
REVIEWS OF BOOKS

A. D. CHANDLER, Jr. and H. DAEMS (ed.), *Managerial Hierarchies, Comparative Perspectives on the Rise of the Modern Industrial Enterprise*, Harvard University Press, Cambridge, Mass., 1980, 237 pp.

Very seldom do I sit down to review a book when I feel like shouting from the rooftops: "Read this one." Here we have a study from which everyone interested in our modern Western culture, whether scholar or practitioner, can draw an abundance of understanding and information. He can find out where in modern society centers of economic power lie, the institutions which make these centers possible, the reasons for their coming into being, and examples of their rise in the major countries of Western civilization.

The object of my enthusiasm is a collection of seven papers which were read at various congresses of economic historians and presented in more finished form at a conference at the Harvard School of Business in 1977. Even then they were further criticized and revised until finally they were whipped into the shape in which they now appear. Not since I first read *The Modern Corporation and Private Property* by Adolf A. Berle and Gardiner C. Mears, which appeared in 1932, have I read anything on business organization from which I have profited so much as from this book.

The first essay here, by Alfred Chandler, Jr., is an account of the revolution in the ownership and management of business in the United States. It is an historical account which the other authors have used as a "model" with which they compare the experience of the country about which they are writing and which in one case, the essay by Oliver E. Williamson, is the object of very keen critical comments. Chandler maintains that the modern managerial enterprise has two major characteristics: (1) it has many distinct

operating units that could theoretically be run separately and (2) it employs a "hierarchy of middle and top-salaried managers who supervise the work of the units..."

No such business structures existed in 1840, but with the building of railways they began to make their appearance. For one thing, the corporate form of organization, which greatly facilitated the managerial enterprise, was given a tremendous fillip and the sheer size of the railways practically precluded the use of the former owner-manager type. Furthermore, railways were almost forced into separate units because of the geographical spread of their lines and by the areas they were built to serve. By 1880 the managerial form was recognized to be the wave of the future and was adopted in many different industries in order to reduce costs.

The experience of Great Britain was roughly similar to that of the United States. Germany, a late-comer in industrialization, passed through a relatively long period of cartel arrangements, which permitted a prolongation of owner-manager forms, but it eventually turned to managers. France, which had fewer mammoth enterprises than Germany, Great Britain, or the United States and which did not adopt the corporate form of organization so extensively as the others, did not enter the managerial system until the 1930's and more notably after World War II.

The reasons for the extension of the managerial enterprise arrangement are complex and varied. Fortunately they are summarized in a last chapter by Herman Daems, a Belgian economist and management consultant. Unfortunately they cannot be further summarized here, for he has reduced them to their smallest comprehensible size. What I would like to stress, however, is this: that which has taken place has led to a greater concentration of control over productive and distributive activity, especially in industry, and a further concentration of economic power. Managers to a large extent dominate boards of directors; they are paid extremely well, even though profits may be low or non-existent; they have such position in our culture that they do much to set the tastes in art and social values; and their successes have done much to influence education. They are verily a new segment of our society with which we must reckon.

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A. CLARE CARTER, *Getting, Spending and Investing in Early Modern Times: Essays on Dutch, English and Huguenot Economic History*, Assen: Van Gorcum & Co. B.V., 1975, pp. 179.

In introducing this collection Alice Clare Carter identifies its common theme as that of recognition of and response to business opportunities in the eighteenth century. There is another theme that also ties these fourteen essays

together. It is the identification and exploitation of unused or under used sources. In essays first published between 1934 and 1970, Carter directed scholarly attention to a number of British and Dutch sources dealing with aggregate economic man, especially as an accumulator and manager of certain types of property. It is well that Carter's essays should be republished (with some minor changes), for there remains much to be learned from these sources and about them.

In the main Carter has been interested in estate accumulation in the realm of financial assets around the middle of the eighteenth century, and in what can be learned from exploring such sources as the London orphans' inventories, which are especially useful for the half century or so after the great fire, British public debt office records, Amsterdam collateral succession inventories, and private collections or groups (London Huguenots) and individuals (members of the Belesaigne family of Amsterdam). Thus she has written on the organization of the British public debt in the eighteenth century, and on patterns of investment in it. One of the intricate problems in this field has to do with foreign investment in the British debt. Carter thought to shed light on that by synthesizing the contents of Amsterdam inventories of estates inherited collaterally, and therefore subject to a tax, for certain years between 1738 and 1800. These inventories suggest that the Dutch held considerably less in the British debt than the British believed, or liked to pretend they believed. In more recent work Carter's attention has turned to diplomacy, where she has considered intriguing questions about Anglo-Dutch relations in the mid-eighteenth century.

From another perspective Carter's contribution to economic history lies not so much in what she has had to say about these topics as in the way she has searched for evidence on them. For it is not merely that Carter has turned to seldom used sources but also that, early on, she showed how quantitative methods might be applied to some sources to increase their value. Her articles of 1953 on Dutch investment abroad alerted economic historians to the existence of the collateral succession inventories and explained one way aggregations might be derived from those inventories.

Thus this collection serves both to remind economic historians of Alice Carter's contributions to our understanding of the past (and to the methods and sources we use), and to bring together a number of essays of which several have become difficult to obtain. The reprinting of these essays also inaugurates a new series of books on the economic history of the Low Countries, especially in the seventeenth and eighteenth centuries, of which Professor Johan de Vries is editor.

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CH. E. FREDEMAN, *Joint-Stock Enterprise in France, 1807-1867. From Privileged Company to Modern Corporation*. Chapel Hill: The University of North Carolina Press, 1979. pp. xv, 234.

Pondering the possible reasons for the alleged weakness of early French industrialization, historians have generally given some weight to the awkward and restrictive legal procedures for organizing large businesses in the modern corporate form (authorization by registration, limited liability, easy transfer of shares). Over a decade ago Professor Freedeman struck several blows at this view in two articles and an essay. Now he has expanded on his earlier work to give us a fuller account of (1) the legal framework for the formation of joint-stock enterprises before 1867, (2) the administrative procedures required for authorization of *sociétés anonymes*, and (3) the statistical breakdown of such authorizations by industry and time period. All this is good to have in fresh detail, but Freedeman has also added important new material. About one-fourth of this slim volume consists of a 52 page appendix which lists chronologically the 642 *sociétés anonymes* authorized between 1808 and 1867, gives their initial capitalization, and provides references to the *Bulletin des lois* (which contains each company's charter and, usually, a list of original stockholders) and to the F¹² and F¹⁴ series at the National Archives (where authorization dossiers range from non-existent to voluminous). He also has added varying amounts of detail about the authorization of some of the more important *sociétés anonymes*. Most important, he has introduced, as a counterpoint to the rise of the *société anonyme*, the rise (and fall) of its rival, even substitute, the *société en commandite par actions*.

Freedeman makes no attempt to carry his companies' histories beyond the date of their founding. He has not attempted the enormous task of analysing the initial stockholder lists for patterns of investment by geographical, social or financial groups. And he does not claim to have delved beneath the surface of his mainly official sources to uncover the complex of personal, political and financial connections and power relationships that underlay the formation of the two most important categories of *sociétés anonymes* after 1840, the railroads and the investment banks. What he has given us is a narrow but neat, clear, and intelligent interweaving of the legal, administrative, and economic strands at the point of formation of joint-stock enterprises in early nineteenth-century France.

