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## REVIEWS OF BOOKS

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PH. BENEDICT, *Rouen during the Wars of Religion*, Cambridge, London, New York: Cambridge University Press, 1981. 207 pp.

J.G. CLARK, *La Rochelle and the Atlantic Economy during the Eighteenth Century*. Baltimore: Johns Hopkins University Press, 1981. 286 pp.

Rouen and La Rochelle are part of one of the great unknowns of early modern European economic history: Atlantic France. This area was a major trading and manufacturing centre, yet we know virtually nothing about it, except for isolated studies of its four major ports — Rouen, Nantes, La Rochelle, and Bordeaux. Most of these studies concentrate on the eighteenth century, particularly on the colonial trade. While Clark follows in this beaten path, Benedict strikes out on new ground in his study of Rouen.

La Rochelle, the smallest of the four cities, was also the only one without a developed interior network of communications and a prosperous hinterland; it was competitive only for luxury products and for local exports (wine and its derivatives and salt). It had no major manufactures, and also had no sovereign court; again, this set it apart from its three sister cities. Its sixteenth-century prosperity, documented by Trocme and Delafosse, rested on wine, brandy and salt exports to northern Europe and France, and imports of expensive textiles, subsequently sent overland via Limoges.

La Rochelle's eighteenth-century prosperity countered this isolation by making the city "a point of long distance transshipment of goods." Clark is quite strong here, demonstrating the structural weakness of the city's position with respect to its main competitors. He presents an effective general descrip-

tion of the West Indian and slave trades, together with new insights into the often under-estimated East Indian routes. Here we see, as at Nantes, the dominance of certain major outfitting (*armateur*) families, their marriage within the merchant group, their network of investments (extensive cross-investment among family and friends), the sophistication of commercial techniques, and the relative cost of outfitting ships for the respective trades. The wild fluctuation in slave-trade profits remind one of the difficulties of Andre Ruiz in the sixteenth-century grain trade: they are a fitting reminder of the constant uncertainties of early modern commerce.

On another front, the book is less successful. Clark's explanation of the lack of Rochelais social mobility is not very satisfying. Robert Forster's recent book on the Depont family of La Rochelle shows precisely a case of social mobility through office, religious conversion, and acceptance into the nobility. Table 3.2 of Clark's work is particularly disturbing in this respect: what is one to make of the Bonfils family, with 14 outfittings per decade from 1740-69, but none thereafter? Many other families show similarly rapid rises and falls.

The Depont family sent out 17 ships in the 1720's, 11 in the 1730's, three in the 1740's, and none thereafter. We know their capital bought land and offices, that they became noble. Did other Rochelais families whose outfittings declined or stopped — the Bonfils, Pascauds, Labbés — do the same? Merchants at La Rochelle did not have access to a local sovereign court: did this reduce propensity to go into offices and the nobility? Was there a movement from outfitter to investor (*intéresse*) among many families? From Clark's evidence, this seems clear for the Belin family. Did this change their social standing, their social ambitions? Did the death of individual outfitters, Labbé for example, lead to dissolution of firms because of social change in family generations? Clark does not deal effectively with these questions.

The conclusion is very weak in its argument for the French state as a third economy in the country, one whose effects were entirely negative. Clark makes no real case for this, or for the contention that government policy undermined La Rochelle's economy; indeed, his analysis shows the structural underpinnings of the decline. The absence of a bibliography, especially a note on archival sources, also leaves the book seriously incomplete at the end.

Benedict's work, by way of contrast, contains a very informative note on archives and a superb bibliography. They are a fitting apparatus for what is, by-and-large, a well-crafted narrative. This is narrative in its new form, based not merely on political events and major actors, but on a detailed analysis of local archives and of the relevant factors of daily life.

Rouen was a true provincial metropolis, a city of 75,000 people. Benedict gives us an immediate feel for the city by opening with a description of the royal entry of 1550; it is a lively and delightful way in which to introduce the variety that was Rouen. The mere enumeration of the various trades gives us a picture of the city that brings its teeming quality to life.

The opening description is followed by a narrative of Rouen from 1559-94, with a concluding chapter relating the Rouennais experience to that of France as a whole. This is arguably the best local narrative extant for any French town in the second half of the sixteenth century. The generalizations in the conclusion are well tempered, and remarkably free of local arrogance: Benedict is quite willing to see the variety of local experience during the Wars of Religion, and to accept the fact that Drouot's answers for Dijon differ from his answers for Rouen.

When Benedict steps aside from his narrative, however, he is sometimes less successful. The section on sixteenth-century taxation is very weak: Benedict knows better than to believe that the proposed levies listed in Table 11 have anything to do with real taxes. He also does not take into account the currency devaluation/inflation of 1575-80, which partly explains both the sudden jump in local rents (in Appendix 4) and some of the rise in taxation. Nonetheless, he is right in his assumption that taxes rose sharply under Henry III.

