. J. Jones would no doubt agree were I at this point to suggest a comparison with the *"caudillos"* of the Spanish *Reconquista*. The tradition of *vexillifer* was revived in Italy in the mid-14th century. Albornoz and the Church legates certainly believed that it was a matter of relying on the tyrannical power of a loyal supporter against the always threatening leagues which were constantly being formed and reformed around Florence and Milan.

The author describes the Malatesta vicariate through the activities of Galeotto and Carlo Malatesta between 1355 and 1417. The institution brought together two distinct forms of power. On the one hand the *gonfaloniere* was the administrator (*rector*) of a demesne, in return for which he paid a certain rent. On the other, he was also at the head of a vast system of local patronage, and it was this that placed him way above potential rivals (such as Massa Trabaria, for example).

It was the period of the Great Schism which really put the institution to test, however. As Captain-General of the Church and as *condottiere*. Sigismondo Pandolfo Malatesta was victim of the double constraint arising from the two obligations and honours. He had to meet both his obligations of payment, and of military service against the Sforza, the Angevins and all the noble or citizen armies which Italy's internal divisions brought into being. Defeat lead to his condemnation in 1462 by Nicholas of Cusa (p. 231), which was a turning point in the history of both the *signoria* and the family, and a warning that the tyranny of Rimini was nearing its end.

The two final chapters (9 and 10) of this lively and informative study deal with the relations between politics and economics in the government of the vicarial *signoria*. The author first described the military, administrative, judicial and fiscal prerogatives of the vicariate *in temporalibus*. The ways in which authority and power were shared in the cities controlled by the family over the whole period is seen against the background of Papal favour and the extent to which it varied. In fact, the rise of these stateless statesmen was not the result of a steady climb and one must take into account the part played by nepotism, by the revivals of classical feudalism (Novello and Sigismondo were *condottieri* in 1434 and 1435), and by their professionalism. One can also see how slow was the development of the modern constraints of the state (*fumantiae, census*) which stretch back to

the mid-14th century. It was only with considerable difficulty that the Prince began to supersede seigneurial and feudal-vassal attitudes.

The "Mighty and Powerful Lord" of Rimini struck his own currency, imposed his will and discipline on men and office-holders in some thirty odd towns, as well as in more scattered possessions in Bologna, Florence, Ferrara, Lombardy and even Venice. The *monopolies,* such as those over the saltpans and deposits, provided economic and administrative unity amongst this mass of rights and possessions. Frequently it was the case that rights of a purely economic nature became confused with those over men to form an irksome means of controlling social orders and classes. Amongst these, the group forming the bureaucratic nobility — the *cittadini* — who performed the functions of court advisers, of privy or cameral councillors (*aulici, commensales* or *camerarii*) were the most clearly defined. Below them were to be found the inevitable class of administrative auxiliaries — the *fanterie paesane,* assize judges, commercial tribunals, were all aspects of the despotism which the Holy See regained by 1500.

The history of the Malatesta of Rimini in the 13th and 14th centuries is that of an attempt to create the powers of a political state and of the acceptance of its authority by a nobility which had spring from the medieval Papal vicariates. The last stages ushered in the world of Machiavelli and of that curious product of the Renaissance, the revival of Princely politics, of which P. J. Jones has here provided us with a truly magisterial study with an example taken from outside the great arena of the Arno valley.

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C. E. MALEFAKIS, *Agrarian Reform and Peasant Revolution in Spain: Origins of the Civil War.* Yale University Press, New Haven and London, 1974, pp. xx + 469.

Now six years old, Malefakis' book has already become a classic, widely reviewed and generally acclaimed. It deserves its reputation, for it is based on an impressive amount of research and is the most careful study of any major aspect of the history of the Second Spanish Republic. By showing the importance of developments in the countryside during the Republic, it forces a reassessment of the reasons behind the outbreak of the Civil War. More generally, it raises the Spanish case to a significant place among modern peasant revolutions.

Few reviewers, however, appear to have evaluated Malefakis' argument, which does not spring clearly from the wealth of information he provides. He deals with three different but intimately related subjects: the

struggles over reform legislation and their place in the political history of the Republic, the political organization and revolts of the peasants, and the structure of landholding and rural society. Although the last subject, which is of greatest interest to economic historians, occupies the opening section of the book, in Malefakis' reasoning it comes rather as an afterthought.