It is instructive to compare Freedeman's study with B.C. Hunt's old but still indispensable account of *The Development of the Business Corporation in England, 1800-1867*. Beneath the differences in legal forms and procedures, the French and British cases were remarkably similar. Even in the eighteenth century, businessmen in both countries found ways to circumvent restrictions on limited liability and easy transfer of shares of ownership. In France, the Revolution abolished all joint-stock companies, but Napoleon's *Code de Com-*

merce essentially restored the categories of business enterprise codified by Colbert's Ordinance of 1673. Was the resulting French procedure for administrative authorization of *sociétés anonymes*, culminating in a rigorous examination by the government's top administrative body, the *Conseil d'Etat*, more lengthy, costly, and uncertain than the English political procedure of authorization by act of Parliament? In any case, English law permitted (or winked at) a variety of means of acquiring limited liability and transferable shares outside the chartered corporation. The French *Code de Commerce* achieved the same results by creating a new form of the old *société en commandite*, the *commandite par actions*. Intended for middling-sized enterprises, this new form, to everyone's vast surprise, soon ranked as an alternative to the *société anonyme*. "Legal formalities for the formation of the *commandite par actions* were minimal". Its director *gerant* alone possessed unlimited liability in return for total control over the company's management. The other owners, viewed as sleeping partners, thus paid dearly for their limited liability. The *commandite par actions* was "a promoter's dream" (p. 48).

Until 1856 when a law attempted to curb abuses by providing means for the *commanditaires* to survey and control the director, the *commandite par actions* marched step for step with the *société anonyme*. It would appear that formations of *commandites par actions* were about ten times more numerous than those of *sociétés anonymes* in the thirty years before 1856, but the total initial capital for the two forms was about equal, and both forms were most prevalent in the same three sectors — transportation, banking and insurance, and mining and metallurgy. Yet one has some doubts. Freedeman's estimate for real capitalization of the *commandites par actions* is at best an educated guess (one-fifth of nominal capital), and it may well be that many an important *commandite par actions* was set up merely as a first step in the promotion of a *société anonyme*. It would be useful to have figures on the number of such attempted and successful conversions. The opposite was also true, of course; a promotion that failed to run the gauntlet to *anonyme* status often fell back on the easier form.

The law of 1856 tolled the death knell for the *commandite par actions* as a viable alternative to the *société anonyme* and thereby had the paradoxical effect of intensifying the demand for free incorporation. So too did the English law of 1855 which added limited liability to the formation by registration permitted by the law of 1844. French legal and bureaucratic conservatism crumbled. With the law of 1867 France became the second major European nation to provide a modern framework for large-scale enterprise. Twenty years ago in two articles that covered some of the same ground as Freedeman's study, Claude Fohlen stressed the unimportance of the *société anonyme* for early French industrialization. He pointed out that up to 1848 83% of total *anonyme* capital was accounted for by the transportation sector and 73% by the railroads alone, a point somewhat underplayed in Freedeman's analysis. Still, after

adding in the role played by the *commandites par actions*, Freedman is surely right in concluding that "French law on joint-stock enterprise, rather than constituting a barrier to development, facilitated it" (p. xv).

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CH. E. LINDBLOM, *Politics and Markets: The World's Political-Economic Systems*, New York: Basic Books, 1977.

Charles Lindblom undertakes in this work a carefully reasoned, systematic investigation of the relationship in the economic sphere between government and markets, both as they serve to strengthen one another and as they function as competing institutions that strengthen their respective roles at the expense of the other. Historical analysis and extrapolations into the future are both kept to a minimum. In the authors' prefatory words,

Most studies that attempt similar breadth ask Whence? and Whither? On the whole, I have relatively little to say about where the systems analyzed in this study came from, or where they are going. My attempt has been mainly to dissect and analyze those fundamental aspects of the systems that have been with us for at least several centuries and show every sign of persisting indefinitely".

The first several chapters are devoted to the analysis of Lindblom's fundamental elements. A careful, if brief, analysis of authority systems is presented, with special attention focused on the manifestations of such systems within the structure of the sovereign state. The exchange relationship that characterizes the market system is similarly analyzed at a level that assumes little specialized knowledge on the reader's part. Lindblom chooses to label his third organizing element "preceptorial systems", understandable as systems dependent on moral exhortation as the major legitimizing force. This latter system seems to be something of an *ad hoc* addition that is included to account particularly for the Chinese experience under Mao, the author apparently feeling that neither of the broader classifications would suffice in providing understanding. Similar extensions and qualifications of otherwise neatly compartmentalized analysis occur throughout the book but should not be understood as the apologetic actions of one who anticipates criticism that his original categorizations are not sufficiently inclusive to explain existing structures. Rather, Lindblom exhibits an honesty that compels him to alter and augment neat conceptual schema when the facts even vaguely appear to so demand.

In separate chapters, the different shortcomings of authority systems and markets as economic organizers are considered. Historical examples cover principally the present century; the Soviet, Chinese, and Cuban predicaments

brought on by the categorical rejection of markets as thoroughly bourgeois institutions is interestingly treated. The market failures cited are primarily those that gained attention in the early part of this century beginning with Pigou and in response to which neoclassical welfare theory originally was addressed. This section also provides several original rebuttals to the growing laissez-faire oriented public-choice theorists. While critics in the Marxian and institutionalist traditions spend little time in attempts to criticize the formal neoclassical analysis of micro-behavior, Lindblom does do so. In Chapter 6, for instance, he makes an intriguing distinction between "preferences" and "volitions" which is, to my knowledge, a distinction not considered by economic choice theorists.

In the next group of chapters Lindblom outlines different forms that market systems have taken and might yet take. His taxonomy in this section is particularly clear and useful. Market systems are divided into four exhaustive categories arising from the possible combinations of public vs. private enterprise as each be joined with either consumer or planner sovereignty. The extent to which any national economy is organized by an authority system rather than the market does not preclude those market elements it does possess being understood in terms of these categories.

The struggle over authority occupies much of the remainder of the book. Lindblom chooses the term "polyarchy" (of which democracy is a subset) to describe those systems in which the struggle is regulated by a distinct set of rules. The most clearly defined thesis develops at this point in the book.

After noting that there is no logical explanation for the fact that all existing polyarchies are market-system dominated (polyarchy presently being a sufficient but by no means necessary condition of a market oriented economy), the author argues that (a) the historical rise of polyarchies coincided with the rise of economic liberalism and can be understood as a means settled upon by liberals for their end, economic liberty; (b) the privileged position of the corporation in the West today has resulted in businesses exercising influence over elected officials far above that which they would enjoy as mere members of a constituency; and (c) to the extent that polyarchic control remains, the corporation exercises preponderant influence in shaping the opinions that ultimately determine polyarchic outcomes.

Though Lindblom's style of writing is clear and concise, there exists throughout the book a certain vagueness as to just what group is being addressed. There is first the measured tone of the synthesizer bridging the gap between academic fields. Largely because of the isolation of disciplines, the author remains aware that he can assume no common body of knowledge among his readers. But in attempting to convey the rudiments of price theory and elementary political science concepts the writing style at times falls into a patronizing style reminiscent of a Samuelson textbook. It should be stressed that in both of these capacities Lindblom is able and provocative; it is the

co-existence of the respective styles that gives rise to a slight awkwardness. While the assumption of his reader possessing a stronger background might have made the work more concise, there is the gain that the book can be used in courses in comparative political systems or comparative economic systems, while proving at least as valuable for the advanced reader.

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W. H. McNEILL - R. S. ADAMS, eds., *Human Migration Patterns and Policies* (Bloomington and London: Indiana University Press in association with the American Academy of Arts and Sciences, 1978), pp. xviii + 442.