There are other, isolated contradictions. On page 58, he claims that local revolts (pre-1572) showed no sign of socio-economic antagonism. In August 1561, however, the Protestant crowd shouts, "Learn how to work ... you've eaten too long without doing anything," at the priests (p. 56). I would classify this as social antagonism, and one suspects there were other social overtones (local ones) in some of the flare-ups. The main point, that socio-economic distinctions do not explain the Catholic-Protestant split at Rouen, rests on more solid ground.

There are certain elements one would like expanded, certain points outside the main narrative that are of questionable validity. Given that Benedict stresses the fact that Rouen is the greatest port of France in this period, and its largest linen producer, one would like a bit more information in the narrative about such matters (he promises an article here on them). Similarly, his emphasis on local conditions leads one to wish for a greater treatment of Norman clientage relationships and their effect on religious preferences (a point he acknowledges).

The isolated comments, so untypical in this judicious narrative, that endogamy of 57-64 percent in Catholic parishes is "a remarkably high level," or that the increasing reliance on *rentes* is "a sign of the growing weakness of the crown, which was now forced to bargain away future revenues rather than extract money through direct taxes," betray a serious misunderstanding of both issues. *Rentes* were often forced loans, frequently bought by officers, never fully paid. They provided ready *cash*, a service for which direct taxes were notoriously ill-suited. Benedict also argues that taxes were rising much too fast; one cannot have it in both ways. As for endogamy rates of 60 percent or so, they seem relatively normal for the period; the higher, pre-1560 rates, on the other hand, do appear "remarkably high." If Benedict wished seriously to raise this issue, he might better have concentrated on such a dramatic change, rather than on a misleading blanket statement about the whole period.

To say all this is to concentrate too heavily on the negative, for Benedict's book is an outstanding one. His clear-cut division of the Wars of Religion into three phases — religious conflict up to 1572, socio-political conflict from 1572 to 1585 or even 1598, and a new, intensely religious phase from 1585-93 — is an important contribution to our understanding of the period. Similarly, his argument that the seriousness of the Protestant challenge of the 1560's, renewed by the prospect of Henry IV's ascension of the throne as a Protestant, explains the religiosity of 1559-72 and 1585-93, makes a great deal of sense. All future accounts of the Wars of Religion will have to use extensively this well-written, thoroughly documented, and insightful examination of a provincial metropolis and a society in those times of troubles.

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M.R. GODLEY, *The Mandarin-Capitalists from Nanyang: Overseas Chinese Enterprise in the modernization of China, 1893-1911*, Cambridge University Press. 1981 pp. 222.

Chinese emigration to Southeast Asia which the Chinese call Nanyang (south seas) had begun long before the area was colonized by the Europeans. But it was in the eighteenth and nineteenth centuries when the British and Dutch recruited *en masse* Chinese labour to exploit their newly established empires that the largest influx was recorded.

In Penang, for example, Francis Light recruited several thousand Chinese two years after he took the island in an attempt to turn it into a giant spice plantation. The venture failed; but the Chinese remained — more than 10,000 according to one Chinese report — and by 1796 had established a monopoly of the pepper trade. The same story was repeated in Singapore. Made a free port after its founding by Sir Thomas Raffles, Singapore attracted several thousand Chinese merchants and artisans from neighbouring Malacca within a year. By mid-century, Tan Tock Seng (Chen Tu-sheang) and his son Chen Chin Chung had come to monopolize the rice trade. In the 1880's, the son also became the acknowledged Chinese leader in northern Malaya and southern Siam and was decorated by the government of Siam apparently for his service as that country's consul-general. His reputation and status were unrivalled among the Malaya Chinese.

The majority of the Nanyang Chinese emigrated from Fukien province in south China while the others came from the Hakka districts of Kwangtung with a sprinkling from Hainan Island and around Canton. Their success as entrepreneurs in Southeast Asia is proverbial. Driven by poverty from their

homeland, they migrated to the multi-culture milieu of Southeast Asia which made available to them a wide range of economic activities the Europeans disdained and for which the indigenous population was unprepared. By sheer hard work many amassed sizable fortunes. Denied social acceptance and political participation, they sought solace in the preservation of their own culture and tradition and a strong identification with any government in their homeland. They were generous in contributions to causes "back home" which they believed would make China strong, for a strong China would enhance their status abroad. Their remittances, averaging more than 150 million dollars a year from 1902 to 1913 (more than half probably from Southeast Asia), were an immeasurable contribution to social and economic change in modern China. This alone should be a fruitful field for scholarly investigation.