Primary attention goes to the conflicts in the Spanish Cortes. The main theme that runs through these sections is that the legislators vitiated an intelligent reform proposed by a non-political technical commission, which emphasized the settlement of tenants and laborers on farms carved out of the latifundia of southern Spain. Conservatives did so because they defended the large landowners, but Republicans and Socialists did so out of ideological intransigence and parliamentary opportunism, and because the prime minister, Manuel Azaña, was not emotionally committed to agrarian reform.

Malefakis argues that land reform was the critical issue of the Republic, a fact most republicans failed to appreciate. From his study of the agricultural classes, he concludes that the major revolutionary force among them was the landless workers of the south. Had they been won over to the Republic by a prompt distribution of land, as the technical commission proposed, they would have provided mass support that it needed. Instead their frustration, when worked upon by the Socialist trade unions, produced a vast occupation of estates in the spring of 1936 which brought chaos to southern Spain and destroyed the authority of the Popular Front government. One can no longer say that the left did not undertake a social revolution until after the military rising in July.

This much seems conclusive; what I find less so is Malefakis' corollary that the failure of the reform constitued a disaster. In his *Twenty-six Centuries of Agrarian Reform* (1965), Elias Tuma points out that until recent decades agrarian reformers tended to confuse the political objective of reform-to destroy a reactionary landed oligarchy-with the economic objective-to increase agricultural output and rural income-believing that the former would produce the latter. On the contrary, he says, in line with Gerschenkron, modern changes in landholding have had beneficial economic effects only when they have been in the direction of large-scale operation. Although Malefakis criticizes Azaña for framing legislation partly with the intention of destroying the *latifundistas* as a class, he tends to fall into the error Tuma points out. He judges the commission's plan politically imperative and in the process accepts as almost axiomatic that it was economically and socially sound. He follows the Spanish reformers in believing that small farmers were the best farmers.

Malefakis is aware, however, that he must justify this line of reform on more than political grounds. This is the major purpose of his preliminary analysis of Spanish agricultural society, which shows its oligarchic and caste nature and stresses the almost complete absence from Spain of the prosperous

one family farm. In a well documented chapter he argues that the southern latifundia are less a response to geographic conditions, as many have said, than the result of developments in the middle ages. Because geography does not impose large properties and because the owners were absentees and did not use capital intensive methods, Malefakis accepts that the latifundia should have been redistributed. He does not ask if the capital-labour ratio may have made the owners' decisions economically valid and redistribution impracticable.

Plans to make small farms went back to the eighteenth century, as Malefakis shows in an important appendix that may go unnoticed, but this was not the only course open to the republicans. Emergency decrees in 1931 raised farm wages and improved the terms of leases, creating a "profound change" in Spanish rural society (p. 263). Even more promising would appear to have been the policy of the dictator Primo de Rivera in the twenties, which Malefakis dismisses as a harmful "hiatus" in the tradition of reform. "For him the solution to the agrarian problem lay principally in the expansion of industry to draw off workers from the farms and in the construction of irrigation works to provide fuller employment for those who remained behind" (p. 436). To me these plans seem closer to the thinking of current development economists than the Republic's projects (although the republicans did also push irrigation). A dedicated cliometrician could undertake to determine whether the republican land distribution would have raised or lowered the per capita income of southern Spain. We could then judge better if in the long run it would have strengthened the Republic or helped the Spanish people.

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J. MAZZOLENI *Le Pergamene degli Archivi Vescovili di Amalfi e Ravello* Vol 1, 998-1264, with tables (Naples, Arte Tipografica, 1972), cxvi, 212, p. XXIV tables.

This work originates from a desire to introduce those studying the Italian Mezzogiorno in the medieval period to the curial writers of the south, drawing on the archives of the archepiscopal court of Amalfi and that of the bishopric of Ravello. But, by virtue of the perfect transcription, and glossary of the utmost value both to medievalists and to all those concerned with the history of institutions in general, which the book contains, the author succeeds in presenting a study of the juridical and political aspects of these documents, and an analysis of the functions and the role in the city of Amalfi of the *curiales*, the *scribae*, the *iudices et notarii* who drafted, redrafted and guaranteed both commercial documents and private charters.