If you are in a quandary about a suitable birthday or Christmas present for a school leaver or young undergraduate with inclinations towards the social sciences, this is it! Nearly 450 pages with a shiny cover and a good quality print of contributions in the main on international migration. Much of the world is covered — ancient China (J. Lee); Europe since the Middle Ages (Ch. Tilly, A.R. Zolberg, H.-J. Hoffmann-Nowotny); the U.S. tradition of "moving on" (P.A. Morrison and J.P. Wheeler) and of illegal labour inflows from Mexico (G.M. Rosberg); the crucial economic and social role of migration in Caribbean societies (O. Patterson); postwar movements in sub-Saharan Africa (P.D. Curtin) as well as the Arab region (J. Abu-Lughod); migration and political control in Soviet Central Asia (A.A. Bennigsen and S.E. Wimbush); rural-urban movements in South and Southeast Asia (T.G. McGee); hundered years of mass migration into Argentina (C. Solberg) and thousands of years of moves everywhere (in W.H. McNeill's historical overview). Most of these papers contain substantial information of interest to analysts of both the pure and the applied kind. There are also specialised contributions on modeling techniques relevant to emigration processes (A. Akerman); demographic and social contexts of international migration policies (G.J. Stolnitz); international human rights constraints (C.A. Auerbach); an economist's view (T.W. Schultz) that will leave many people unsatisfied; a moralist's view (M.E. Marty) that will satisfy few because it does not really get to grips with the burning contemporary problems; and a sociologist's view (E. Shils) that the reviewer found rambling and less than illuminating.

Quality and length inevitably differ in a collection such as this. I shall merely draw attention to three papers which readers might find of special interest.

Tilly's essay is, analytically speaking, perhaps the most ambitious. He sets out to show that Europe's history was characterised not so much by periods of mobility or immobility as by decisive shifts among different types of movements. He distinguishes local, circular, chain and career migration

in individual and collective versions but admits that he has not made all the distinctions he could think of. He does indeed draw a fair amount of explanatory power from these four types in his casual history of movements within and from Western Europe. However, the reader wonders how much of reality Tilly's typology can really explain when the four types portrayed in a mobility matrix (p. 51) seem to cover only a minority of all mobility cases. Or maybe it is his supply-side perspective that weakens the typology?

Zolberg has something to say on the supply side and it is worth quoting in full what might be called his policy-determination argument (which in the contemporary Western World with its right of emigration but lack of right of immigration can easily be seen to turn into a demand-side determination):

"On the whole, in a world of states, internal migration is controlled to a considerable extent by those who go'; as for international migration, the choices of individuals are very much constrained by the preferences of 'those who send' and 'those who receive'. Although specialists on migration have long acknowledged this reality... I believe that they have not dealt with it satisfactorily. The more analytic among them seldom take it into account in the elaboration of theories, most of which are founded on the axiom of individual choice. From that vantage point, the obstacles that states of putative origin and destination erect in the path of many who want to go, as well as the coercive measures that produce departures among many who would prefer to stay, are treated as events that lie beyond the explanatory domain of migration theories, as residual error does in an equation. But since the residual error accounts for more of the variance than does the equation, migration theories of this sort are, at best, useful as counter-factual models, suggesting what might occur under conditions of relatively free movement, and, at worst, trivial or even ideologically misleading. In contrast, the specialists who deal with the residual error component — emigration policies, forced population exchanges, expulsion, immigration policies and their concomitants such as naturalization law — tend to be a-theoretic. Produced mostly by historians or political scientists interested in a segment of social reality within specified time limits and in particular countries, by specialists of international law, and by students of international organisations, the literature on these matters constitutes an array of discrete bits." (pp. 241-2).

Zolberg provides 40-odd pages of empirical verification and leaves the reader with the feeling of having witnessed the first unveiling of a structure that will prove sound and solid.

To go back very briefly to the supply vs. demand controversy (which is not really treated as such in the various contributions but which is touched upon by almost each and everyone of them), it is of course necessary that there be candidates for migration; otherwise no move would ever take place. But it is not enough that an individual wants to move into another country. He will be shown the door unless there is a demand for his presence and the political power structure sanctions it. The important distinction here is bet-

ween necessary and sufficient determinants. This is not a matter of "either-or". It is the interplay of factors of different weight which shapes international migration.

Zolberg's piece ends with the Second World War, Hoffman-Nowotny's covers the Western European situation since that time. His is not a contribution one can recommend. His view, for which he claims "intellectual honesty" (p. 86), admits only one kind of migrant, namely the non-returning settler of the golden 100 years of immigration into the United States. He makes hay of the proliferation of terms in post-war Europe. He actually tells untruths when, for example, characterising the Federal Republic of Germany: "The policy of the Federal government explicitly aims at the rotation of foreigners and the postponement of integration". (p. 98). If the author had read but one of the many policy statements made year after year before his article was written, he could never have written such rubbish. (To name but three sources to prove the point: Deutscher Bundestag, Drucksache VI/3085 of 31 January 1972; Leitlinien zur Ausländer-beschäftigung of 6 June 1973; Förderung von Massnahmen zur Eingliederung ausländischer Arbeitnehmer in die Wirtschaft und Gesellschaft der Bundesrepublik Deutschland of 31 January 1974). Politically, one may have a great deal of sympathy for the "J'accuse" kind of article written by Hoffmann-Nowotny. Intellectually, however, he has done his cause, and himself, a disservice.

What terms and distinctions might be helpful in clarifying the labour migration scene outside traditional settlement moves and refugee flows? Under a demand-side perspective one could differentiate between *limited-time programmes* where temporary means what it says, i.e. only for a time. They are characterised by voluntary exit after a time or enforced departure when the time is up. In post-war Western Europe this type of labour migration was chiefly found among seasonal workers in France and Switzerland. *Guest worker employment* was the hallmark of Western Europe. The term itself is not official but very popular and, used correctly, it explains well what has been happening. Guests were invited to stay as long as they wished; they were expected to leave, eventually; but, barring political or social misconduct, they were not forced out. A third distinction concerns the practice of admitting *foreigners subject to restrictions* regarding, for instance, duration of stay or access to jobs. This, too, is different from limited-time programmes because restricted admission does not entail forced exit. All Western European countries have a progressive adjustment system where restrictions in matters of economic and social rights are gradually lifted. Sweden is so far the only country to have lifted the political restrictions on foreigners in elections according to the principle of "taxation calls for representation".

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- V. PEREZ MOREDA, *Las crisis de mortalidad en la España interior. Siglos XVI-XIX*, Preface by Jordi Nadal, Madrid, Ed. Siglo XXI de España, S.a pp. 526.

Moreda's study stands out as a work of undoubted scientific rigour amongst the spate of demographic studies which have been appearing in recent years. Its theme is the mortality crises, in both their standard and their catastrophic forms, affecting the provinces of the Spanish interior — the two Castiles, Leon, Extremadura and Aragon. For the XVIth century, the study is based on the parochial registers which were compiled voluntarily in many bishoprics and ecclesiastical administrations, many of which have been continued down to the present. From the XVIIth century onwards, of course, such registers became obligatory. Unfortunately not all the relevant registers were available, many having been destroyed or dispersed, while others are not open for consultation. This means that the study is based on a series of sample registers, but despite these limitations the validity of Moreda's conclusions is greatly strengthened by the geographical breadth of the area and the long time span from which they are drawn. The reader is provided with a vivid fresco of the regions studied, which utilises the registers of over seventy different parishes over a period of more than three centuries.

While previous studies have been concerned mainly with family reconstruction, Moreda concentrates on the problem of mortality and he puts forward a fresh interpretation of the phenomenon — not so much in terms of the methodology used, but rather in relation to the causal links between demographic crisis and catastrophic mortality rates. Traditional interpretations suggest that there was a close relationship between the demographic recession in XVIIth century Castile and critical mortality rates, and although these are seen as the result of exogenous factors, particularly subsistence crises, they are also considered to be independent in some way from economic mechanisms. But Moreda demonstrates that 'a comparison over time of the frequency, geographical extension and intensity of the mortality crises of the XVIIth century with those of the XVIIIth and XIXth centuries leads one to very different conclusions from the traditional view of the effects of catastrophic mortality rates on the demographic evolution of the periods in question'. In addition to the undeniable influence of subsistence crises, factors intimately related to the pattern of economic development also directly affected the size and distribution of the population.