The volume under review by Michael R. Godley is a praiseworthy but by no means exhaustive attempt to relate the entrepreneurial careers of a very small group of Chinese capitalists in Southeast Asia to the spasmodic efforts of the late Ching government to introduce modern commerce and industry into China. The most important figure, Chang Pi-shih, on whose career in fact this study is primarily based, had emigrated from Kwangtung, worked as provisioner for the Dutch in the Netherlands East Indies, made a fortune in Sumatran coastal shipping, and built a net-work of banks, mining enterprises, and plantations on both sides of the Malacca Straits. Subsequently he extended his empire to China by investing in wine-making, textiles, brick factories, glass works, and later spear-headed a drive to build railroads in south China.

Chang must have been a good promoter of his own schemes, for he attracted the attention of some of the leading reformers in China and was even granted an audience by the Empress Dowager to whom he submitted a twelve-point proposal for the modernization of China (Ch. 5). Some of his ideas were progressive enough, including his criticisms of the conservative gentry, bureaucratic corruption, and foreign encroachment. But his plan to recruit overseas Chinese merchants and give them the same rights as were given other foreigners would make *them* imperialists just like the foreigners. The author does not mention the subsequent history of Ching's business ventures in China. Even if they had succeeded, they would have contributed only to China's vertical growth like other foreign-owned extractive industries or import substitutes and not to the country's real development.

The author rightly points out that Chang and his Nanyang mandarin capitalists were transitional figures who operated within the traditional system but with financial power and managerial skills.

The book which contains a useful bibliography, is informative and well-written. However, insofar as it is basically on the career of one man, Chang Pi-shih, the title is somewhat misleading.

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P. KRIEDTE, H. MEDICK, J. SCHLUMBOHM, *Industrialization before Industrialization: Rural Industry in the Genesis of Capitalism*, transl. B. Schempp with contributions from H. Kisch and F.F. Mendels. Cambridge University Press, Cambridge, and Editions de la Maison des Sciences de l'Homme, Paris, 1981. pp. ii, 335.

In this book (originally published in German in 1977) we have the systematic attempt by three Marxist scholars to formulate an analytical framework for the study of proto-industrialization. The first essay, by Peter Kriedte, addresses the broad question of the origins of proto-industrialization and does so by analyzing the medieval division of labour between town and country and its gradual replacement, in some regions of Europe, with handicraft industries manned by peasants but producing for the market. The emergence of such large concentrations of rural industry is then accounted for by the presence of seasonal unemployment in agriculture, population pressure, and the loosening of seigneurial controls as well as by bottlenecks in the urban labour supply, the pull of foreign demand, and the stifling influence of urban guilds. In a subsequent essay Kriedte discusses the transition from proto-industrialization to industrialization — a transition that took place in the most straightforward manner in England, while on the Continent it was slower and uneven, with some areas, notably in eastern Europe, failing to make that transition and indeed experiencing a process of outright de-industrialization. The reasons behind these divergent developments Kriedte identifies in the different social and institutional environment: in his view, XVIIIth-century England, having more completely swept aside feudal controls over the labour force, restrictions over the free disposition of land and capital, and guild regulations hindering labour mobility, was in a position where it could respond more adequately to the built-in inadequacies of proto-industrialization, and notably a backward-sloping curve of labour supply typical of subsistence peasantry; a lack of control over workmanship; the increasing distance between merchant-entrepreneurs and peasant craftsmen; and mounting difficulties in gaining access to sources of hydraulic power. On the Continent (and the more so as the institutional framework bore stronger marks of the Old Regime) the transition could take place, if did take place at all, at a much slower pace.

The second contributor to this volume, Hans Medick, has focused on the role of the family as the unit of production in the proto-industrial phase of the economy. As he sees it, the peasant family was drawn into cottage industry under the pressure of demographic expansion as the peasant holdings no longer could provide sufficient food for a rising number of mouths; turning to handicraft industry thus became a survival strategy for the peasant family, and, the latter, being only partly dependent on wages for their living, could afford to work at rates of pay that were below the minimum requirements of subsistence. This condition was often compounded by a growing burden of debt as well as by an accelerating rate of demographic growth as peasant craftsmen tended

to marry younger and to have larger families. The outcome was, according to Medick, peasant immiseration. The other side of the coin was, of course, that such low wages enabled the employer to realize higher profits. Rural industry, Medick concludes, thus provided a dominant impulse in the genesis of modern capitalism. In a second and especially lucid essay the same author focuses on demography and, in particular, on the demographic "overreaction" of the peasantry when confronted with expanding employment opportunities. The remaining two essays are the work of Jurgen Schlumbohm. He departs somehow from the approach adopted by Kriedte and Medick in that he rejects the notion of one proto-industrial system and identifies different types or phases of proto-industrialization, emphasizing, however, that these need not unfold everywhere in a neat time sequence. Those phases include: rural handicraft industries the output of which is sold directly by petty producers to consumers: the *Kaufsystem* in which producers sell their wares to a merchant; the putting-out system under which the merchant provides raw materials (and occasionally even tools) to rural artisans and pays them for their labour; and, lastly, the centralized workshop where the worker (much as in the modern factory) is turned into a wage-earner pure and simple. In this typology, then, the crucial variable is the increasing role played by the capitalist in the production process. In his second essay Schlumbohm discusses the political and legal framework of proto-industrialization and argues that the expansion of the market economy which accompanied proto-industrialization benefited from, but did not cause, the emergence of the modern state insofar as the latter freed land, labour, and capital from feudal and guild restrictions.

