REVIEWS OF BOOKS

A. ATTMAN, *The Russian and Polish Markets in International Trade, 1500-1650*. Publications of the Institute of Economic History of Gothenburg University, 26, Göteborg, 1973, pp. 232.

In this book Artur Attman returns again to the areas and the problems which he first studied in the 1940's. In his thesis, which was published in Swedish in 1944, he discussed the role of commerce in the Livonian crisis between 1550 and 1600.[1] The conflict between Swedish, Danish, Polish and Muscovite interests in the Eastern Baltic was, and still remains, a favourite topic of numerous Northern historians who usually attempt to establish one particular cause of the continual wars.[2] Attman's thesis was novel in that it discussed each port, the conditions of their markets and the area of their hinterlands in turn. An important chapter was devoted to their trading balance. The author concentrated in particular on Russia; he argued that the 'Russian market' was in fact a supply market, and he paid little attention to the demand for foreign goods.[3] He described the areas in which wax, hemp, flax, furs and other Russian goods were produced, using a variety of sources but mainly the accounts of travelling merchants. He

[1] ARTUR ATTMAN, *Den ryska marknaden in 1500-talets baltiska politik, 1558-1595*, Lund 1944.

[2] A recent monograph offers an up to date bibliography and a new approach to political questions: KNUD RASMUSSEN, *Die livländische Krise*, Copenhagen 1973.

[3] In this Attman was, and still is, unique among non-Russian scholars who have usually laid greater stress on Russia's demand for foreign goods (especially, if not exclusively, arms) as well as on the possibility of establishing commercial connections with Persia by way of Russia. Cf. SVEN SVENSSON, *Den merkantila bakgrunden till Rysslands anfall på den livländska ordensstaten, 1558*, Lund 1951.

emphasized the unity of the Russian market, in particular the ability to send goods to either the Livonian ports or to Archangel on the White Sea. In order to assess trading balances he collected all possible evidence for the ports between Archangel, Viborg and Gdansk (Danzig), and drew on the scanty data for overland trade as well. The results were almost uniform: Russia had a surplus balance of trade.

Attman's conclusions were severely criticized by Sven A. Nilsson [4] who considered that the deficit of imports to the West was compensated by the surplus balance of payments in Leipzig. Generally speaking Nilsson rejected Attman's main arguments. Subsequent research has concentrated on the mechanisms of payment and on the credit systems in Northern Europe, and also on Baltic trade.

Now Attman has returned to the subject, he has broadened its scope. He does not now deal with commercial policy but adds Polish markets to those of Russia. A comparison of the bibliographies and footnotes in both books indicates the developments that have taken place during the thirty years that elapsed between them. The author consistently revises the earlier text, and a special mark indicates new footnotes and additions to the original ones. New research has added much to our information on the balance of trade in many of the ports and even more about trade in general and Attman himself has added new foreign archives to his earlier sources (Tartu (Estonia), Kiew, Leipzig and Copenhagen). The results which he presents in this book are convincing, and this reviewer's recent research, which is based on different evidence, fully confirms the author's conclusions, but there are several remarks to be made, however.

Customs sources are usually restricted to only a short period and cover only certain ports. The variations in the balance of trade should be measured, for otherwise individual cases are of rather doubtful value. However, it is the consensus of evidence that makes them convincing. It seems that the trade balances did not fluctuate over short periods. But the case of Gdansk, which Attman uses, following Miss M. Bogucka, A. Maczak and J. Schildhauer, is instructive. The value of goods in customs registers was often quoted on the basis of abstract prices drawn from legal taxes. One cannot believe that all cargoes of rye sold in two years (over thirteen hundred items) fetched the same unit price.[5] On the contrary, the English (also Scottish and French) customs declarations filed in the Sound Customs House clearly

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[4] SVEN A. NILSSON's review of A. ATTMAN's, *Den ryska marknaden...* in «Scandia», vol. XVI, 1945.

[5] MARIA BOGUCKA, *Handel zagraniczny Gdanska w pierwszej polowie XVII wieku* (Gdansk's Foreign Trade in the First Half of the XVII century), Wrocław 1970, table XXI: figures for 1641 and 1642. There is more similarly fictitious data and only from 1643 do the prices in the Gdansk customs registers begin to be slightly differentiated. One could quote numerous similar examples for other towns.

prove that gross prices fluctuated strongly according to season and year. C. Biernat's critical remarks on the interpolation of figures where toll registers (*Pfahlbücher*) were lacking also ought to be taken into consideration.[6] It was chance, rather than a real statistical probability, that the customs registers from Gdansk which are still in existence provide exact figures or — to be precise — figures that roughly agree with estimates made on the basis of the Sound Toll Registers.

Greater stress should also be laid on the specialization of the ports. From 1570 to 1630 Gdansk and Elblag (Elbing) formed a system with a clear division of labour: the latter, a principal port of the English Eastland Company, absorbed the majority of foreign cloths, whereas the former concentrated on, and grew rich on, the grain trade. Consequently separate calculations of their trading balances would bring only very limited conclusions. Attman's argument would also gain rather than lose if he disregarded such second-hand evidence as that of « an enormous trading surplus » on the Vistula in the first half of the seventeenth century. That « surplus » was quite simply due to the fact that all heavy goods such as grain, timber, ashes etc., were brought to Gdansk down the river, whereas it was pointless to transport the imported commodities up river, as it was slow and difficult.[7]

In keeping strictly to the plan of his earlier work Attman has made it more difficult to broaden his new survey. What happened to gold and silver imports in East and Central Europe? Why did the surplus in the balance of trade not raise the price level? Attman briefly refers to the hoarding of money by the rich and the monasteries in Russia, and much money was also sent on to Persia. But the surplus from Polish Baltic trade also hardly saturated the country's economy. Attman provides evidence showing that gold and silver were exported to Turkey, and most probably to Muscovy and other countries.[8] But what is still more important is that because of Poland's

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[6] Cf. Czesław Biernat, *Apogeum handlu gdanskiego w perwszej połowie XVII w.* (The Apogee of Gdansk Commerce in the First Half of the XVII century), « Zapiski Historyczne », vol. XXXVII, 1971. Attman quotes this paper but does not discuss its conclusions. I agree with Bogucka on the timing of the commercial boom in Gdansk, but Biernat's criticism of the sources should not be dismissed.

[7] Attman, *The Russian and Polish Markets*, p. 149. These estimates were made by Honorate Obuchowska-Pysiowa on the basis of the river toll registers.