Until the end of the 12th century there was considerable resistance to establishing a full curia in Amalfi, and it was marked by use of Neapolitan script which was rapidly distorted and simplified. Mlle Mazzoleni relates this to the longstanding municipal tradition in Amalfi, which had survived from Roman antiquity. In fact, Amalfi remained virtually a republican city even under the rule of its "counts", until 860. The city magistrates, the *prefeturi*, appointed at first annually, then for life, and finally on a hereditary basis, did not become a dukedom until 958. Coming into contact with the Normans in 1096, the city was taken by Roger Borsa in 1131. It was the Aragonese who finally introduced feudalization, but this was dismantled when the Duchy was sold in 1582.

Under the different political regimes of the 11th and 13th centuries, the *Ars Notariae,* or guild of notaries, remained firmly a caste of lay notaries despite changes in its outward appearance. At first an *Ordo Decurionum* with the duty of maintaining municipal customs, Justinian law and aulic ceremonies, the Guild slowly changed into an *Ordo Curialium,* as in Naples. Rather than a municipal corporation, it became an hierarchical college from which the Duke would select his proto-notary and his personal advisers. But the transformation from caste to administrative hierarchy was not accompanied by any decline in its power — in fact the contrary occurred. By the late 11th century the *curiales* always enjoyed the right to *complere* or *absolvere* without needing the intervention of ordinary judges. The Norman rulers also preserved the Curia, which was directed by a *stratiot.* It was in the 13th century that competition from public notaries began, evident in the spread of Gothic script, who imitated and undermined the Amalfi curia, The notarial court was then abolished by Frederick II and was not revived at the time of the restoration of Robert of Anjou in 1313.

Mlle Mazzoleni also reminds us vividly that in this part of Italy, the main influence in terms of style, forms and methods of drafting, remained Byzantine, even though the Amalfians had until the 11th century energetically opposed any Greek interference in the city's affairs. Not only were indictions for a long time those of Greek computation, but certain *titulatures* of the 1060s continued to perpetuate Byzantium (cf. Document 1, from 998).

The documents which concern the economic life of Amalfi (including sales, interesting contracts referring to arboriculture and plantations in the 13th century, wills etc) show the way in which from an early date specialized and diversified agriculture spread, starting from the numerous concessions of land made by the parishes. The world of the sea, however, hardly makes an appearance in the books of these learned city men — three contracts for the formation of different *Societates*, and a document by which Alexander IV confirmed the ownership by the Chapter and *communitas* of certain houses in Tripoli (doc. xcii, 1256) serve to remind us that history made the Amalfians willing protagonists in the great events taking place in Sicily and beyond the seas between the late 10th and the mid-13th centuries.

But the use of Byzantine currency right up until the end of the 13th century, when it was replaced either by Sicilian coins or else by local imitations of them, served to draw this society, which one might otherwise have considered to be marginal, back into that vast Mediterranean complex of men and things. And finally we are left with a picture which Mlle Mazzoleni has drawn in bold outlines (p. xxi) at the end of her description of the struggles to achieve control over the city which went on for over a century (from 958 to 1131) — the history of this coast is that of a steady deterioration, of a commercial decline in which the speed with which land changed hands is perhaps an indication that migration amongst the population was very high.

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R. G. Robbins Jr., *Famine in Russia, 1891-1892: The Imperial Government Responds To a Crisis.* New York: Columbia University Press, 1975, pp. 262.

The winter of 1890-1891 began early in Russia and was unusually severe. Premature frost, inadequate snow cover, and a cruel spring were then followed by drought and dust storms. The result was a serious crop failure; the all-Russian grain harvest was the smallest in a decade and 26 percent below the norm. In sixteen eastern and south-eastern provinces the situation was truly catastrophic. In those provinces cereal production declined by 45 percent on average, when compared with the harvests of 1888-1890. By the late summer of 1891 starvation and disease were poised for a murderous ride across the plains of Russia. Yet, despite some savage forays, that ride never really took place. The crude death rate in the sixteen most seriously affected provinces increased from 38 per thousand in the years 1881-1890 to 48 per thousand, and Robbins concludes that at most one percent of the forty million inhabitants of the sixteen provinces died « as a direct consequence of the disaster » (p. 171).

Robbins contends that losses were kept under control because the Tsarist government was able to plan and carry out a fairly effective food supply campaign, which relieved the stricken population. Thus the government could and did respond to a grave crisis. These conclusions are quite revisionist.