This brief summary cannot possibly do justice to these six essays with their wealth of information and their penetrating analysis. Suffice to say that we have here an important and highly instructive book. Its value to students of early modern economic history is enhanced by 129 pages of footnotes that represent an unparalleled bibliographical repertory on the early history of industrialization as well as by the reproduction of two well known and admirable articles, one by Francklin Mendels on XVIIth-century Flanders (1975) and the other by Herbert Kisch contrasting proto-industrialization in Silesia and the Rhineland (1959) to which a provocative "postscriptum" by the same author is appended.

For all its unquestionable interest, however, *Industrialization before Industrialization* contains a number of statements that fit the authors' theoretical assumptions better than the historical record. At one point, for instance, Kriedte states that in recent times, as "the terms of trade between agrarian and industrial products have — to all appearances finally — shifted in favor of the latter (...), the countryside has come under the economic dictate of the city" (p. 160). Quite aside from the fact that it is far from proven that terms of trade have an irreversible tendency to turn against agriculture, the possibility should have been envisaged that higher productivity in agriculture may well offset declining

farm prices. Equally debatable is Medick's assertion that "the family economy of rural handicraft producers allowed the trader or putting-out capitalist to realize a specific 'differential profit'" (p. 50) as rural producers "offered their labor below cost" (p. 51). This may have been true at an early stage when rural industry competed with high-cost urban crafts, but ceased to be so as soon as (and certainly by the XVIIth century) rural industries came to compete against other rural industries. I have trouble, too, with some of Schlumbohm's conclusions about the impact of proto-industrialization on the peasantry. It is his contention that each phase, insofar as it represented a further inroad of capital into the production process, undermined the rural craftsman's "independence", made him "subordinate and weak" (p. 102), deprived him of part of his income (p. 104), and reduced his chances "of benefiting directly from increases in productivity" (p. 116). Underlying this is the dubious assumption that any gain accruing to the capitalist could only be at the workers' expense — an assumption Schlumbohm himself undermines when he writes that dependence on a merchant "lessened their (the rural craftsmen's) marketing risks" (p. 104) and one that would have been further damaged had he taken into account Kisch's convincing argument (p. 186) that it was precisely the failure to advance from the *Kaufsystem* to the putting-out system that spelled disaster for the rural weavers of Bohemia. Lastly, I am disturbed by Kriedte's statement that in the course of proto-industrialization, "there emerged an indirect transfer of values, because the compensation of the labor contained in the commodities that were exchanged was much lower at the periphery (i.e. eastern Europe and Latin America) than at the core (i.e. western Europe), and the difference, insofar as it was not offset by lower productivity, was transferred to the core" (p. 34). Whatever the merits of the "unequal exchange theory" implicit here, the evidence presented in this book does little to support it, for no attempt is made to compare levels of pay and of productivity in Europe and outside Europe. Unless this is done, such a sweeping conclusion must be viewed as a mere hypothesis awaiting empirical verification.

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- M. MITTERAUER—R. SIEDER, *The European Family: Patriarchy to Partnership from the Middle Ages to the Present*, Translated by Karla Oosterveen and Manfred Hörzinger. Chicago: University of Chicago Press, 1982. Xii plus 235 pp.

In this useful little survey Michael Mitterauer, who is Professor of Social and Economic History at the University of Vienna, and Reinhard Sieder, a research associate at the University's Institute for Economic and Social History,

combine data from their own research with their understanding of the enormous secondary literature on the history of the family. The book first appeared in German in 1977 as a text for beginning students, and we are happy to have it now in English translation with a somewhat windy Foreword from Peter Laslett. That Laslett should present it to an English-speaking world is quite appropriate, for not only have there been institutional exchanges between Vienna and his Cambridge Group for the History of Population and Social Structure, but Mitterauer and Sieder adopt a positively Laslettian schema for their analysis, emphasizing household structure and composition.