[8] Attman, *Ibid.*, pp. 161-172. The evidence of a surplus for Turkey in its trade with Poland is abundant: e.g. *The Travels of John Sanderson,* Works issued by the Hakluyt Society, 2nd series, vol. LXVII, 1930, p. 169. John Sanderson to Richard Stapler, from Pera, 17th April 1597: « Divers merchants come latelie out of Poland; they have bought wyer, lattin plates, knives; neither coniskins nor tinne, but most redie mony ». Less serious evidence comes from a description of the Carpathian mountain robbers, who reportedly decorated their hats and long moustaches with English gold coins (rosenobles); *Ungarischer oder Dacianischer Simplicissimus...* 1683, chap. XVII. I used the Polish edition *Wegierski badź dacki Simplicissimus,* translated by Danuta Reychmanowa, Cracow 1967.

social and economic structure, money passed directly from the hands of foreign and Gdansk merchants into the coffers of the rich landowners, who spent it mainly on foreign commodities, so that very little money was invested in the country's economy. An attempt could also be made to compare the value of the money surplus in the Baltic trade with population figures, and although any comparison must remain very approximate, a probable estimate would work out at a few groschen per head, that is only the daily wages of a journeyman. So the impressive percentages of export surplus may not have been of equal importance in terms of the GNP.

The post-war debates on commerce in Northern Europe in the XVII century often concentrated on credit and on money circulation. Attman only touches on these problems which is sometimes detrimental to his argument on the balance of trade. The only serious omission in the bibliography is Zdzisław Sadowski's *Coinage and the Beginnings of Poland's decline in the XVIII century* (which has not been reviewed since it first appeared ten years ago). This was written by an economist who had no knowledge of Polish commerce in the XVII century but who had a thorough understanding of currency problems. On the basis of discussions of the money market and contemporary economics in Poland, Sadowski showed that at least in the 1620's the trade balance went into deficit. This is important as Sadowski's book, as well as the whole course of recent research, should direct those studying trading problems to look at monetary problems as well. When the main problems of trading balances seem to be settled one ought not to be satisfied. The East European region should then be studied not only as supply markets but also as complex and interdependent social and economic structures.

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A. H. R. BAKER and R. A. BUTLIN (eds.) *Studies of Field Systems in the British Isles*, Cambridge: at the University Press, 1973, pp. XXVI-702, numerous maps and tables.

Since economic historians will use this bulky, immensely scholarly book as a source of primary data on agricultural organization it as appropriate to list its contents as to indicate its scope. The body of the text consists of

[9] ZDZISLAW SADOWSKI, *Pieniadz a poczatki upadku Rzeczypospolitej w XVII wieku*, Warsaw 1964. Cf. also BARRY E. SUPPLE, *Commercial Crisis and Change in England, 1600-1642*, Cambridge 1959, pp. 89-98 etc., where the English trade balance is discussed in relation to the Eastland (i. e. Baltic) trade. I also try to prove that the rates for bills of exchange drawn in Amsterdam on Gdansk from 1625-1650 reflect the actual balance of trade exactly: ANTONI MACZAK, *Miedzy Gdanskiem a Sundem. Studia nad handlem baltyckim od polowy XVI do polowy XVII w.* (Between Gdansk and the Sound. Studies in the Eastland Trade 1550-1650), Warsaw 1973, chap. VIII on the Dutch trading balance with the Eastland.

twelve substantial studies of the evolution of field systems in eight English regions, plus North Wales, South Wales, Scotland and Ireland. All but one of the contributors (as well as both editors) are geographers. They have produced a collection of finely-detailed researches and extended descriptive writings. The English regions which they discuss are the Northwest; Northumberland and Durham; Yorkshire; the West Midlands; the East Midlands; East Anglia; the Chiltern Hills and their surrounds; and their surrounds and the Southeast. Within these broad regions some localities and even counties are understandably treated in greater depth than others, reflecting some spatial uneveness in research to date, although uneveness is more apparent in the different periods emphasized by contributors: the geographical coverage is really very thorough. The only conspicuous gaps are south-central and south-western England. In addition to the regional chapters, the editors supply an introductory chapter on the difficulties in generalization and on the processes of development in field systems.

The basic concern of historical geographers with field systems is with reconstructing their status at given dates and tracing the evolution of their pattern through time. The period chiefly dealt with here runs from the late middle ages to the seventeenth and eighteenth centuries. A majority of the chapters describe in great detail the arrangement of the fields associated with the settlements of each of a number of topographical sub-regions, drawing heavily on local illustration. The main evolutionary shift identified in field systems (and agricultural organization and production) took place between the late middle ages and about 1700. The timing of the change was regionally staggered and a great deal of evidence is provided on this as well as on the ability of communal systems, though failing overall, to adjust to changing circumstances.

All the authors recognize this malleability of field systems. They certainly present the layout of field systems as of central scholarly interest, a viewpoint which few economic historians will share, but also as the spatial manifestation of ways in which past communities ordered and reordered the division and grouping of units of their farmland. The contributors, to greater or lesser extents, find the motive forces behind rearrangement in changing population densities, levels of demand, and new methods of farming. Although they sometimes disturbingly equate population and demand, these matters are all of concern to economic historians. On this dynamic view, field systems are the physical expression of changing institutional modes of sharing out the immobile and (in general) increasingly costly factor, land. Because of this ultimate overlap in interest, economic historians will be able to derive from these studies material on alterations in property rights, communal decision-making, land use, crop mixes, and husbandry techniques, material which emerges in the course of the geographers' delicate labour of reconstructing the morphology of the agrarian landscape.

Obviously, and as indeed the editors insist, the proliferation of local studies of former field systems during the last decade or two has made some earlier generalizations about the origin and distribution of types seem untenable. The particularist and formalist approach of historical geography, rather more than the accidental irregularity of local research, does not however readily make for synthesizing. It is consistent with this that the chapters on Scotland and Ireland, whose authors had to stand farther back from the minutiae than those dealing with smaller English regions, are clearest on the outlines of agrarian change. It is noticeable, too, that the generalizations most fruitful of research have come and still do come from the economic historians. The editors are quite frank about the derivative character of historical geography and the limitations of its preferred sources, maps: « in themselves, however, they usually convey little information on either agrarian function or genesis » (p. 16). The editors indicate strongly that the present is a time when a new synthesis could contribute more than additional case studies. They do not themselves offer any new general interpretation by proposing the model from Ester Boserup in which rising intensities of cultivation and complexities of rotation are made to depend on the increase of population. The degree of intricacy of field systems — seen by the editors as developing from infield-outfield to the more and more elaborate systems of larger and larger settlements — is thus to be explained as density-dependent. After the extreme sensitivity to local peculiarities shown throughout the book this seems a switch to over-generality. Nevertheless it offers the hope that students of field systems will turn to demographic (and market) analysis and put greater stress on the economic purpose of various arrangements of fields. This will be a welcome outgrowth from a concentration on the structure of field systems. Historical geography is surely somewhere along the academic continuum with economic history and economic development; its genuinely admirable scholarship could be better absorbed by these neighbouring disciplines if it adopted a more functional approach.