Conventional wisdom — an amalgam of myths, according to Robbins — maintains that the Tsarist government first tried to ignore and deny the food crisis. Then, under pressure, it acted incompetently to provide inadequate relief. Finally, despite the opposition of some officials, the government was rescued by the extensive relief efforts of local organizations (the *zem-*

stvos) and private volunteer groups, which combined to ease the suffering of the peasantry. Basing his study largely upon primary documents in Soviet archives in Leningrad and Moscow, Robbins convincingly presents a different account.

The government began to deal with the crisis at an early date. In February 1891 it was decided to reduce by 50 percent railroad rates on the shipment of grain into areas hit by crop failure. At their last meeting before the summer recess in late June the Committee of Ministers authorized special grants to the provincial *zemstvos* for the purchase of grain; and between July and October massive, direct grants of 29 million roubles were made. At the same time there were problems of evaluating information and coordinating state efforts with local *zemstvos*, which actually distributed most of the grain to famine victims. Thus it was not until the fall of 1891 that it was "obvious that only the shipment of enormous [additional] quantities of grain into the stricken provinces could prevent a major calamity." (p. 62).

Given the state's decision by the fall of 1891 to give adequate relief the highest prority before any budgetary or balance-of-payments considerations, transport of grain became the crucial question. The railroads serving the region of drought were few, and they were soon overburdened by the need to ship great quantities of grain to a traditional exporting area. Yet the establishment of a special Temporary Administration for the various railroads involved, which was charged with moving shipments at any cost and which was ably led by an ispired if dictatorial manager named Vendrikh, untangled the backlog of shipments in the dead of winter and allowed vital supplies to reach their destinations. Purely private efforts were of quite minor importance compared to those of the central government, which willingly cooperated with the local *zemstvos*. Robbins concludes that the reasonably effective response of the government in the famine year "suggests that Tsarist regime was more resilient, its grip on life firmer, than its critics, past and present, have maintained." (p. 176).

Robbins has made an important contribution to our understanding of the famine of 1891, and particularly the formation of government policy and the operation of state and *zemstvo* institutions. As he himself acknowledges, however, many aspects of the famine are touched briefly or not at all. The profound causes of the crop failure and the condition of agriculture in general are scarcely mentioned. Robbins simply accepts as axiomatic that the famine indicated the desperate plight of Russian agriculture and the peasantry. Nor does he discuss how public opinion viewed the calamity and analyzed its significance. In this connection, he only alludes to the contemporary debate among educated Russians of the time over whether Russia actually experienced a "famine", with its connotations of mass starvation, or a "crop failure", a natural catastrophy for which relief could be provided. Robbins apparently sides with those who insisted that a real

famine, and not a crop failure, occurred in 1891. Yet, on balance, the data he presents seem to support the contrary view.

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L. G. SANDBERG, *Lancashire in Decline: A Study in Entrepreneurship, Technology, and International Trade*, Columbus, the Ohio State University Press, 1974, pp. xv, 276, Tables, Appendixes, and Index.

Professor Sandberg is re-examining the development of the British cotton textile industry from the 1870's to the onset of the Second World War. Instead of producing another general industrial history, he has focussed on two major areas: the adoption of new technology and developments in overseas markets. This book is a critical evaluation of the behaviour of entrepreneurs in this important industry. It is specifically concerned with their alleged failure to adopt the most efficient technology and to be effective salesmen in foreign markets. Sandberg uses formal economic theory sparingly, but relies heavily on quantitative evidence. The result is a book strikingly similar to Donald McCloskey's *Economic Maturity and Entrepreneurial Decline: British Iron and Steel, 1870-1913* (Cambridge: Harvard University Press, 1973) in terms of methodology, organization, and conclusions.

The first third of this book is an examination of the diffusion of new technology in the industry prior to the First World War. British entrepreneurs were much slower than their American counterparts in adopting ring spinning after the 1870's. They used the newer technique extensively for producing yarn up to a count of the mid-40's, but used mules for higher counts, while American entrepreneurs used rings for virtually all yarns. Sandberg's data indicate that savings in labour and capital costs from ring spinning were significantly smaller in Britain than in the United States across a wide range of counts. In Britain, these savings were eliminated at counts above the mid-40's by the added cost of the longer staple cotton required for ring spinning, while this was not the case in the United States. The adoption of the automatic loom was also slower in Britain than in America during the two decades prior to the First World War. Sandberg argues that the expected rate of return from investment in automatic looms was much lower (c. 4% versus 11-15%) in Britain than in the United States. This disparity existed because of lower capacity utilization and stronger labour unions in Britain. Sandberg concludes from these cases that British entrepreneurs were economically rational in their choice of technology.