Since the average intensity of the mortality crises in central Spain was not greatly different from that in other European countries, and was indeed in some cases rather lower, the author argues that although the influence of catastrophic epidemics was important in the short-term, these alone are not sufficient to explain the slow rates of demographic growth and even of falling population in certain of these regions. In fact, during the XVIth century Spain experienced a certain growth in population, and although this was never

explosive it was nonetheless significant despite the repeated epidemics which ravaged the country in the period.

For long periods the plague held sway unchallenged, and was accompanied by successive agrarian crises and the almost continuous rise in prices. The resulting poverty brought about a series of other consequences by the end of the century, and typhus, small pox and diphtheria all began to assume endemic and epidemic proportions. But the argument that the great Castilian epidemics were the cause of the demographic stasis of the central provinces is undermined by the fact that after 1600 the graph of baptisms in centres like Murcia and Ciudad Real which had not been touched by the epidemics began to dip. And during the XVIIth century the great epidemics had also extended outside Castile. Plague struck in Andalusia for the first time in 1507 and persisted there in almost endemic form throughout the 1520s and 1530s, before moving into Catalonia where it reaped innumerable victims and reached levels comparable with those of the terrible outbreaks of the 1590s.

On the other hand, the great plague of 1599 and 1602 affected Castile alone -- but this was the only major outbreak in the XVIIth century, and there was no further visitation in the inland regions even though both the Mediterranean and Cantabrian coastal areas (the Levante and Andalusia) were hit by outbreaks of plague spreading from Europe in the mid century which caused great damage. And although the main period of population decline in Castile occurred around 1620, Moreda argues that a drop in the baptismal curves is evident for the inland regions from about 1570. As Chaunu has claimed, this was probably the result of a form of cautious malthusianism, due to resort to *coitus interruptus* and abortion, both practices being widely diffused in rural Europe from the earliest times.

The plagues therefore did no more than strengthen tendencies which were already evident. And this is confirmed by a comparison of the situation in the interior with that in the provinces of the Mediterranean littoral (and one should remember that the expulsion of the *moriscos* in 1609 caused particular damage in the Levante). Felipe Ruiz Martin, for example, has shown that despite the population decline experienced in southern Andalusia in the XVIIth century, rural Andalusia never suffered the depopulation typical of Castile and Estremadura. Other factors were instrumental, therefore, in determining how population was distributed, above all emigration. The scale of emigration cannot be adequately quantified, yet there can be little doubt that emigration to the Americas was considerable and, as Moreda frequently explains, it is also probable that the appearance of plague in the interior at the start of the century encouraged movement towards the southern provinces if not to the New World.

Throughout the XVIIth and XVIIIth centuries the Spanish interior continued to suffer subsistence crises accompanied by epidemics and infectious diseases. The XVIIth century saw the spread of typhus, malaria and small

pox, while the following century witnessed an increase in infant mortality and more disease. Yet the XVIIIth century marked a shift from demographic stagnation to expansion in Spain. Between 1723 and 1797 the population rose by a little less than 4%, and although this was less than the European average it was nonetheless an increase and was distributed unevenly amongst the different provinces. To return for a moment to Catalonia, we have already mentioned that between 1635 and 1659 the province suffered from the multiple damage caused by plague, war, the billeting of large numbers of soldiers, and monetary devaluation. But Nadal and Girault have shown that the region was also attracting large numbers of immigrants from France, many of whom probably found their way to other provinces as well. Dupaquier, Armengaud and Reinhard have followed Vilar in estimating that the Catalan population increased by some 121% during the XVIII century.

The picture provided by the parish registers enables us to identify a number of cycles in this broader process. Between 1714 and 1738 there was a wave of expansion related to the relative abundance of food supplies, whereas population growth slowed down considerably between 1739 and 1763, to be followed by a further burst of expansion and another period of stagnation. The relative over-population which occurred between 1725 and 1735 caused agricultural wages to fall and agrarian rent rolls to rise. These two factors brought demographic expansion to a halt, so revealing the working of a form of self-righting mechanism.

But these phases in fact obtained throughout the peninsula, and are not in themselves sufficient to explain the different developments resulting in different areas. In Galicia, for example, despite the mild climate and the general fall in prices, there was major de-population, due mainly to emigration. In Catalonia, on the other hand, there was no interruption in the process of demographic growth, despite the severe subsistence crises of the early years of the XIXth century which were aggravated by the war and saw the diffusion of a small pox epidemic and other forms of disease closely connected with poverty and atrocious sanitary conditions. In addition, notwithstanding the cholera epidemic which hit the province in the mid-XIXth century, the demographic consequences of these great mortality crises proved to be insignificant.

By exploring and widening our knowledge of these problems, Moreda's study makes an important contribution to Spanish history, and provides a well documented and soundly argued analysis of the reasons lying behind the economic and demographic decline of Castile. In concluding one should also mention the statistical data based on the annual mortality records provided in the appendix, although the publication of the full series, together with the data on birth and marriage rates, is to appear in a future volume.

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L. POLIAKOV, *Jewish Bankers and the Holy See from the Thirteenth to the Seventeenth Century*, London, Henley and Boston, Routledge & Kegan Paul (The Littman Library of Jewish Civilization), 1977, pp. xii + 275.

Poliakov's research on Italian Jewish bankers, and their relations with the Holy See appeared in the original French edition of 1965; the volume now published, as the author informs us in the preface, is a more popular and less specialized edition omitting almost all the documentary sources and restricting the footnotes to bibliographical references. This abridged edition accentuates "motivational" aspects of the story (one could almost say the collective psychological aspect) for which Poliakov reveals a particular predilection, somewhat to the detriment of scientific and historical rigor.

In the original version, the work had limited interest as far as source materials were concerned as they were restricted to a sample of the Roman notaries and to two files of the *Congregatio de usuris* (XVIIth century) with only occasional recourse to documentary sources in Lucca and in the duchy of Mantua. In addition, the reader cannot overlook the author's failure to utilize S. Simonsohn's work on the Jews of Mantua, published in Hebrew in 1962 (and now available in an English edition).

Therefore, if in the French edition, the task of illustrating an interpretative thesis prevailed over historical reconstruction, this tendency is further accentuated in the present edition. Poliakov develops the theory that in the history of Italian Jews, ecclesiastic power had much greater influence than the State; and that therefore it is more useful to study the attitude of the Church through the controversialists and through Papal bulls. The political position of Rome is largely conditioned by local pressures and the pre-eminent factor often takes the form of sermons, especially of the mendicant orders. The political decisions by government confronted with the Jewish presence constitutes only a background element, so that the author can, "contrary to persistent legends", apply to the different Italian states between the XVth and XVIIth centuries the theory that "neither the increase in the number of Monti di Pietà, nor the strict measures the Counter-Reformation popes took regarding the Jews had any notable influence on the practice of Jewish banking, at least in the short term," quoting as proof the opinion of an anonymous consultation of 1670-75 (p. 82).

In the course of four centuries, Jewish lending, according to Poliakov, made very good progress: a sharp rise, a period of great expansion, then a long decline and stagnation. But economic factors (money market, capital and investments) and social factors (supremacy of a noble state over another, struggles between factions, wars and famines) were not as important as technical factors (on the basis of the treatise, *The Eternal Life*, of the famous banker of the XVIth century, Yehiel Nissim da Pisa), and, above all, theological positions, (Poliakov, a well-known historian of anti-semitism, returns here to the history of banks).