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R. Forster, *The House of Saulx-Tavanes: Versailles and Burgundy, 1700-1830,* The Johns Hopkins Press, Baltimore and London, 1971, pp. X+277, $ 15.00.

The sub-title of Robert Forster's study of a French noble family points to the book's major virtue, that of bridging the two great gaps, temporal and spatial, that cut across modern French history. By focusing on the last decades of the *Ancien Régime* as well as the Revolution, and by following the fortunes of the family in Versailles (or, more to the point, Paris) and in Burgundy, he reminds us of continuities in French history too easily obscured by routine compartmentalization.

As to the family, they are somewhat disappointing. First because they remain a rather wooden lot, particularly the men, whose rare recorded utterances run to arrogance and insensitivity. But disappointing also because the Saulx-Tavanes embody so perfectly the too-conventional figure of the French courtier, from their domestication to the extravagance of their life-style and their snuffing-out in the coarse and prosaic burgeois century. Not all noble lineages were so parasitic in the 18th century or so definitively eclipsed by the fall of the *Ancien Régime.* Forster knows this, of course (though many people understimate the continuing place descendants of the privileged 2% have occupied in French society), and he must report the disappearance of the family (by failure of the male line) as it happened. Perhaps the bias is built in: except in revolution, the papers of French families remain private until the last of the line is long buried.

The non-specialist, and this reader is one, will be most interested in the intersection of the family history with a number of larger historical and historiographic questions. Glossing over Forster's considerable, and largely successful, efforts at gathering and organizing documentary evidence, including quantitative data, we shall turn to some of these. Suffice it to say that this is a readable as well as an informative book. Forster, however, gives little emphasis to contributions the monograph might make to various historical interpretations and debates. Familiar with the relevant literature, he may simply have chosen to reserve for other occasions entanglement in what are often messy, not to say bitter, controversies. But the general reader may well want a few roadside markers.

First a few brief comments. Forster helps tidy up one old problem, the fate of *émigré* property. Since noble forests were not (at least in this case) sold off under the Republic, and the Restoration deducted debts and seigneurial rights from the value of sequestered properties, compensation involved only manageable sums. In fact, the Saulx-Tavanes restitution, some 280 francs, was among the thirty largest such payments (p. 60).

In other cases, the story raises questions rather than contributing answers. Was it unusual for laymen to command extensive tithing rights (*dîmes inféodées*)? Was the economic offensive (seigneurial reaction) of the 1780's motivated by « need » or, as others have suggested, by emulation of more business-minded bourgeois landlords and « seigneurs »? Finally, what of the so-called « rural bourgeoisie » and the Revolution? While this is far too complex a problem to treat here, Forster's evidence suggests that the rural élites have been miscast and underestimated by doctrinaire refusals to see that they could perfectly well fit into the old system and still lead, and profit from, the attack that overthrew it. Non-privileged and local, enterprising and literate, they enjoyed enough power and respect to offset the resentment or envy generated by their multiple roles as creditors, lawyers, or landlords, not to mention grain dealers, rent collectors, local officials, and employers.

We turn now to two major questions of particular interest to the economic historian. The first, admirably served by such a multi-generational history, relates to the precarious financial/demographic equilibrium of noble families in the *Ancien Régime*. Given the imperative necessity to avoid social descent even for younger offspring, and the often self-defeating nature of such avenues of financial gain as were open, it is not excessive to characterize the ecological situation of the families as tragic. Specifically, they were threatened by happiness itself in the form of numerous and fertile marriages or long lifespans.

To begin with, the Saulx-Tavanes were not, in the relevant sense, really wealthy, though their financial position and rank placed them just below the very top of French nobility. With a gross income of 94,000 *livres* in 1763 and 165 000 twenty-five years later, they may appear scandalously opulent when 300 *livres* was a good annual working wage. But net income (after taxes and debt service) fell perhaps 20,000 *livres* short of covering even the current expenses of maintaining the family's rank in society. Relative costs are interesting, by the way: legal fees, along with clothing and other « manufactures », loom large in the budget, while food, servants' wages, education, and charity absorbed only modest amounts.

The real financial story is not in these flows, however, but in the large and partly uncontrollable changes in the stock of wealth associated with the milestones of family life. Happy events were generally difficult, for example children coming of age, while deaths and arranged marriages provided a lift, often a crucial one. In particular, all surviving offspring had to be provided for: marriage portions were onerous, but commissions and church posts also cost substantially. Yet the chief recourse, the royal purse, demanded thoroughgoing dedication to the parasitic life of the courtier: constant in attendance, visibly idle, ostentatiously prodigal of purse.

Forster notes the social and economic consequences for France: profits drained from enterprise by the social ascension of the rich (as well as by tax exemptions and transfer payments favouring the passive nobility). Yet nobles families too were threatened. A narrow channel ran between bankruptcy and extinction, the more so as daughters claimed some share of the wealth while providing no help to the survival of the line. In the twilight years of the House of Saulx-Tavanes, family claims on the estate, under the new and egalitarian *Code Civile*, were settled with difficulty, only to have the line die out. Otherwise, the post-Revolutionary nobility, now free of the restrictions imposed by caste and court life, could more easily hold its financial own under the rules governing bourgeois France that it had previously. Indeed, most noble families undoubtedly did so.

The second topic on which we can touch is that of estate management and rural enterprise in France . The *Ancien Régime* has often been called « feudal », and the Saulx-Tavanes were nothing if not representative of the old economic system. Yet the second Duke reminds this reader of no one so much as of that quintessential bourgeois of the next century, Henri Germain. The Lyons' banker, like the Burgundian landlord and courtier, would commit no resources to enterprise based on new technology. Both pursued day-to-day profit by resolutely minimizing inputs, but devoted their real attention to big deals typically involving state finances. And both drained capital from the periphery toward the center.

What entrepreneurial possibilities were open to the Saulx-Tavanes? In comparison with English counterparts, they must be reckoned squires rather than magnates. The Burgundy estates consisted of 8 000 acres, of which over 5 000 were forest and the rest badly scattered in small parcels. Only 40 000 *livres* of annual revenue derived from these estates (1780), less than the income from royal pensions and seigneurial rights. By contrast, a large English estate ranged from « several thousand » to 40-50,000 acres, with gross revenues at least ten times what the Saulx-Tavanes received.[1] The passive nature of their estate management is brought out by the low salaries paid to estate agents (something like a tenth the prevailing English scale for stewards on large estates), suggesting other interests, and also by the arrangement Forster confusingly calls the « principal tenant ». These men were, in fact, « farmers » in the other sense of intermediaries, advancing the rent for one of the three Burgundian estates and monetizing the sub-rents they collected in kind, all this for a small fraction of the gross rental in lieu of fee.