Much of the remainder of this study is a re-evaluation of the overall performance of the industry before the First World War, particularly with

respect to efficiency and to exports. First, Sandberg argues that G. T. Jones' efficiency index, which shows no increase in overall productivity in cotton textiles over the period 1885-1914, contains serious errors. A revised index suggests an improvement in the productivity of factor inputs (excluding cotton) of roughly 25-30%, approximately the same productivity advance achieved by the Massachusetts cotton textile industry during this period. He also re-examines the record of exports. By adjusting raw export data to reflect changes in the quality of exports, Sandberg produces a more favorable view of British export performance in the late Victorian era. This adjustment deflates the extremely high growth rates of the middle of the nineteenth century and increases the rate of growth in exports achieved during later decades. For example, the expansion of exports between the decades 1825-34 and 1865-74 falls from 744% to 490%, while growth between 1885-94 and 1905-13 increases from 28% to 42%.

The final third of the book is a detailed analysis of the industry's export record over the period 1815-1939. Sandberg disaggregation of British cotton textile exports on a regional and then on a national basis is illuminating. Well before the First World War, Britain was extremely vulnerable to the commercial policies of the importing countries. However, it was more significant that important new cotton textile industries were rapidly developing overseas in countries like Japan, China, and India, where domestic producers were *not* protected by high tariffs. Britain lost significant markets during these years, notably in Italy and Brazil, but these losses were offset by the growth of sales to India. This market accounted for only about one-tenth of British cotton textile exports in 1835, but by 1913, nearly half of all exports or one-third of British output was sold there.

Britain's disastrous export performance after the First World War was due in large part to the dramatic decline in exports to India. The critical development was the rapid growth of the domestic textile industry there. Sandberg does not deny the importance of Japanese competition, particularly in Asia, but argues that two-thirds of the decline in British cotton exports between 1913 and the late 1930's was the result of increased self-sufficiency by foreign countries. He also contends that Britain began to lose its comparative advantage in textiles relative to Japan and a few other countries about 1880. Cotton textiles were a labour-intensive industry and the relative scarcity of unskilled labour in Britain put her entrepreneurs at a distinct cost disadvantage internationally. Sandberg concludes that Britain's relative decline in cotton textiles was not the result of entrepreneurial failure, but was caused by factor beyond the control of her managers.

Sandberg's analysis of the industry's development is generally careful and convincing. Unfortunately, the estimates of production costs, which are the foundation of the analysis, are suspect bacause they are based almost

exclusively on secondary sources. The cost records of individual firms could have served as a valuable check on those estimates. It should also be noted that at several points, the book suggests that while British managers were rational in their choice of technology, given the economic framework in which they worked, the existence of that framework may reflect major failures on their part. For example, the higher transportation costs for yarn in Britain, one of the cost differences which made ring spinning relatively unattractive in Lancashire, was the result of the lack of vertical integration achieved in the United States. Similarly, the relative incompetence of British entrepreneurs in dealing with labour unions was a major cause for their reluctance to use automatic looms. Although Sandberg may have left some unanswered questions, this book is nevertheless a significant re-evaluation of the British experience in cotton textiles and will be standard reading for serious students of economic history.

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L. SCHOFER, *The Formation of a Modern Labor Force. Upper Silesia, 1865-1914.* Berkeley-Los Angeles-London, University of California Press, 1975. xvi and 213 pp.

A study of labour can focus upon a variety of problems. While in the past most studies concentrated on the labour movement, a growing number of recent studies deal with such questions as labour culture. Lawrence Schofer, a student of Hans Rosenberg, has chosen to center his study around "the workplace itself". This is a sound approach since all other aspects of labour history derive from this fundamental one, and there is still a lot to be done to clarify some of the most elementary questions particularly for regions which have been rather neglected by recent research.