This lack of attention to the underlying political-social reality is at the basis of numerous inaccuracies, which makes Poliakov's thesis difficult to follow. There is little point in a simple enumeration of errors in the translation (Novellara instead of Nuvolara, p. 80; in Siena the "Casa della Misericordia" and the "Casa della Sapienza" are not separate foundations since the first is part of the second, which is the University; "Moise of Pavia" is from the Pavia family, but lives in Lodi, p. 116; the Spanish debt to the Lombard Jews was neither 32,000 nor 153,288 ducats, but 32 thousand scudi, which corresponds to about 153,288 lire p. 163). A more serious error, however, is the statement that "not that all Franciscans advocated the expulsion of the Jews" (p. 160), based on the analysis of the *Summa Angelica* of brother Angelo da Chivasso (referred to in Latin as Clavasio). Not only does the author explain "the relaxation of the canonical prohibition on lending at interest, under the influence of growing social pressures" of the great mercantile patriciates, toll contractors and army provisionists, but he makes the exclusion of the Jews from the financial market neither a consequence nor a prerequisite of this. And in fact at Chivasso, the group of citizens concentrated around brother Angelo (of the rich Carletti family) obtains, first the expulsion of the Jews and then inserts in the Statutes the prohibition to propose readmission. If now we pass to Jewish aspects of the research, I limit myself to the case of Jacob Landau (pp. 17-19), to whom Poliakov refers critically for not lending at high interest in the 80's of the XVth century in Pavia. This deals with an unusual situation, which is comprehensible only in terms of Landau's personal experiences. Landau, in fact, moved to Naples while the anti-Jewish controversy supported by Bernardino da Feltre raged in Pavia, to find himself a victim of the general expulsion decreed by the king of Spain. He was, however, the grandson of another fugitive, Albertino da Vicenza, who in the 30's had established himself at Pavia as a banker after having fled from the Veneto (p. 62).

The volumes of documents and essays on local history and the continuing renewed enthusiasm for research in local history (even in the Jewish sphere) over the past twenty years must render somewhat premature the author's interpretations which are heavily based on collective psychology.

RENATA SEGRE
Venice

D. PRODAN (ed.), *Urbariile Tarii Fagarasului* Vol. 1 (1601-50), Academiei Republicii Socialiste Romania, Bucarest 1970 pp. 968; *Urbariile Tarii Fagarasului* Vol. 2 (1651-1680). Academiei Republicii Socialiste Romania, Bucarest 1976, pp. 1019.

There is a wide range of sources for studying the history of serfdom and feudal relations in Transylvania, the most important being the seigneurial re-

cords, which include inventories, accounts, tithe registers and other similar data. The most complete sources are, however, the seigneurial *urbarii*. These provide extensive information about life and conditions in rural society, and on the agrarian economy in general. The very tithe *urbarii* is indeed indicative of this, being derived from one of the most important attributes of rural society — the *urbum* or *urvum*, meaning the plough, the principal instrument of economic production.

The *urbarii* not only provide extremely precious information on the day to day business of labour, feudal rights and obligations, and on the possessions of the great estates. They also constitute an irreplaceable source for a quantitative study of rural life in a period when the system of serfdom was spreading rapidly.

The publication of the *Urbariile Tarii Farasului* follows that of other seigneurial accounts which have been used to great advantage by the historian D. Prodan in his fundamental study 'Serfdom in Transylvania in the XVIIth Century.' The two volumes consist of the seigneurial registers for the county of Fagaras, and were selected by the editor because Fagaras was one of the largest feudal estates in Transylvania. It was ruled directly by its prince, and was also an independent country with its own states, as well as being one of the most populated areas of Rumania and the one in which Rumanian institutions were best preserved.

The volumes include seigneurial records, inventories, economic instructions, accounts, lists of wages, personnel, agreements etc. The first volume is comprised mainly of the seigneurial registers (published in full) and inventories (over 55 texts in all), with reference to the three lordships which in the early XVIIth century made up the lordship of Faragas — that is Faragas, Porumbacu and Comanca (the last two being the smaller). The introductory essay provides many useful suggestions and ideas for further research on the documents published. An analysis of the economic instructions given by Gabriele Bethlen and Giorg Rakoczi I (between 1623 and 1634) is followed by a detailed description of the social features of the lordship, which was made up of a number of relatively large villages, inhabited by a large population of agricultural labourers, herdsmen and artisans, each with a quite distinct economic and legal status.

The seigneurial registers list the population of the lordship, and show that this was steadily expanding. In 1632, for example, there were 2,329 heads of families, in 1637 3,678 and in 1640 4,040, distributed respectively at these dates in 43, then 52 and finally 56 villages and two towns. The records of total numbers also vary, but again reflect the growth in the population and number of settlements. The population was divided into the following social categories: the serfs, who made up the majority; the citizens and artisans; the nobles and boyars (the hereditary nobility dating from the time when Faragas had been part of Wallachia); neighbours — an institution originating from the same period; 'libertines' and infantrymen — the garrison of the fortress; the 'Greek'

merchants of Faragas, priests, beggars and cripples. The status of the subjects varied greatly, as did their social position, and the variations were very great -- running from the poor who did not even possess an animal at one end to the wealthy at the other. The registers also contain valuable information for studying the social order, and the incomes of the different groups in the three lordships.

The registers recorded feudal obligations: rents, which doubled during the period; customary and illicit 'gifts'; tithes -- be they on cereals, on pigs, sheep, or bees; labour services, following the prescriptions laid down by Gabriel Bethlen. There is also a great deal of information of seignorial rights over woodland and mountains, game, water, fishing, milling and the small demanial enterprises (water mills, the glass works in Upper Comanea and Upper Porumbacu, stone mills and powder mills, and aquavite production); on the activities of the inn-keeper, the butcher, the customs officials and fines. There is also much information on the allodial economy. The later documents also provide information on the organisation of the three lordships of Faragas, on their economic structure, their military strength, their inhabitants, on the demanial buildings and their adjoining properties (the mills, workshops, etc.) and on the organisation of agriculture.

The documents published in the first volume then offer the possibility of much further study, and they provide sufficiently detailed and extensive information to allow us to compare the development of individual aspects of the economy with broader structural changes, and to follow the adaptations and transformations that occurred. Whereas the first volume is taken up largely with the demanial accounts, the second contains inventories, further accounts, lists of military and demanial personnel, lists of wages and the prices of various consumer products. There are also documents of every kind touching on a wide range and variety of aspects of life on the demense lands and surrounding them: the inventories of the economic and military endowments of the fortress of Faragas, instructions on various matters, the demanial inventory and register of the lordship of Porumbacu (1680), the accounts of the Princess Boremisza, the wife of Prince Michele Apafi 1 and mistress of the lordship. There are also numerous documents on the serfs, the boyars, and on the schools and clergy in the lordship.

The valuable introduction to the second volume describes the essential features of the documents published. The Princess's instructions on the recruitment and duties of the dignitaries of the lordship (which frequently makes one think of the prototype of this type of source -- the *Capitulare de Villis*), and of the demanial magistrates, as well as those sent to the military commanders, show how the fortress and the estates were governed. Both those issued in 1675 and others probably contain precepts which were observed throughout the Princess's lands.

In 1652 the lordship of Faragas covered 46 settlement with 2,513 subjects,

obliged to pay feudal rents, gifts and taxes. The documents published by D. Prodan provide a wealth of information on attempts to avoid or sell (with right of redemption) servile dues, on the extent of emigration, and on the boyars (although exempt from fiscal dues they still owed military service).¹ There is also much information on the clergy, churches and the Rumanian schools, as well as references to the fortress of Faragas and its military provisions. But the greater part of the documents are devoted to aspects of the demanial economy. It would appear that the number of farming instruments inventoried were inadequate to the size of the lordship, while the number of demanial mills seems excessive.