With an absentee landlord, part-time agents, and farmers who only collected rents, what wonder that agriculture progressed little? There was neither investment by the seigneur nor willingness to share in the financing of tenant improvements, while the nine-year leases forbade change in agricultural practices. Repairs were grudgingly made, and tenant turnover was encouraged at least in the last decades of the *Ancien Régime*. As for enclosure, no hint of it. Yet not all of France was so static. A Saulx-Tavanes had drained marshland at mid-century, while later refusals to participate in investment testify to continuing initiatives of tenants. There was tension between tradition and change in the economy of pre-Revolutionary France, even if the line did not run neatly between the Second and Third Estates.

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1 G. E. Mingay, *The Eighteenth Century Land Steward*, in E.L. Jones and G.E. Mingay, Eds., « Land, Labour and Population in the Industrial Revolution » (London, Arnold, 1967), pp. 3-4.

Not the least of the irony in the story of the Saulx-Tavanes is that their account books and prosaic money worries are more alive today than their exalted rank and proud finery. Forster's study helps us see the practical and ordinary sides of events we know so well as drama and epic. Under that demythifying influence, one may be permitted to wonder whether, in the end, the principal economic effect of the Great Revolution was not merely to democratize a French predilection for property over enterprise that made the Saulx-Tavanes at once too opulent and too insolvent to survive.

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S. GOLDENBERG, *Columb. Omul şi fapta,* Cluj, Editura Dacia, 1973, pages 222, (one map).

Until recent years Romanian historians have shown little interest in the problems arising from the great voyages of discovery in the general process of maritime expansion. This was certainly true of strictly academic studies based on structural theories. In this context Goldenberg's monograph certainly increases our knowledge and also provides a model for a structural interpretation of the problem defined below.

The success of a monograph is extremely complex, and to some degree ambivalent. The form of study requires that the phases of historical investigation be interrelated, set in a general context and, in a very skilful way, co-ordinated. The first step, documentation, calls for the accumulation and classification of facts; the second, critical interpretation, demands a total view, an attempt at generalization and comparison. It is the correct mixing of these ingredients that ensures success. Documentation that is too dense, the mere amassing of facts as such, only serves to obscure the text and make it difficult to follow. Even critical interpretation has to be kept within reasonable limits, so that the unlimited expansion of the theoretical framework does not obscure the concrete realities of time and events which are always indispensable to any historical work. As a result the monograph ideally requires an equal distribution of these two methods. And in S. Goldenberg's study this happy balance between documentation and criticism is achieved.

One of the first things to strike the reader is the author's use of climate as an explanatory factor in the study. In the chapter devoted to the Vikings' expansion and sudden decline in particular, Goldenberg stresses the favourable climatic conditions that prevailed in Northern Europe between 800 and 1200, a period which has been described technically as the « second climatic optimum ». This, the author stresses, prompted the « Viking adventure » but was not its cause. But relating this climatic situation to the more essential factor of social and economic changes that came about in the structure of Norman society does provide a more complete interpretation of the expansion. Although the study revolves around the legendary figure of Christopher Columbus, Goldenberg avoids the temptation to indulge in apologetics and to add romantic details to the portrait of the leading figure of the age of the great discoveries. He seems to share the opinion of the distinguished Romanian literary critic, Edgar Papu, who, in his book entitled « The Voyages of the Renaissance » (Călătoriile Renaşterii) set the trans-oceanic voyages of Iberian seafarers against this more general picture. Both Columbus the man and his exploits are seen in terms of the general framework of the period.

Taking this position, Goldenberg completely demolishes the traditional view that the great discoveries came about when the land routes for trade to Asia were blocked by robbers. The author argues that the cause lay in the broadening of scientific horizons, which was speeded by the flood of technical inventions, the advances made in nautical astronomy, navigational instruments, the development of cartography and the construction of caravels, — in all these the author sees the real stimuli to maritime expansion. In the same context Goldenberg points to the cardinal importance of the growth of capital, the blossoming of humanism and the scientific spirit in general. With regard to these factors the beginning of the great discoveries does not only mark the progress into a more advanced time-scale but also into a more extended area of embarkation, running from Italy to the shores of the Ocean. If we see the phenomenon as a whole the Italian prelude is not merely a reminder of something dead and gone but forms a sort of pre-history to the European conquest of the world.

It was completely natural to find the Italians among the first to embark on the great adventure, driven as they were by the three-fold economic, scientific and moral climate which, in the XIV and XV centuries, was restricted almost exclusively to the Italian peninsula. Goldenberg points out that the achievements of the Genoese navigators could well demonstrate the victory of the law of historical progress over the instinct of self-preservation.

But Christopher Columbus and his exploits were not directly connected with the part played by Genoa in the enterprises of discovery and colonization. His famous voyage does not form a chapter in the economic and maritime history of Genoa. Nevertheless during his career as a navigator he pursued those new horizons which the Genoese had foreseen. The starting point for his great achievement came at the end of the XV century when he settled in Spain, where he became Cristóbal Colón, just as the Venetian Giovanni Cabotto became John Cabot on his arrival in England. These changes of identity, to which one could add many more examples, are of an almost symbolic importance.

The industrial and commercial advances of the second half of the XV century and the ever-increasing volume of trade, which was made possible by the more peaceful atmosphere in international relations after the end of the Hundred Years War, all brought about great development in the means of exchange. But the scarcity of precious metals in the West — a consequence of the deficit in the balance of trade with the East and a limited improvement in mineral resources — was a serious obstacle to the circulation of currency. It was not only in international trade that the lack of specie presented problems. This was also true in the political field for centralised and absolute monarchies with their more efficient bureaucratic machinery and larger armies. Consequently the only possible solution was offered by Columbus's plans to explore the area beyond the limits of the Portuguese maritime and commercial monopoly which he presented to the kings of Spain just when the Crown's resources had been exhausted by the *Reconquista.* The rivalry between the two Iberian states was then to be ended by expansion in the only direction that was still open, across the Atlantic towards the West to the supposed Eldorado of Asia.

This book leads us to the conclusion that the personality of the famous explorer was conditioned by the complex circumstances surrounding him. One cannot doubt that this psychological and emotional temperament left some mark on events. In our opinion the structural approach to history, which this monograph employs, as well as the authentic « epistomological section » that the author carries out on Western civilization at the end of the XV century, determine the real success of the work. And this lies not simply in solving problems, but also in making new ones emerge.

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H. G. JOHNSON and A. K. SWOBODA (eds.), *The Economics of Common Currencies*, Proceedings of the Madrid Conference on Optimum Currency Areas, Harvard University Press, Cambridge, Mass., 1973, pp. 302.