Upper Silesia is such a region if we take into account that the work of Polish historians has only rarely been absorbed by Western scholars. It is one merit of Schofer's study to have made of these studies. But he has also made use of regional archives and he presents his own findings mainly from German sources. Political administration as well as ownership and management of the firms was German during this period, while the labour force was predominantly though not exclusively Polish.

Ethnic structure was reinforced by the fact that the Poles were Catholic while the German administrators were usually Protestant. This aggravated the class-cleavage but also complicated it. Economic issues tended to become loaded with religious and nationalistic overtones. This explains partly why labour unions came into being late and why they were divided. The German social-democratic unions could never get a firm hold in Upper Silesia except

amongst the predominantly German workers in the metal-working industries, and the Polish unions remained firmly in the hands of the Catholic clergy. Only in the last years before World War I was there common action.

This is one reason why Schofer did well to concentrate on the formation of the labour force rather than on its organization. He starts out with the growing demand for labour in the Upper-Silesian heavy industry, quantifies the growth of the labour force by branch as well as by sub-region and discusses the ethnic and occupational origins. The structure of agriculture and population migration rank high in his search for factors contributing to the growth from the supply side. Noting that the employers felt very strongly about a "shortage of labour", not only skilled but also unskilled, he analyses the recruitment policy of the firms and their drive to develop a reliable *Arbeiterstamm*. From there he moves to the conditions of work: hours worked and pay received. Finally he discusses some problems which contemporaries identified as major ones, the large turnover of the labour force and its frequent absenteeism. He does this within a framework of worker-management confrontation since he sees both phenomena as part of a conflict between employers and employees, similar to labour protests and strikes which forms the topic of his last chapter.

Throughout his book Schofer insists that problems of the labour force cannot be dealt with adequately if they are isolated from labour-management relations since the attitudes of labour are shaped by the behaviour of men who run the mills and mines. If a "modern labour force" is to be developed, management has to be "modernized" too. In this respect management of Upper Silesian industry failed badly. The employers wanted an obedient, docile labour force and relied on punishment rather than on incentives. What they got was an unruly work force with low productivity, endemic absenteeim and high turnover — compared with the Ruhr or other Western European mining regions.

This point is well taken, but Schofer does not explain why the Upper Silesian employers acted as they did when they knew that other regions like the Ruhr had better experience with higher wages and fewer draconian punishments. His employers and managers remain shadowy figures. I suppose much more could have been said about their motives, beliefs and techniques if he would have made them a more integral part of his study. But his workers do not fare much better, because their socio-cultural environment does not get adequate treatment. Another group is nearly totally lacking — the foremen, who carried on day-to-day dealings with the miners. Their behaviour was probably more important in shaping workers' habits and reactions than the upper-level managers and decision makers. And since they were mostly Germans who may have despised the uneducated, peasant-like, Catholic, and Polish work-force, and who treated them as sergeants treat recruits, we might find in their attitude an important key to

the apparent lack of productivity and modernity of the Upper Silesian work force. Equally lacking is a deeper analysis of the differences between firms — Prince Pless is occasionally mentioned as an unusual employer who cared more for his workers than others — and between different branches. Also the hierarchies within the firms are only mentioned but not studied. Thus some of the more exciting questions remain unanswered. Altogether this is a useful, but not very imaginative study which lays some groundwork on which others should build.

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R. J. SHAFER (ed.), *A Guide to Historical Method*. Dorsey Press, Homewood, Illinois: 1974. Pp. xiv + 255.

The university history teacher, marking a range of undergraduate and graduate writtten work, will appreciate at once the basic moral dilemma involved in attempting to control the construction, presentation and style of such work. On the one hand there seems to be an obvious need to introduce some control into the process of writing history - at least at undergraduate level. One indeed is often driven to wonder whether students have mastered the rudiments of grammar and spelling before reaching university. There will always be great scope for coaching students in the trade of history. In serving their apprenticeship they should at least learn to frame questions, collect appropriate evidence in a systematic way, assess that evidence objectively, then present it in a well-reasoned, systematic way, as well as in an attractive and readable style. Here comes the other side of the coin, however. How much are we entitled to dictate a style to a student? For this is what we will inevitably do. A certain monotonous regularity of style is often apparent in student essays beneath the masking methodological and structural errors. When ploughing through essays from a class of undergraduates it is the individual style and flair of a piece that usually impresses, rather than a neat, precise piece of work, but a piece which is boringly written for all that. It is on the horns of just this dilemma that the teacher is confronted with a book such as that being reviewed, and is left wondering exactly what to do with it.