The book's general argument is that (a) previous sociological accounts of the family's transition from big to small have misunderstood most aspects of the story; and (b) that family life does indeed undergo major shifts in function, but we must be careful to specify what functions were really lost, and what retained. Fitting these arguments onto a bed of fact is a bit tricky, given the diversity in past family life that two decades of research on the subject has now turned up. But in fact Mitterauer and Sieder do a first-rate job of discerning basic trends from bizarre anomalies which some eager-beaver scholar has managed to uncover in Overboodelsbach, announcing it to all the world as "typical." Chapters discuss changes over the centuries in the family life cycle, functions gained (such as emotivity) and lost (such as protection), the life of the young, and the dramatic transformation in the care of the elderly that has gone on over four hundred years.

Along the way we pause at some fairly disputatious watering holes, such as the issue of how loving and sexually gratifying the modern family is as opposed to the traditional. The authors come down, interestingly, on the side that sees the development of love in marriage as part of a new "bourgeois model of family life." When the reader is confronted with some of the colourful Austrian evidence on these and other matters, it is hard not to agree.

The book is ideal as an introductory text to the history of family life in Europe, with special emphasis on Central Europe. For professional scholars its signal virtue is its extensive bibliography, updated since the 1977 German edition. Anyone wishing to know the basic literature on questions like the life of the aged in traditional Europe need only open up *The European Family*. Ditto for household structure, or the family and the guild system, or numerous other questions on which historians are obliged, from time to time, to give classroom lectures.

For the professional scholar the book's great disadvantage is its lack of references. Nothing is footnoted, none of the evidence documented, save a couple of demographic tables in a discussion of changes in the age-distribution of the population. Including the references, and the many pieces of evidence that one would have been tempted to drag in alongside them, would, I suppose, have ruptured the framework the authors initially intended, a short, accessible, guide for students. We might hope that these two fine scholars will make good

the omission when they present their own extensive findings about the history of the family in Austria.

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M. OLSON, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities*, London: Yale University Press, 1982, xi, pp. 273.

Like others before him, Marcus Olson reiterates the point that very little is really understood regarding the reasons for the rise and fall of nations and peoples. Why was it that Western Europe rose from the relatively primitive dark ages into a civilization that by the nineteenth century dominated almost the entire world? How do often historically insignificant and peripheral nations rise to greatness? While historical accounts abound, compelling explanations for the rise of the West, the growth of Holland, Britain, and France during the commercial revolution, the industrial revolution in Britain, or the emergence of rapidly growing economies in America, Germany, and Japan during the nineteenth century, remain to be made.

Rather than repackaging a variety of textbook explanations for these occurrences, Olson has in this very ambitious book generated a single theoretical framework within which the rise and decline of nations can be reexamined. Its purpose, however, is not to provide a monocausal explanation for economic and social change. The goal for this work is to offer clues overlooked by others who have studied this historical terrain.

Olson's general argument is that special interest groups emerge over time within a nation and have a profound influence on economic growth and performance. Stable societies with unchanged boundaries and undisturbed by invasion or revolution tend to accumulate rent-seeking collusive organizations. These "distributional coalitions" direct their efforts toward the redistribution of income and wealth in a nation rather than the production of additional output. On balance these special interest organizations reduce efficiency and aggregate income in the society and make political life more divisive. They reduce the society's capacity to adopt new technologies and reallocate sources in response to changing conditions. The accumulation of distributional coalitions over time leads to an increase in the complexity of economic regulation as well as the role of government.

If distributional coalitions can bring about economic decline why have some nations grown so rapidly? The reason according to Olson is that upheavals of one kind or another destroyed the existing coalitions and therefore the barriers to economic growth and progress. What followed were the so called "economic miracles." Two of the major sources of upheaval in Western economies have been war and revolution. This observation, Olson declares, points out an inherent conflict between the economic returns of peace and stability and the losses

which come from the accumulating distributional coalitions that flourish only in stable environments. With this conflict in mind, Olson suggests we appreciate anew Thomas Jefferson's assertion that a little rebellion now and then is a good thing.

In an incisive chapter on the historical role of jurisdictional integration and foreign trade, Olson supplements the traditional comparative-statics theory of international trade by integrating it with his theory of economic change over time. He uses this more robust methodology to assess the historical role of institutional arrangements governing trade from the creation of nation states in the West to the formation of the European Economic Community. In sum, his argument is that while free trade promotes economic growth through gains from specialization, more importantly free trade and factor mobility evade and undercut distributional coalitions.

There are several areas of investigation which Olson has avoided in this work. First of all he has not applied his model of economic change to the Soviet Union or any of the Eastern Bloc countries. Nor has Olson examined the persistently unstable and yet underdeveloped countries of Latin America and Africa. Questions raised regarding the applicability of his model to the explanation of their economic performances will, according to the author, be addressed in subsequent publications. Finally, Olson does not assess the role of the state as a non-passive player in the economic development of a society. Fortunately, a theory of the state is emerging as can be seen in the recent publication of Auster and Silver's *The State as a Firm, Economic Forces in Political Development* and North's *Structure and Change in Economic History*.