It is not possible to recommend this book unreservedly to economic historians of Europe. While there are a number of papers which will interest monetary historians — on which more presently — the bulk of them lie in international monetary theory. Presented in March 1970 at a conference in Madrid, one in a series of such conferences held in recent years, they use a short-hand jargon which is likely to be incomprehensible to many. The economic historian will recognize many familiar concepts — Gresham's law, the price-specie-flow mechanism, seigniorage, etc. He is unlikely, however, unless he has followed the discussion, to be fully up on optimum currency areas, the assignment problem, money illusion as it relates to exchange-rate changes, the liquidity basis of balance-of-payments, etc., concepts which the participants bandy about without explanation. The papers, moreover, are highly uneven. Several run for only a few pages. Discussion of the papers is confined to two. Several are summaries of theses only vaguely related to the central theme. The text is marred by sloppy copy-editing and proof-reading. Despite these shortcomings, historians of the economy of Europe may wish to note a number of points about *The Economics of Common Currencies.*

In the first place, international monetary analysis has left Keynes and returned to the world of Hume. One paper by S. W. Arndt does use the Keynesian assumption that the price level is fixed, but in virtually all others, prices are free to move, and this movement even serves as the critical variable in the adjustment process. A fascinating essay by Jürg Niehans suggests that the gold-exchange standard can be stabilized in the face of Gresham's law, and even prove superior to a pure gold standard, much as Jacques Rueff used to but no longer advocate, or a pure dollar standard, provided that the price level is held steady. The analysis is elegant even though the economic result is trivial: a stable price level maintains the production of gold and requires discipline in the issuance of, say, dollars. If both monies are issued in constant proportions, Gresham's law is held in abeyance on the balanced-growth path, under bimetallism or any other two-money system. But the economic historian can only applaud Niehans' and especially his *C-C* curve in Figures 2.1 and 2.2 beyond which stability collapses.

Secondly, two papers by R. A. Mundell, who organized the conference, are worth noting, together with their acute criticism by B. Balassa. In « Uncommon Arguments for Common Currencies » Mundell maintains that a common currency reduces the risks of disturbances by random fluctuations such as war, crop failures, strikes, etc. The model is a curious one, postulating two countries with counterpoised seasonal production of a single non-storageable commodity. In this case, a flexible exchange rate which requires balance of exports and imports of each country each season will kill off the two countries successively as neither has anything to sell for imports of food in the dead season. A more general argument can be made that international money is needed as an international store of value to convert income earned unevenly over time into continuous consumption. (If the product had been storageable, domestic money would suffice).

The other Mundell paper, « A Plan for a European Currency », is misnamed. It is less a plan than a plea, full of rhetorical ellipses — « Which bank is more likely to ' fail ', the Bank of France or the First National City Bank »? « Anything a lira can do a Euro-dollar can do better »; « It seems too obvious to mention the ease with which the U.S. balance-of-payments problem... could be solved once Europe has created its own regional money ». Hortatory passages and purple writing are interspersed with potted history of European monetary unity under the Roman-Byzantine Empire, which will interest some historians, and set on edge the teeth of others.

A. B. Laffer's « Two Arguments for Fixed Rates » are really one, and closely relate to Mundell's « Uncommon Argument ». Fixed rates and monetary integration increase stability when the disturbance is a domestic one. Flexible exchange rates are destabilizing domestically, and, if the disturbance is transitory, set in motion resource allocations which must later be reversed. But as G. Haberler points out, a fixed rate communicates outside instability to a stable economy, and a flexible rate fends it off. Which of these analytical models is more relevant is a good question for economic historians who have watched the continuous widening of financial markets, nationally and internationally, up to 1929 and after an interlude, from 1945 to 1970 or so.

The paper on the price-specie-flow mechanism by T. J. Courchene presents a formal model inconclusively tested by econometrics, with pious initial and concluding quotations from David Hume. T. Guggenheim presents a very brief account of « Some Early Views on Monetary Integration » with quotations from Copernicus, Bodin and a French statesman, de Parieu, but nothing on the Italian and German monetary unifications of 1860 and 1870, respectively. A four and one-half page discussion (plus tables) by A. Kafka on Latin America makes the interesting suggestion that multiple exchange rates are a form of regional exchange depreciation. To the extent that regions trade mostly with other regions inside the country, rather than with foreign countries, this idea, while bright, is unacceptable. For the rest there are standard-quality, international-monetary papers by A. Swoboda, H. G. Grubel and R. A. Aliber; two previously-published papers on the crawling peg by the Yale contingent, R. W. Cooper and W. Fellner; extracts and summaries from only dimly related theses by R. M. Dunn, Jr., R. Almonacid and M. Guitian. If Harry Johnson, the editor, did not pull the collection together with an analytical synthesis, it may be because he had trouble finding the unifying theme.

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R. E. Jones, *The Emancipation of the Russian Nobility (1762-1785)*, University Press, Princeton, 1973, pp. XII, 326.

The relationship between the state and social classes in Russia remains a crucial and much disputed problem. Did the autocracy rule above the classes in the name of the national interest or did its policies reflect the conflicting interests of those classes? A bias toward one or the other of these interpretations penetrated Russian historiography long before the revolution. After 1917 the polarization between the two positions increased at least until recently when efforts were made on both sides to shade the arguments and thus reduce the gap separating political-institutional history and socio-economic history.

In this monograph on the interaction of the state and the nobility under the reign of Catherine II (the Great), Robert E. Jones lines up with the partisans of the neo-state school, but he does not neglect the social dimensions of autocratic politics. He takes the position that the « golden age of the Russian nobility » was more the product of a conscious policy by the state to forge an alliance with the nobility in order to guarantee national security and internal stability rather than a series of concessions wrested from it by a rising noble class. Arguing convincingly that the Russian nobility never achieved the level of social cohesion and class consciousness necessary to establish a political programme, he demonstrates that even before Catherine the reduction of the nobles' service obligations reflected the military and administrative requirements of the state. When Catherine seized power, however, the changing position of the nobility in the state and society forced her to confront two dangerous questions. As a usurper in need of consolidating her power quickly, she sought to balance off rival leaders of the court factions who were themselves powerful nobles. At the same time she inherited « a situation unprecedented in eighteenth century Russia, the emergence of a provincial nobility, privileged yet separated from the state service, whose way of life was in conflict with the laws and institutions established by Peter the Great » (p. 46). How could she retain the autocratic form of government and her own personal independence while at the same time transform the provincial nobility into a satisfied and effective branch of the central government? The rest of her reign was devoted to pursuing two parallel and sometimes contradictory policies. Beginning with the Senatorial Reform in 1763, continuing with the reorganization of the functions of the governors-general and culminating in the expropriation of the Church lands and serfs, Catherine opted for the bureaucratic solution. In criticizing the failure of these measures Jones correctly stresses the shortage of trained administrative personnel and the deeply rooted differences between the military and fiscal needs of the state and the local needs of the provincial nobility. He neglects another basic fault in these plans, the conflict between the principles of centralization and devolution which, as S. Frederick Starr points out in his recent work, *Decentralization and Self Government in Russia*, recurs in the nineteenth century in much the same form. Moreover, the unresolved jurisdictional disputes between bureaucrats in the capital and in the provinces were further complicated by the attempts of governors-general, such as Sievers in the Baltic provinces and Rumiantsev in the Ukraine, to build their own regional power bases by converting the particularist sentiments of the local non-Russian nobility into support for their own schemes of administrative reform. The ethnic-regional aspects of eighteenth century politics deserves a fresh and detailed examination and its omission here is regrettable.