The Shafer volume starts full of good intentions. In high style it reels off the reasons why « man views his past with boundless fascination ». It certainly provides some useful hints to the student on, for instance, the collecting of evidence, the use of an index-card system, in the writing of footnotes and bibliographies. But it should be made clear to the potential user at the outset that this is a guide and not a bible. Guides should be treated in a pragmatic way and not dogmatically. The sections on the use

of evidence, in particular, channel the reader into a fairly narrow perspective position. This could be dangerous in inhibiting the maturing of what might be called historical intuition, for want of a better term. This is a delicate plant, and confrontation with a volume like Shafer, with all its elaborate rules and procedures, reducing history to a series of steps like a dance, might be enough to ensure that the sapling never becomes a tree.

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C. TILLY (ed.), *The Formation of National States in Western Europe*. Princeton: Princenton University Press, 1975. Pp. xiv, 711.

The subtitle of this study sponsored by the Committee on Comparative Politics might well be: *Eight Political Scientists in Search of a Theory*. The search fails, as the editor acknowledges; but the collection of essays is valuable and could, in the end, contribute to its larger purpose.

The proximate task the authors set for themselves was (p. 48): « to compare the carrying out of some particular state-making activities in several major European countries over a substantial segment of the period since 1500 ». Behind this definition lay an historical question, a contemporary question, and an element, at least, of a political theory:

— How, in fact, were the major states of Europe built?
— Does that process illuminate state — and nation — building in the contemporary world of developing nations?
— The heart of the political process lies in the extractive — coercive activities of the state, with all their ramified consequences.

And behind these questions and the guiding theoretical hypothesis lay, in turn, both a general scepticism about recent political development theory and a specific reaction against its emphasis on "nationhood". Most of the authors, in Tilly's evocative phrase, have "sneaked back to the state". We are, for the most part, out of the world of Parsonian sociology, back in the harsher world of war and taxes and cops.

Samuer Finer sets the tone with a chapter on war and the military in the building of the European nations and states. He traces out in some detail the changing scale and methods of warfare from mediaeval times to the French Revolution, catching well the complexities of their dynamic interactions with fiscal policies, political alignments, and social structure. He adds a brief, schematic passage on the interaction since 1789, yielding, at once, modern concepts of nationhood, the welfare state, and total war.

Gabriel Ardant follows on with a deeper analysis of the methods and scale of taxation imposed by centuries of chronic warfare in a pre-industrial European society where tax revenues were hard to come by. He demonstrates how much of the structure of the various states and their political and constitutional arrangements flowed from the often losing struggle of the rulers for additional resources required for military purposes. Ardant also concludes with an addendum on the two centuries since Europe was transformed by the French and Industrial Revolutions.

Rudolf Braun pursues the same theme with a different purpose: to explain how the solution to tax problems was both shaped by and then helped determine differences in sociopolitical structure in Great Britain and Brandenburg-Prussia.

The extraction-coercion theme is then expanded. David Bayley considers thoughtfully what in modern history caused the contemporary differences in administration, style, and function of the police forces of Great Britain, France, Germany, and Italy. As the editor notes (pp. 58-60), this chapter might have linked better with the previous essays if it had focused on earlier times and applied a broader definition of the police power; for the police, or their equivalents, were much engaged in early modern Europe with tax and food problems. Charles Tilly next considers how the food resources for the cities, military forces, government staffs, and landless peasants were mobilized from the peasantry. He underlines the centrality of the food supply to the operations of the state and specifies the multiple consequences of this often extractive and coercive rather than commercial process. But he also notes the stimulating effect on agriculture of the requirements of London and Paris. In Chapter 7 Wolfram Fischer and Peter Lundgren tell us from what classes and how the administrative and technical personnel were mobilized in Britain (Fischer), France, and Prussia (Lundgren), to conduct the tasks of government.

Essentially unrelated to the argument of the rest of the book, Stein Rokkan tries his hand in Chapter 8 at a multi-dimensional, dynamic matrix designed to explain the political experience and outcome for state — and nation — building in Europe. It is rooted in a mixture of Parsonian functional differentation and Hirschman's classification of decision systems. It moves through time in four phases: state-building; the mobilization of the masses; popular participation; and welfare redistribution. I can only report that I found the structure unpersuasive. Other may judge it fruitful.