The incompleteness of Olson's work should not, however, be a detraction from what he has accomplished. Indeed Olson has made a major contribution toward our understanding of what accounts for economic and social change. He has shown the consequences of special interest groups upon the development of the Western world.

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TH. C. OWEN, *Capitalism and Politics in Russia. A Social History of the Moscow Merchants, 1855-1905* (Cambridge University Press, 1981).

American historians attempting to write the history of so un-western a phenomenon as the Moscow merchants face an immense challenge. Trained to reconstruct history from written documents they encounter not only great scarcity of evidence but also the virtual impenetrability of a group of people still closer to oral than to literate culture; they deal with a subject for which their western vocabulary of socio-political analysis is hardly suited. Their Soviet counterparts, incidentally, are no better off: Marxism, too, does not encourage the needed anthropological empathy with cultural otherness.

Professor Owen offers a history of the Moscow merchants from the Crimean war to the Revolution of 1905, bringing together social, economic, political, and ideological factors peculiar to that important yet geographically limited group. He does not deal with the differently conditioned "capitalists" of St Petersburg, Poland, or the Ukraine, omitting thus many aspects relevant to a consideration of capitalism and politics (whatever these terms may mean when applied to Russia in the late imperial era). In regard to the Moscow merchants his history is based on impressive scholarship drawing on the limited documents available.

With their help the author traces the emergence of the Moscow merchants as a self-conscious group under the impact of the Crimean war and western industrialism. Lacking capacity for expressing their own state-of-mind they sought help from Slavophiles, pan-Slavs, and even the proponents of Official Nationality, weaving their own need for modernization and tariff protection into these ideologies, sometimes with glaring inconsistencies (not noted by the author). We see the merchants' ambiguous attitude toward autocracy, observing the convergence between merchant interests and government policy as well as their continued conflict, none more marked than over labour legislation. Excellent passages deal with the Moscow city Duma, relations with *zemstvo* liberals, and the growing concern over questions of foreign trade and policy. A major chapter accounts for the rise and fall of the merchants' liaison with the liberal opposition in the fateful year of 1905. Throughout we find a plethora of names of leading merchants (who, however, remain lifeless in the narrative) as well as of sociological background data. The appendix provides an impressive table listing the affiliations of eighty-seven merchants with various organizations and their marriage alliances. These details are invaluable to interested scholars.

The weaknesses of Professor Owen's approach show up in his effort to find a comprehensive interpretation for the documented evidence. According to his Preface, he wants to trace the cultural, economic, and political evolution of the Moscow merchants in a triad of familiar terms: *traditional*, *capitalist* (with reference to Max Weber but modified in the same breath as *merchant ideology*), and class-conscious *bourgeois* ideology. Yet at the same time he asserts that "the Moscow merchant estate was in fact neither a genuine class nor a bearer of liberalism." In other words, it remained traditional all along.

The contradiction jars the reader at almost every turn. It spoils the conclusion in which Owen asserts that "a new generation of merchant leaders... emerged from the trauma of the Revolution of 1905 as a mature, class-conscious bourgeoisie," but offers only contrary evidence; two pages later we read that "Given the mutual reinforcement of ... economic and cultural factors it appeared highly unlikely that a liberal political consciousness would develop within this social group." The author's bottom line on the Moscow merchants is that "Russian nationalism, articulated persuasively by the prophets of Slavophilism and Official Nationality, outweighed the desire for economic and political

freedom." In other words, his frequently used terms "bourgeois" and "liberal", like his analytic framework, make little sense. In general he shows only a limited grasp of basic Russian realities before 1917.

Despite the author's claim to the contrary, the book thus hardly contributes to the "comparative social and political history of modern revolutions." Yet it offers useful evidence for an appropriate assessment in that context. Moscow merchantdom in its backwardness, one might argue, was caught in all the contradictions of reluctant adjustment to modernity enforced from western Europe. The closest parallels to its evolution, therefore, were not the progressive western capitalists, but the merchants of other Eurasian countries who by the nature of their business craved stability and in their defensive ignorance dreaded social change. Trying to cling to their identity the Moscow merchants never acquired any insight into the socio-political realities in the world that prepared their doom.

Alas, the author possesses little insight himself. His book, like so much of American scholarship on Russia and the Soviet Union, is long on research and short on understanding.

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M. PESSINA (ed.), *Relazioni di Marsilio Landriani sui processi delle manifatture in Europa alla fine del Settecento*, Milan, Edizioni Il Profilo, 1981, pp. xix-420.