Parallel to the bureaucratic option, Catherine sought to involve the nobility directly in the governmental process without conceding it any real power.

Her first initiative in this direction was to provide the nobility with the opportunity to redefine their own rights by drawing up notebook of grievances (*nakazy*) and electing delegates to a Legislative Commission. Disillusioned by what she considered the display of narrow class interest, Catherine allowed the Commission to wither away, but not before she had gained valuable insights into the nature of the nobles' opposition. Above all « it posed no direct political threat to Catherine or her government » (p. 162), and there was no danger of a *fronde*. Nevertheless, the Pugachev uprising in 1774 which shook the government to its foundations forcefully demonstrated that the state bureaucracy alone was incapable of ruling the provinces. Even as Catherine strengthened the bureaucracy in the provinces, she also increased the role of elective representatives of the provincial nobility especially in those areas where the nobles themselves had already expressed grievances. Jones maintains that the nobles initially responded energetically and enthusiastically to their new rights and duties. This encouraged Catherine to complete the process of emancipating them by promulgating the Charter of the Nobility in 1785 which « imposed a rational, comprehensive and comprehensible order on public affairs » (p. 287). His balanced critique of the Charter concludes that Catherine had done her best for Russia but the Russian nobles failed to fulfil their part of the alliance. The evaluation remains inconclusive in part because the author has so little to say about the effect of Catherine's legislation on the nobles' economic position which in the long run had a great deal to do with the disintegration of provincial life. Although his thesis is less revisionist than he claims, the monograph provides the best introduction in English to Catherine's policies and is a considerable improvement in both style and analytical power over Paul Dukes, *Catherine the Great and the Russian Nobility*.

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R. Katzenstein, *Technischer Fortschritt, Kapitalbewegung - Kapitalfixierung*, Akademie-Verlag, Berlin, 1971, pp. 222.

R. Katzenstein, *Zur Frage des tendenziellen Falls der Profitrate*, in: « Beitrage zur ' Stamokap ' Debatte », No. 8 der Hefte zu politischen Gegenswartsfragen, Pahl-Rugenstein Verlag, 1973, Köln, pp. 37-48.

At a time when crises seem to characterize the western economies, and interpretations are attempted in terms of waning or continuing trade cycles, or are seen as new stages in the intensification of the crisis of the capitalist system, it is again technological progress that is and has been attracting considerable attention on the part of economists. J. Steindl writes [1] that

[1] Josef Steindl, *Capitalism, Science and Technology*, in « Socialism, Capitalism and Economic Growth », C. H. Feinstein edit., Cambridge University Press 1967, p. 205.

« Technical progress is a child of war; of haphazard shortsighted business interests », while Maurice Dobb notices the « curious feature of the American statistical series of production and employment that the year 1919 constituted, apparently, a watershed. In the decades prior to this, expanded production had come predominantly from an expansion in the labor force, with higher productivity per man playing a subordinate role ». Subsequent to that date, the expansion of production « was mainly built on higher productivity ».[2]

For Robert Katzenstein, in the book here under review, technical progress is in its fundamental aspects a process which decreases « necessary labour » in favour of additionally available labour (Mehrarbeit). As such it enlarges « the scale of possibilities for the development of social production in every direction... it creates new spheres for the movement of capital » (p. 7). The result of this process, and at the same time its very condition, is a continued change (Umschichtung) in the relation of living to frozen labour. This process adds also to a new distribution of social labour, though the latter may have been caused by different factors. Technological progress in thus one of the most important factors influencing and stimulating economic growth.

The concrete setting for this analysis, Dr Katzenstein finds in the major technological revolution presently under way, the beginning of which he sees in middle Europe and Japan (but not in the United States) in 1957-58. He shows, using the example of the development of the Federal Republic of Germany, the main direction of the structural changes in the manufacturing industries, with an above average development in the mineral oil and chemical industries, and a far above average growth in the electrotechnical and automobile industries, but a below average performance in the consumer goods industries; there was no area of economic activity in which investment per employee had not increased. The size of investment per employee had in fact increased dramatically: eight times in the building industry, seven times in agriculture, and in energy production more than three times. In assessing the structural changes and their influence upon the fixed part of the total social capital, Dr Katzenstein concludes that indeed a broad transformation of a technical character had taken place, and that this transformation was primarily characterized by the fact that it succeeded in lifting the production techniques of relatively retarded industrial fields to a higher technological level. It was this development of the progressive socialization of industrial production that prepared — and provided the perspectives for — the real and quick progress of the technological revolution on the broadest basis.

These highly interesting, precise and detailed investigations are then followed by an analysis of the specific forms which the process of technical

[2] Maurice Dobb, *Papers on Capitalism, Development and Planning,* International Publishers, New York 1967, pp. 38-39.

progress assumes within the frame of capitalist production relations. Dr Katzenstein shows convincingly that the objective limitations to technological investment (*e.g.*, that the extra profit produced must be net after the destruction of the old investment; and that the profitability of modern technology presupposes such a high degree of socialization of production, that in the given economic structure it may not be available, and/or may well be impossible to generate) can yet never lead to a cessation of further development of the productive forces. The real problem lies, therefore, in the dialectic relation between the increase in the degree of socialization of production, and the capitalist productive relations. The limits for the development of the productive forces are a clear expression of the kind of problems which capitalist development has to face. Thus, to appreciate the limits to technological progress under conditions of state-monopoly capitalism, it is necessary to see that it is the inherent and unavoidable problem of the latter to try to break through the narrowness of the private property relationships that are basic to capitalism, in order to create the conditions for the development of social production *qua* profit production. The present form of capitalism does not overcome private property relations — which are, after all, the very means for the private appropriation of social production — but tries to make the activity of private capital possible under the present-day forms of the socialization of production. Examples of the latest stage in this contradictory development are seen in the purchase by the Federal German State of entire coal mines, or in the case of Italy state purchase of a major part of the share-capital of the chemical firm of Montecatini-Edison in order to be able to insist on certain policies considered necessary in the interest of the economy as a whole.