The second volume also contains much information on the military and administrative personnel of the fortress and the lordship, on dues owed to the lord's court, and on the wages of the functionaries. The final chapter is particularly interesting in this respect, and D. Prodan shows that there was a close relationship between wages and prices, the adopting for this purpose the prices listed in the official price index, issued by Diet given that comparable data is lacking in the documents published (p. 46). Even though these prices often fluctuated even within a single season, they were accepted as customary prices and were generally respected. But this means that it would perhaps have been better to have tried to fix the relationship between wages and prices from the prices given in the account books of the period — in other words, with market prices rather than those fixed by the Diet of Transylvania.

The documents also enable us to follow the development of agricultural production on the demanial lands. In the period 1650-80 there was no great change on the allodial lands, and grain yields were about 3:1 (the average yield from individual fields, although we have only one example to draw on, was 2.7:1) which was much the same as in the earlier period. The numbers of livestock varied.

To conclude, then, the 'Demanial registers of the County of Faragas Vol. 2' provides a wealth of information on consumption (characterised by a small range of products put on the market at irregular intervals) and on the incomes of the demanial estates (feudal rents, customs, redemption payments for feudal services, fines, dues from offices such as inn-keeping etc). Analysis of the documents contained in the volume shows that the lordship of Faragas contained by and large all that was needed to satisfy its requirements. Indeed, due to the enterprise of its subjects, and the ways in which it was organised, its wealth grew considerably and with it the prestige of its lord and his influence on the society of his age, as well as his ability to protect his possessions. The publication of these documents has posed a number of problems for D. Prodan, especially those in ancient Hungarian which appear in the second volume, due to their highly complex orthography. Although this has inevitably meant that various

¹ Cf. D. PRODAN, "Boieri și vecini în Țara Făgărașului în sec. XVI-XVII", in *Anuarul Institutului de Istorie Cluj*, VI, 1963, 161-312.

cedillas and signs over or below the letters (which in fact have little importance) have had to be eliminated, a consistent approach has been followed in both volumes and the transcription of the documents is accurate and true to the originals.

From what has been said it will be clear that the documents contained in these two volumes are of truly exceptional importance, and that they shed light on new and varied aspects of rural society, paving the way to new interpretations and problems, and so must be seen as a major contribution to the history of the rural population. But unfortunately there are no translations of the texts, both for the technical reason that to have added translations would have increased the size of the two volumes beyond reason, and because an accurate translation of the texts would have raised enormous problems in establishing the correct meaning of many terms on which there is as yet no general consensus among experts.

SAMUIL GOLDENBERG
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S. P. SCHATZ, *Nigerian Capitalism*, Berkeley, University of California Press, 1977, 294 pp.

Professor Schatz is one of America's foremost scholars on Nigeria. He has spent many years in that country and has written a number of well-researched books and articles on various aspects of its economy.

His latest volume examines the Nigerian process of industrialization in a climate of intense African nationalism. The first part of the book contains a survey of Nigeria's economy from independence in 1960 through the Civil War and up to the mid-1970's. Because of her large population, natural resources, and strategic location, the oil-rich Nigeria is popularly considered the would-be-giant of Africa. Despite this image, according to Professor Schatz's figures, the annual growth rate of Gross Domestic Product, per capita, up to the outbreak of the Civil War in 1967, was a mediocre 1.4 percent; if oil production is included, it was 1.9 percent. After the end of the Civil War, from 1970 to 1973/74, the official average annual growth rate of GDP including the oil production, was 11.7 percent (p. 25), but without oil production it was only 4.6 percent (p. 26). Impressive as these growth rates are, the author notes that "the masses of the people continue to be in abject poverty and lacking in the basic necessities of life" (p. 31). Even though 70 percent of the population lives in the rural areas, the agricultural output decreased considerably during the 1970-1974 period. Massive *Landflucht* is to a great extent responsible for this decline. People flock to the cities where they expect the streets of Lagos to be virtually paved with the oil-dollars! Urban unemployment is enormous, and, according to the author, in 1975, it was 37.5 percent of the wage labor force. In 1974 inflation was running at the rate of 25 percent and, in 1975, it reached the

75 percent level (p. 36). It has been declining subsequently. Corruption, too, was widespread (p. 38). After the quadrupling of the oil-prices in the fall of 1973, Nigeria entered a boom and the era of euphoric planning (p.47). The Five-Year Plan, to be completed in 1980, made rosy projections for the country's future. In 1972, Nigeria promulgated its first Indigenization Decree which was designed to increase native ownership and management in the modern sector of the economy. The foreign investor was considered "a constant irritant" (p. 41) and the Nigerian government wanted to raise the country's self-reliance.

Part II (pp. 65-147) examines the indigenous private sector of the economy. Because of the inadequacies of the native business community, the Nigerian government had initially relied heavily upon the foreign investor to develop, that is, own and manage, the modern sector of the economy (p. 65). The author stresses repeatedly that Nigeria suffers not from the shortage of capital, but from the shortage of viable investment projects (pp. 45, 75). Nigeria has an abundance of traditional traders and small entrepreneurs but lacks businessmen who have made what the author calls the technological, organizational, and marketing leaps (pp. 80-81) which render modern industry possible. At the present time, the prevailing business milieu discourages the formation of corporations (p.88) and thus the growth of privately-owned large scale industry is nipped in the bud. Furthermore, Nigerian businessmen prefer quick returns on their investment. For this reason, they favor dealing in real estate, in import-export, or in rendering all kinds of services, but are reluctant to enter industrial production. As a result, the government has no choice but to finance, own, and manage the country's modern industries (p. 93). The chapter on the prevailing economic environment is particularly instructive for it deals with the problem of the material, human, and institutional infrastructure. It documents in great detail the existing negative externalities which cause great hardship on the private sector and raise the costs of production. For instance, start-up operations of industrial plants require more working capital than in the USA (p. 102), the marketing facilities are inadequate; and the available indigenous managerial personnel is "deficient in supply and inadequate in quality" (p. 104).

Part III, (pp. 151-253) deals with the various government-assisted programs to the Nigerian businessmen to further industrialization. These are especially revealing chapters, which no specialist concerned with economic development can afford to miss. They deal, for instance, with the methods of the so-called "demand channeling", designed to favor the indigenous businessmen in awarding government building contracts; this favoritism turned to be very costly (p. 183). The Approved Manufacturers Scheme, the Yaba Industrial Estate, and the Loans Programs to enable the indigenous firms to raise working capital which they could not obtain through regular commercial channels (p. 227) provide a wealth of institutional facts on the all-too-often neglected aspects of economic development. In the author's view, the indigenous business com-

munity did poorly and that was the reason why the government turned to increased reliance on foreign-owned enterprise (p. 237). This state of affairs continued after the Civil War as well, although the recently promulgated indigenization measures are designed to alleviate the situation considerably.

The centerpiece of the book is chapter Seven which examines the divergences between profitability and net social benefit. It is the author's view that profitability is not "an index of the social utility of directly productive investments" (p. 130). In contemporary Nigeria, there is paucity of directly productive ventures and public investment could make a considerable contribution to economic development. In fact, Professor Schatz argues that social benefit divergences generate many intangible effects on the society about which the economists have little to say, but which are exceedingly important (p. 145). The author offers a plan of his own in the form of "pragmatic developmentism" (p. 281), and notes that such a policy takes full account of the divergences between the net social utility and profitability of economic activity (p. 281). The principal vehicle for this approach is the use of public enterprise (p. 282). He equates pragmatic developmentism with "thoroughgoing socialism" (p. 283), a doctrine to which the author himself subscribes (p. 286). Specifically, he suggests five distinct policies for Nigeria's development: first, the production should not be governed by pecuniary profitability but by the requirements of full utilization of resources. Second, mass mobilization of people should be instituted to overcome the prevailing underutilization of men, machines, land, and industrial facilities (p. 284). Third, the economy should be restructured to become "inward-looking", to reduce any reliance upon the capitalist economy (p. 285), and to redirect trade towards socialist countries. Fourth, the government should own the means of production and invest all surpluses according to the plan (p. 286). Fifth, the entire society should be made socialist by transforming it into participatory democracy (p. 286). Professor Schatz also believes that "state coercion does not seem promising for Nigeria" (p. 279). His goal, as noted, is "thoroughgoing socialism" (p. 279), which is likely to be preceded by a transitional period of indefinite duration, during which capitalist-type institutions will be gradually phased out.