This edition of the reports of Marsilio Landriani published by the prestigious *Edizioni Il Profilo* forms part of a wider series in the economic history of Milan and Lombardy in the XVIIIth century. The reports arose from a journey made in the years 1787-9 by Marsilio Landriani, a Milanese patrician who was related to the Borromeo family and a man of considerable expertise in the physical and chemical sciences, at the request of the Austrian government. Landriani visited a number of European countries in which the first stirrings of what was to become the industrial revolution were evident, and his specific task was to report on developments in scientific inquiry and the progress achieved in particular branches of manufacturing in each country.

Aldo De Maddalena's introduction on the economic development of Lombardy in the second half of the XVIIIth century provides an useful description of the background to Landriani's mission. He argues that the growth that had taken place in the region gave Landriani's journey specific purpose. Following the financial reforms introduced by the State governor Pallavicini in the 1750s and the completion of the cadastral survey in 1760, there had been real signs of economic expansion, especially in agriculture where the exports of a number of high value-added products like rice, silk, cheese and butter showed marked expansion. There was, however, little indication of any revival in manufacturing industry, with the sole exception of silk production — but other sectors were

still firmly held back by the guild system and the structure of domestic production. Neither during the reigns of Maria Theresa nor Joseph II were there any signs of progress in manufacturing. Landriani's mission, therefore, was a clear reflection of the Viennese government's concern to find ways of encouraging industrial development in Lombardy and represented 'an example of the wide-ranging efforts to intervene more directly in the field of economics which were to be expressed most fully in the context of what is termed the reformist Enlightenment of the XVIIIth century'.

Landriani's comments filled fourteen reports — which have been critically edited by Mario Pessina — and which were sent to the Austrian government. They contain accounts of economic and social conditions at Geneva, Lyons, Dijon, Basel, Strasburg, Baden, Mainz, Cologne, Eberfeld, Bremen and parts of Holland, Paris, Normandy and some of the leading English, Scots and Irish cities. Shortly after his return to Vienna in 1789 Landriani set off on another journey through "Austrian Germany" which must have provided an ideal complement to the previous tour but of which unfortunately no reports have survived.

The published reports were not all written immediately after Landriani's visits to the places described. Eleven out of the fourteen deal with particular localities, while the last three are devoted to more general subjects — the cotton industry; linen bleaching; paper-making. As it has been remarked, it is in these three that the "most direct and systematic observations, and suggestions for remedies to be applied in Lombardy" are to be found. Landriani looked at these different industries having very much in mind what might be of use in Lombardy, acting as a sort of filter between the theoretical and applied inventions of the countries he visited and the needs of his own country. But the reports, thanks to careful editing and annotation, can also be read as a description of the technological changes that were taking place in these years especially in England but not England alone, and were changing the whole system of manufacturing. Although he concentrated in particular on the cotton, linen bleaching and paper-making industries because these were the ones that he thought most important for Lombardy, he showed a wider interest in anything that would facilitate either qualitatively or quantitatively better and greater production. He provides an extremely detailed description of the Creusot iron works, but equally detailed pictures of ribbon making and other branches of the clothing industries.

How aware was Landriani of the significance of the changes that he observed? Although he recognised that in England and the Low Countries and also elsewhere methods of production were being devised and applied which marked a real break with the past, there is little to indicate that this Milanese nobleman understood that these different developments were about to change the whole economic structure of Europe. The passages on Arkwright, on the spinning jenny and the cotton mills, on the process of iron smelting used at Coalbrookdale

appear through Landriani's optic as discrete and unconnected developments rather than as elements of a single process of change. This is not to suggest that we should expect to find in Landriani a delineation of the general stages and contours of the process of industrialization which have been at the centre of the attention of economic historians from Ashton to Landes: but what is clear is that the changes that Landriani observed could have been transported to other places and contexts only with great difficulty since there was little awareness of the wider social and economic background from which they had sprung. Yet Landriani's sense of proportion and balance, which never gives way to an uncritical acceptance of all that was new, make these accounts an important, indeed in some ways unique, historical document and enable us to see through the eyes of a particularly well qualified and experienced contemporary the changes that were taking place in the European economies in this period. Although they did not lead to any practical action in his native Lombardy, the reports are of considerable documentary interest and importance for this crucial period in modern social and economic history.

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A.P. THIRLWALL (Editor), *Keynes as a Policy Adviser*, London: Macmillan, 1982. pp. xiv + 180.

Keynes College (University of Kent, at Canterbury) has sponsored biennial conferences on aspects of Lord Keynes' life and work. Papers and discussions at the fifth such conference, held in 1980, are assembled in this volume under the editorship of Professor A.P. Thirlwall.

Actually the papers and discussions cover both more and less ground than the title suggests. They are not limited in time to "the *General Theory* and after," but go back in time to World War I (although not to the India Office) for the background to the *Economic Consequences of the Peace*, and touch the postwar discussions centering about Bretton Woods and the American loan. Keynes' "posthumous" paper on the U.S. balance of payments is not mentioned, and guesses as to how "Keynesian" rather than "Kaleckian" his advice to the Attlee Government might have been after 1946 are not made more enlightened by this conference. Furthermore, the last third of the volume is not so much about Keynes himself as about the general problem, highlighted by the "policy adviser" aspect of Keynes' career, of the role of economic advisers in government, particularly in the British government.