As to other impacts of the technical revolution: Dr Katzenstein sees the problem of unemployment today primarily as an immediate result of the present technical revolution rather than as a problem of the cyclical movement of production. He illustrates his case aptly with the experience of the United States where even a significant rate of economic growth proved incapable of pushing the unemployment rate below 5%. The danger of chronic unemployment arises as the result of the redistribution process of social labour, the latter being one of the unavoidable consequences of the technical revolution. The consequent necessity for anticipatory social planning Katzenstein sees as not being possible within the given price-profit mechanism.

In carrying the above analysis still one step further, Dr Katzenstein argues in his paper « Zur Frage des tendenziellen Falls der Profitrate », that at the present level of socialization, profit does not function either as impetus to or as regulator of production and productive forces to any significant degree. It is rather the state which enters into the process of accumulation and mobilises the capital. In spite of the fact that such intervention must produce social conflicts, the reason for the intervention is that without it the processes of the appropriation of profit, its distribution and realization, capital would not be able to function. The complex of causes that leads to the tendency of the rate of profit to fall, leads under present conditions, and increasingly so, towards the formulation of economic policy by the State.

The operation of this law lays bare a goal versus means conflict, in that the goal, the improved realization of capital, encounters in its realization the development of the productive forces. As a result, the realization of capital will in the end diminish. But a fall in the profit rate has never been a barrier to the development of the productive forces; the stimulus for economic development had always been the extra profit and thus the increase in the rate of profit for the individual capitals. Only in the subsequent extension of the process to the total social capital did the profit rate exhibit its tendency to fall. Dr Katzenstein now argues convincingly that, since variable capital may become immeasurably small relative to constant capital, we can observe already today branches of industry where variable capital can hardly be said to play a significant role, as *e.g.*, in the cigarette industry, though the productivity of this labour can still be increased. While the volume of labour can no longer be decreased, the volume of products can be increased and thus extra profit be earned. Or constant capital can be cheapened in a situation in which the organic composition of capital is already extraordinarily high. The fact, therefore, that the realization of capital is not concerned with its consequences over the long run, but with the short-run productivity of its productive forces, is not in conflict with the operation of the law of the tendency of the profit rate to fall, but the result of the internal conflict within the latter between its aim and its means.

This analysis of the operation of the law of the tendency of the rate of profit to fall is extremely significant, as it throws light upon a major dilemma of capitalist production. With the relative growth of constant capital and the absolute increase in its capacity to produce, the profit aim can be realized only through a short-run multiplication of sales. To provide for a proportionate increase in the socialization of production has been in the past a matter of long-run economic development. If the urgently needed short-run increase in sales cannot be made to materialize, the mass of profit realized must be diminished, and the cost of capital per unit of output will be increased. Any corresponding price increases could only react unfavorably upon sales and thus sharpen the dilemma. In Dr Katzenstein's formulation, this dilemma is represented in the inability of capital to afford a weakening of its cyclical position in the highly uncertain hope of realizing its output over the long run. It is a rule of thumb in investment decisions today, that the costs of any new improved technology have to be amortized within four years out of the additional profit realized. The introduction of the new technology is, therefore, dependent upon an extraordinary high saving on the cost of, *e.g.*, raw materials, *i.e.* circulating constant capital. Even in this case, the realization of the profit would still depend upon the acceleration of sales, *i.e.* its predictability, with a sufficient degree of certainty. In the absence of these conditions, the organic composition of capital would, at this point, present a limit to the further development of the productive forces. It is, thus, the extreme difficulty of capital to produce ultimately the necessary degree of socialization of production, that sets its own limits of development. Capital can utilize technical progress only to the degree that the potential for profit increase can be realized. Dr Katzenstein demonstrates by means of concrete examples, that the possibilities for technological advance have been used to about 25%. The problem of the introduction of technical progress appears here simultaneously with that of the destruction of existing capital. The relationship between both is shown to be mutually reinforcing.

A review can merely indicate the wealth of insight that is obtained through the method of analysis employed by Dr Katzenstein. In determining the degrees of socialization of production and laying bare its interdependencies it becomes possible to anticipate the problems generated by the present technical revolution. Thus, *e.g.*, the large-scale destruction of capital necessary from the profit point of view is meticulously planned in advance over long periods of time. While such plans are being kept secret by the corporations, it becomes mandatory for labour and its organizations to fight for access to such plans, instead of being suddenly presented with them. The effects of such industrial planning are particularly telling with regard to education. While change in technology and structure of production, in the interest of society would call for an educational planning that would make it possible for labour to adjust to these changes quickly and efficiently, the corporate « planning » trains the few and dismisses the many, and thereby demonstrates its utter lack of interest in the level of qualifications of the working people, as well as in the welfare of society as a whole. The most urgent problems of society remain unsolved.

It is rare that an economist provides the lay reader as well as the professional with so appropriate a methodology leading to eminently useful insights into currently ongoing economic and social processes.

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K. H. NIEBYL, *Studies in the Classical Theories of Money*, New York, Columbia University Press, 1946, Reprint New York, AMS Press, 1973, pp. XII-190.

When Engels and Marx were engaged in the study of old economic theories they occasionally remarked that they were wrong in theory but historically right. For instance, when the Mercantilists thought that surplus value and profit were primarily gained in foreign trade. Surplus value and profit originate in production, naturally, but if foreign trade consists above all in piracy and the exchange of non-equivalents as it did at the time of the Mercantilists, in the 17th century, it may also be said that surplus value and profit are so accrued.

One is reminded of such an approach to the analysis of economic theories by the procedure Niebyl adopts in discussing pre-classical and classical theories of money. For example, when he writes:

« We propose to understand money always by what it does, never by abstractions which on the surface seem to be similar for different periods. It should already be clear at this early stage of our inquiry that there is little connection between the function of money in mercantile society and the role of money under the conditions of industrial production » (p. 5).

Or when he says:

« The classical theory of inflation expressed thus merely the phenomenal coincidence of a specific historical period. It should be understood as a ' special case ' in doctrine formation created by and applicable to a special historical situation only. This, however, inferentially amounts to saying that the absence of the same conditions necessitates the formulation of correspondingly different concepts » (p. 79).

Today, this extremely fruitful approach to pre-classical and classical bourgeois economic theories, explaining them by objective economic conditions and by the desire to understand and master these conditions with the help of theories that have been consciously or unconsciously adapted to them, will be far more appreciated by economists and economic historians than when this book was first published 30 years ago. So the reprint seems fully justified.

There is, however, yet another reason why the study of this book is valuable today. Niebyl's exposition deals largely with problems of inflation which are presently indeed playing a substantial role in the discussion of monetary and economic problems in general. How our day and age differs from the time when Alfred Marshall wrote to James Bonar! « The only very important thing to be said about currency is that it is not nearly as important as it looks ».