The editor (who introduces the volume in a long, initial chapter) then sums up, reflecting on the present somewhat chaotic state of political development theory, coming down hard on some of his contemporaries but disclaiming any definitive higher wisdom. He concludes with some observations on how the European state system, once created, spread its control out to the other continents, giving way in time to new nation states, which,

along with others, are drawn into a global state system, at just the time when the nation state may be losing the capacity to handle its classic security and economic functions.

My reactions to this serious but inconclusive exercise can be summarized in three propositions.

1. *In comparative political analysis the unknowns are always going to be greater than the number of equations.* Unique features of geography, culture, and historical experience in various parts of the world should lead us to expect great diversity in political patterns. Put the other way, we must be extremely modest about the limits within which we are likely to find uniformity in comparing historical sequences or cross-comparing contemporary nation states. (In my view, Rokkan's effort fails because it violates this precept of modesty).

2. *There are two fundamental flaws in trying to relate the contemporary process of political modernization to that of early modern Europe.* The central facts about the European states of that era are that they lived down to the 1780's essentially with traditional technologies, improved *ad hoc* from time to time, but denied the more or less regular flow of new technology which distinguishes the subsequent two centuries; and they were caught up in an arena of power which decreed that war would be endemic. Within these painful constraints, their struggles for revenue and food, the dispositions made with landowners who were often essential for the conduct of war, their efforts to build loyal bureaucracies fall rather more with the story of the ancient city states and empires than with the current developing nations. This was the correct perception of S. N. Eisenstadt (*The Political Systems of Empire*) to whose findings the authors of the present book might well have exhibited greater sensitivity. The contemporary nations live in a world with vast pools of technology available for application, experiencing average rates of growth in output per capita never known in Europe before the 1780's. They also administer populations which expect more or less regular increases in real income and welfare per capita — again a perspective quite different from that of the peoples of early modern Europe. The resources governments extract go in much higher proportion to investment and welfare purposes. Their military budgets vary a good deal as proportions of the budget and of GNP; but except in the Middle East and a portion of Southeast Asia, war has not been endemic despite the contentious world arena of which they are a part. We should not, therefore, expect great illumination of their political problems by re-analyzing the workings of the major European nations from the sixteenth to the eighteenth centuries.

3. *Nevertheless, the extraction-coercion process, at the heart of this book, is part of a correct approach to a political theory which ultimately*

could permit fruitful comparative political analysis. The authors, in my judgment, moved in the right direction in starting with war and taxes, although their framework is incomplete. In an important aside, the editor notes his retrospective regret that the administration of law and justice was not included among the function studied. The same regret might well have been expressed about policy towards the national economic base which, like tax policy, was related to war, but took on wider overtones as the mercantilist era proceeded, and about policy towards popular welfare, which was related to political order but also, in some places, was touched by simple equity considerations. What we need for a viable method of comparative political analysis is an agreed defintion of what the state is about. At all times and places, from African tribes to contemporary America, from Mao's China to Giscard's France, the state is concerned with three broad functions: external security; the constitutional balance between domestic order and the provision of justice; and the tasks of welfare and (where the concept exists) of growth. Scarce resources (including political capital) are mobilized and expended in these directions. They sometimes conflict and always interact. They stem, as Plato perceived and Freud reaffirmed, from fundamental contending aspects of the nature of man. "The state within us », in Plato's phrase, becomes the basis for the actions of collective political units when they are organized — simple or complex, in environments of technological stagnation or expanding technological frontiers. From Adam Smith forward to, say, Richard Musgrave, students of public finance have used these three categories in dealing with the functions and expenditures of states. A large body of statistical data now exists for comparative analysis. As I tried to demonstrate in *Politics and the Stages of Growth,* a systematic comparison of the political process in this widened tripartite input-output framework can help distinguish what was uniform and what unique among political units in traditional, transitional, and the various stages of modern societies. I believe this is also the proper matrix to study the supra-national security, constitutional, welfare and growth arrangements which have emerged piecemeal in a time when, as Tilly correctly senses, nation states by themselves are increasingly less capable of shaping their destinies.

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