The first formal session dealt more generally with Keynes the economic adviser. The papers were by Lord Kaldor and Professor Tom Wilson (Glasgow), with "official" discussions by Professors Donald Moggridge (Cambridge) and A.J. Brown (Leeds). They presented a generally stagnationist Lord Keynes as at once a one-man Council of Economic Advisers and a series of one-man Mis-

sions, consulted all over the monetary-fiscal-international-economic maps, with occasional forays into other areas, such as social security. Keynes seems to have dazzled (and irritated) people as often as he convinced them. ("The man convinced against his will/Is of the same opinion still.") The point is made that one had to be a more intelligent layman than the common or garden successful politician, bureaucrat, or tycoon to profit, on any basis beyond blind faith, from Keynes's advice — even without its occasional undercurrents of condescension and insult. Furthermore, on many issues, Keynes himself was changing his mind, at least tentatively and for purposes of argument, at an alarming rate. Lord Kaldor claims that he was zeroing in by a process of damped oscillations to something like the Wynne Godley (and Kaldor) plan of over-all or macro-economic protectionism as a major support for expansionary macroeconomic policy. I hope Kaldor was wrong; if not only Britain or the U.S. but every major economic power were to fine-tune its protection level upwards to support its foreign balances against successive ravages of fiscal whoopee and currency over-valuation, the future of world trade would be dismal indeed. Could this have been what Keynes himself would have desired?

After the conference, it appears, Lord Balogh submitted a supplementary memorandum to these papers, published here as pp. 114-124, on the "reparations" controversy of the early 1920's, on the validity of Keynes' advice aimed at minimizing these payments. Balogh criticizes all participants in this controversy, Keynes included, for limiting themselves to price-elasticities of demand at supposedly-constant national income levels. Balogh is correct; having made the point in his own classes, the reviewer is glad to find higher authority.

The second session of the conference dealt specifically with the Treaty of Versailles and its *Economic Consequences*. Professor W.H. Carr (Sheffield) presented the main paper, with Professor Robert Skidelsky (Warwick) as official discussant. Novel to this reviewer, at least, was Carr's psycho-historical interpretation of the *Economic Consequences* as, among other things, Keynes' peace offering to pacifist Bloomsbury friends alienated by his participation in the war effort. More important, of course, was the (additional) pro-Keynesian reply to charges made most explicitly by the French economist Erienne Mantoux's *Carthaginian Peace* (1946), to the effect that Keynes' undermining the authority of and support for the Versailles Treaty had unwittingly paved the way, first for Hitler and then for Nazi expansionism, 15-20 years later.

Quantitative issues are raised by these charges, which can probably never be settled. It is true that Keynes' book, exaggeration and hyperbole included, encouraged German nationalism, the *Drang nach Osten* variant among others. It is also true that the almost instinctive swing of British policy and opinion during the 1920s, against France and the Francophile "Little Entente" which temporarily dominated the continent of Europe, could have been anticipated on "balance of power" grounds with no encouragement from Keynes. And I for one would doubt the importance, for either Hitler's accession to power

or Hitler's aggressiveness, of either factor in the absence of the Great Depression throughout the world and the Brüning deflation in Germany itself.<sup>1</sup>

The final paper by Sir Norman Chester (Oxford) on "The Role of Economic Advisers in Government," with formal discussion by Professor A.W. Coats (Nottingham), is, I fear, rather too British and too gossipy for maximum helpfulness to the non-British reader, unless (like my own Canadian and partially-Americanized colleague Craufurd Goodwin), he brings to the material a non-British background in the same problems and can venture useful comparisons or generalizations. The major problems of the paper, however, are world-wide. For example: Should the economic adviser be allocated to a single office or ministry, or should he serve as trouble-shooter over a wide range of issues like Keynes himself? should he have a permanent or temporary "on-leave" appointment? Should he be a political appointee, or should he have security of tenure? How should his loyalties be divided between his discipline, his superiors, and (sometimes) his conscience or even his pocketbook? (My old teacher F.H. Knight used to warn us that "there is no vested interest like the vested interest in an idea.").

This is, in summary, a worthy addition to the burgeoning "Keynes" literature, which any follower of Sir Roy Harrod in pursuit of Keynes' biography, or of the Johnsons in pursuit of his *Shadow*, will surely want to consult.

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<sup>1</sup> Professor Carr claims (p. 92 and n. 23) that the lauded German recovery of the 1920's had turned to stagflation *before* 1929. This is difficult to believe despite the German sources cited.