Professor Schatz's characteristic mode of expression is the essay, and it reflects his long-term preoccupation with Nigeria's economic problems. He asks direct questions about the nature of the country's industrialization process and, rather than hiding in a fog of abstract mathematics and garbled English, he gives straightforward answers. The strength of this volume lies in the wealth of institutional data, something that contemporary mathematically-oriented model-builders usually overlook or do not bother to collect. He is thoroughly familiar with the existing legislation affecting economic development, he is aware of the almost incredible shortage of competent administrators and managers, he lists the deficiencies of the indigenous business community, and he airs his disappointment with the poor economic performance of the economy.

His analysis of economic problems is straightforward, thorough, convincing, and is supported by hard-to-get material.

However, Professor Schatz's normative views, as expressed on his blueprint for action (i. e. his suggestions for the implementation of "thoroughgoing socialism") raises a number of questions. For one, he does not believe in massive compulsion (p. 279), and suggests that mass mobilization should take a voluntary form. But, in this reviewer's opinion, the basic deficiency of Nigeria is precisely the utter lack of social discipline, without which rapid industrialization is well-nigh impossible. Rapid industrialization also presupposes the ability of a society to generate, on a sustained basis, investible resources, which can be poured into the basic industries and the necessary infrastructure. It also presupposes the existence of administrative capacity to direct the massive effort to bring about the needed *Industrial Revolution*. Accelerated industrial revolution is by no means an impossible phenomenon. It was carried out in communist Russia, where Stalin's terror was used as an instrument of compressing the process into four decades. If Nigeria is ever to reach Dr. Schatz's ideal of "thoroughgoing socialism" it may well need a period of massive compulsion (to which Dr. Schatz objects) to put the country on the launch-pad of industrialization. It will also be a period of maximum prices, wages, rents, coupled with rationing of consumer goods, labor conscription, floorspace controls, quantitative-output planning, and an efficient executioner for economic crimes as well. Is Nigeria prepared for such an "investment war" and the social discipline that comes along? Most likely not. Hence, Professor Schatz's suggestion of voluntary socialism does not appear adequate for the task on hand.

NICHOLAS BALABKINS
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- I. SEMMINGAEN, *Norway to American: A History of Migration*, translated from the Norwegian by Einar Haigen. Minneapolis: University of Minnesota Press, 1978.

This volume is a condensation and translation of a major study of the migration of Norwegians to the United States. The period covered is from about 1825 to the early years of the XXth century. The research deals with a process of adaptation, acculturation and assimilation to the New World by the new migrants. The book describes in a clear, picturesque manner the trials and tribulations of those who chose to make the move. The author of the books doesn't claim to analyze the underlying process that impelled Norwegians to leave Norway, or the processes that occurred when they arrived. At the same time, the translation is clear, well written and interesting. The study provides a good orientation and a coherent overview of the migration phenomenon and contains interesting case histories and anecdotes.

Of particular interest to this reviewer is the information contained in the book concerning the strong pacifist and liberal political orientation in significant segments of Norwegian communities in the United States and in contemporary Norway. This insight emerges from the description of the role played by the Quakers in the initiation and support of the movement to migrate, and the financial and social supports given to the migrants when they arrived here.

Exposure to the Quakers began in 1807. During a war with England in that year, many of the crews of ships that were captured were interned there. The English who helped to comfort and care for these crews were Quakers. After the war, the crews returned to Norway. Some of them organized Quaker societies in Christiania (Oslo) and Stavonga. These Quaker groups came into conflict with Norwegian law and the state church, but, despite the conflict, Quaker groups maintained close contacts with England, and frequently brought Quaker visitors from England.

This period (1807-1830) of Quaker penetration and influence overlapped with the emergence and growth of the Haugean movement. This was a national religious movement that began in Stavonga and spread to the surrounding area. The movement was indigenous to Norway, and was not subject to persecution during the 1830's when migration developed. The beliefs of the Haugean movement were similar to those of the Quakers and encouraged and supported the mushrooming interest in migration. The development of the Haugean movement is vividly described giving the names of the prime movers — with some descriptive background information about them, and their importance and contributions.

Although the work is written in a vivid, clear, and informative style it is weak from an academic or scholarly point of view. It is highly descriptive and filled with case histories, but only touches the surface of a very significant phenomenon. There are many unanswered questions here concerning the underlying processes and dynamics of migration. It appears that events just developed; families just decided to move; Quakers and Haugeans just encouraged and supported the migration, etc. The whys' and wherefores' are vague.

This book is, in part, concerned with the processes of "adaptation, acculturation, and assimilation" involving Norwegian migrants. However, it aggregates the experiences of many families, particularly the experiences of the leaders of the movement. There is little discussion of the socio-political climate in the new world that acted to interest and absorb Norwegian migrants during the early years of the migration; and no explanation as to why the socio-political climate continued to attract such large numbers over the next hundred years. There is also limited information concerning the socio-political climate in Norway, during the early years, that might have prompted Norwegian families to uproot themselves and move such long distances to start new lives. Was it political or religious harassment, economic pressures, or other factors? The reader is left to guess at the answers from the material presented.

Why did the Quakers urge and support migration in the first place? Why did the Haugen movement do so? Did the two movements have contact with each other during those early years? Is it purely a coincidence that they had similar ideas?

When these questions are answered, it will provide us with a better understanding of the "pushes" and "pulls" that constitute the conflicts potential migrants must resolve, and once resolved, how they deal with their decisions.

ROBERT J. KLEINER
Temple University

P. VILAR, *A History of Gold and Money, 1450-1920* (London: NLB. Atlantic Highlands: Humanities Press. 1976. Pp. 360).

This is an English translation of a work which first appeared in 1969 under the title *Oro y Moneda en la Historia*. It is a broad survey which, some introductory remarks on the ancient and medieval background apart, essentially begins about 1450 — 'when what is called the "price revolution" of the XVIth century was already in preparation' — and then threads its way through to the twentieth century. In the process a chain of developments is revealed, linking together the major areas and periods of precious metal production in the modern world: gold from West, North-West and South-East Africa in the late fifteenth and sixteenth centuries; gold and silver from the West Indies and South America in the sixteenth and seventeenth centuries; gold and silver from Brazil and Mexico in the eighteenth century; and gold from California, South Africa and Eastern Australia in the nineteenth century.

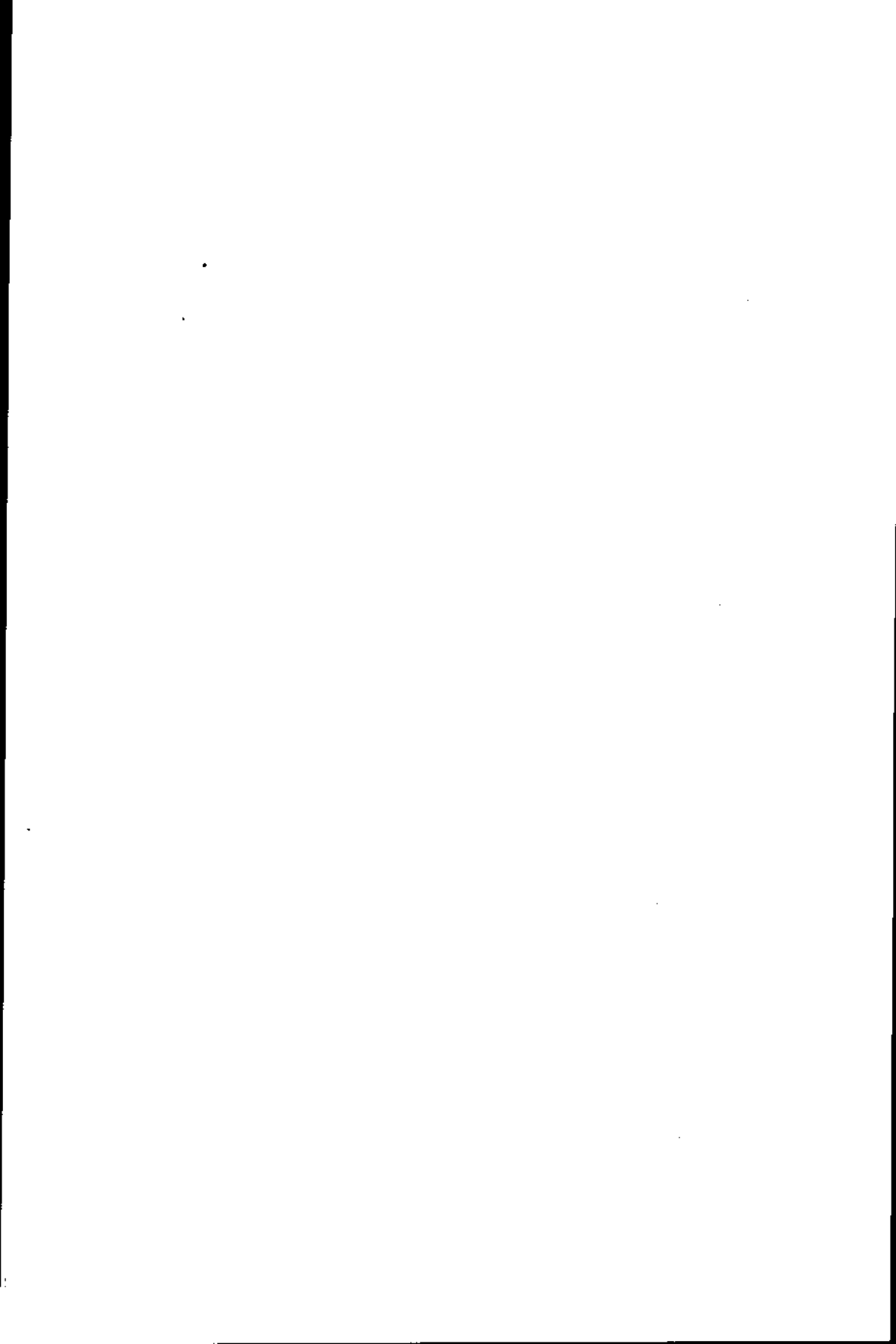
Running parallel with this primary theme of discovery and exploitation is a second, equally impressive; namely, the impact which the new supplies of precious metals had on the economies not only of the lands in which they were produced but also of the leading European countries. The central place which Spain and Portugal quite rightly occupy at the beginning of the story is given added definition by a discussion of developments in Italy and France and then, later on, embraces both Holland and England. An essential feature of the argument, which could so easily have led to confusing fragmentation on the one hand, or to over-simplified views of the importance of money on the other, is that of 'the conjuncture'; "the totality of all the conditions" in which a problem or an event is located". The three critical periods given this total overview are 1450 to 1530, the eighteenth century, and the nineteenth century. The coverage in each case is not identical but the effect is the same: the reader is compelled to consider the main theme of the book against a background of developments in such things as population, agriculture and trade.

Overall, the book bears the rather staccato imprint of the university lecture course from which it originated and, though necessarily schematic at all stages,

handles the later period, especially the nineteenth century, less convincingly than the earlier. Moreover, there are gaps; most noticeably any discussion of the credit pyramid which was built on the basis of each fresh influx of precious metals and which necessarily, therefore, had important 'conjunctural' implications. Nevertheless, this is a most stimulating survey which not only draws together the fruits of researches conducted by a host of scholars from many different countries over a very long period of time but also, as in the excellent chapter on Potosi, brings us face to face with contemporary documentation.

Professor Vilar writes with refreshing clarity on a difficult subject and constantly seeks to answer important questions: why did men seek so actively for gold in the later fifteenth century?; why was the impact of precious metal production on the European economy seemingly less in the seventeenth than in the sixteenth century, even though seventeenth-century production was probably the larger?; why were the Mexican mines profitable when apparently wages were six times as high, costs twenty times greater and four to five times as many workers were required as in Europe?; and so on. This is a book which all who are interested in Professor Vilar's broad theme will read with profit and, I fancy, subsequently return to for further guidance.

CHRISTOPHER E. CHALLIS
University of Leeds



ANNOUNCEMENTS

EIGHT INTERNATIONAL CONGRESS OF ECONOMIC HISTORY

The following is a list of the topics and organizers of the "C" sessions for the Budapest Congress (August 16-22, 1982). Persons wishing to participate in a session should correspond with the designated organizer.

1. Silk in economic history: a comparative perspective (Murat CIZAKCA, Bogaziçi University, Economics Department, P.K. 2 Bebek, Istanbul, Turkey)
2. Landwirtschaft und Ernährung im zweiten Weltkrieg (Bernd MARTIN, Historisches Seminar, 78 Freiburg i Br., Werthmannplatz, German Federal Republic, and Alan MILWARD, Manchester)
3. Le développement de la technique agricole au XIX^e et au XX^e siècle (Harald WINKEL, Universität Hohenheim, Postfach 106, 7000 Stuttgart, German Federal Republic)
4. The comparative study of the development of social welfare (Pat TRANE, Goldsmith's College, London S.E.14)
5. Timber and timber industries after 1850 (Jorma AHVENAINEN, University of Jyväskylä, 40100 Jyväskylä 10, Finland)
6. La famille et la vie économique (Andrzej WYCZANSKI, Institut d'Histoire, Rynek St. Miasta 29/31, Warsaw, Poland)
7. Frappes et circulation monétaire et taux de change (Eddy VAN CAUWENBERGHE, History Dept., Katholieke Universiteit, Leuven, Belgium, and Martin KOERNER, Staatsarchiv, Bahnhofstrasse 18, 6002 Lucerne, Switzerland)
8. Geschichte der Korporierten Gemeinschaften im Lichte der nord-europäischen Städtegeschichte (Klaus FRIEDLAND, Schleswig-Holsteinische Landesbibliothek, 2300 Kiel 1, German Federal Republic)

9. Ideology and reality in travellers' accounts (Antoni MACZAK, Radna 2/4 m 21, 00341 Warsaw, Poland)
10. Production, commerce, et consommation du vin (I. OROSZ, Budapest, Hungary)
11. Finances publiques et croissance économique au XIX^e siècle (M. KATUS, Budapest)
12. The emergence of the integrated Baltic-European-North American maritime system from c. 1780 to c. 1870 (Jake KNOPPERS, 3458 Holton Avenue, Montreal H3Y 2G5, Canada, and Artur ATTMAN, Göteborg, Sweden)
13. Histoire économique des loisirs (Wray VAMPLEW, Flinders University of South Australia, Bedford Park, S.A. 5042)
14. Bergbau und Edelmetalle in Mitteleuropa (Stefan KAZIMIR, Historický Ústav, Klemensova ul. c. 19, 88416 Bratislava, Czechoslovakia)
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