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R. Romano and C. Vivanti (eds.), *Storia d'Italia*, Einaudi, Turin, vol. I (1972), pp. XXXVI, 1064; vol. III (1973), pp. 20, 1544; vol. V (1973) two parts, pp. XXXVI, 2172.

The great range and diversity of the events which have taken place on the Italian peninsula in the course of the centuries has always posed major obstacles for those who have attempted to write its history, especially after the Risorgimento had created a political and moral necessity for a 'national' history. Many of Italy's leading historians from Cesare Balbo to Benedetto Croce, or from Gioacchino Volpe to Luigi Salvatorelli, have devoted themselves to establishing the basis on which to reconstruct Italy's past and to define 'the unity of Italian history'. The editors of this *History of Italy*, however, have preferred to avoid this tradition. Rejecting the philosophical interpretations of the past they have opted for an explicitly empirical approach in which the history of Italy become simply the 'efforts of man in that country called Italy' (vol. I, p. XX). At first sight this might appear to be a useful expedient to sweep away the idealist, nationalist and historicist fantasies of the past, with a broom fashioned from the scientific method of the *Annales*, tempered in varying degrees by Marxism, were it not for the fact that the chosen periodization immediately draws this new *History* into the traditional debate. By excluding the entire ancient period and by starting in the Romano-barbarian era, Italy is defined as a modern nation which emerged from the later medieval melting-pot in a form which was essentially different from that of Roman Italy. This is, of course, the solution that is adopted in the majority of histories of Italy. First and foremost the editors have been faced by the necessity of providing some sort of unity which goes beyond simple geographical fact and they have tried to meet this by devoting the entire first volume to an attempt to discover the 'original characteristics of our past' by 'defining the phenomena which form the horizontal structure, the framework of our history' (vol. I, p. XXVI), without at the same time making any concession to the demon sociology. The explicative power of the metaphor 'horizontal' cannot, in these terms, be said to be great, but in order to clarify it recourse is made to that 'valid interpretative key' to the reality of the Italian past provided by Gramsci's concept of the 'moment of hegemony'. An essentially political and cultural criterion is then used on which to base a work which claims as one of its innovations — if one can talk of innovation now that the *Annales* school has been in existence for 50 years — the dethroning of the old-fashioned *political history* from its privileged position to give a more worthy place to the whole gamut of phenomena, from philosophy to sport and from art to cooking, in which the varied progress of the peninsula finds concrete expression, in order to 'reconstruct diachronically the typical features of our way of being Italians' (vol. I, p. XX).

If this was the objective no one could deny that the editor has employed a massive mobilization of resources to attain it. There are, it is said, more than 70 contributors, and among them the most prestigious names of Italian and non-Italian culture. The typographical presentation is authoritative and shows no trace of the current 'austerity'. There has been massive publicity organization at every level, with widespread support from conferences, public presentations, an Oxford 'imprimatur' and a series of articles with varying degrees of official prompting. Everything in fact that is needed in these days of the mass media to secure the general interest of the public and the cowed applause of the majority of reviewers, or rather of the 'critics' as the editors themselves (R. ROMANO and C. VIVANTI, *Da dove veniamo, dove andiamo,* in « Libri nuovi Einaudi », No. 14, January 1974) have referred to them in the jargon of the theatre, which is by no means as out of place as it might appear. Nonetheless from its very first appearance some bewilderment has been expressed, and this has now hardened into increasingly firm and emphatic refutations. The critique of the structure of the first volume has been both consistent and unequivocal. This is the volume which claims to provide that 'horizontal structure' on which the entire series is to be based, but which turns out to be simply a collection of chapters on themes which certainly are of importance in Italy's history, as in that of any country. But one must state, at the very least, that quite apart from the naturally very unequal standard of different contributions, each contributor sticks closely to his own specific subject without any direct reference to the others. Had Gramsci's 'interpretative key' in fact been seriously adopted, then surely the essay on 'The Forms of Power' should have provided a useful reference point for the other sections of the volume. But from the very start that chapter, which is extremely good, was entrusted to Giuseppe Galasso who is most certainly no Gramscian. As far as the others are concerned, the essays on agricultural history, legal history, linguistic history and art history were left to establish their individual framework independently, at times invoking the Gramscian concept of 'hegemony', at others simply ignoring it. As a result the concept in no way serves to connect the different essays. This makes an essay like R. Romano's, which advances passionate claims for thematic unity, seem all the more isolated. The essay claims to provide nothing less than 'a general model of the Italian economy' covering 'a span of 15 centuries' (vol. 1, p. 255) — which is to say that Alboinus and Agnelli can be treated together and explained by virtue of a single matrix. One may hazard the guess that the study of Italy's economic history will continue to follow the paths it has always taken and in much the same way, despite the appearance of this model, influenced by it for neither good nor bad. It also remains to be explained why it is that although even the history of the 'stage' is included among the 'original characteristics' that fundamental aggregate, the population, is excluded, and relegated to the rank of the so-called 'documents' alongside sport and fashion, despite everything that modern historians (and by no means demographic historians alone) have been teaching for 25 or 30 years now. It also remains to be explained why it is that, in the country which, *pace* Cattaneo, is the seat of the longest and least interrupted urban civilization in Europe, the Italian city is not included amongst the 'original characteristics'. And after De Sanctis, Carducci, Croce and, of course, Gramsci, why is it that the history of literature figures only as a 'document' of linguistic history?

Goaded and driven on by such expressions of bewilderment, which are numerous though scattered and intermittent, — the cultural industry is all on the other side — the editor tried to react by adopting a new 'official' stance (cf. *Libri nuovi Einaudi*, January 1974, *cit.*). Now we are informed (R. Romano - C. Vivanti, *cit.*) that while the work should still be placed within the Gramscian 'ideological' framework (which is something different from the 'interpretative key') its 'principles' are drawn from anthropology and 'ethnohistory' without belittling the contribution made by history itself — the 'principles' that is but not the 'methods' which are quite different. What with ideology, principles, methods, marxism, anthropology, ethnohistory, the net is thrown increasingly wide in the hope of grouping or forcing together Malinowski and Chabod, Kula and Cantimori, Zuidema and Luzzatto, in a single unit, the composition, although not the variety, of which is certainly disputable. In addition to these there are also, of course, Bloch, Febvre, Braudel and the other numerous and weighty names which are referred to incessantly to strengthen the ranks of the variegated army of claimed patrons and ideal guarantors of the undertaking. But this time we finally learn that 'the major historical and historiographical problem which lies at the heart of this work and which also guarantees its renegade but coveted « unity » is the relationship between the aged 'country' and the youthful 'nation' — a problem which 'every history of Italy which we possess' has 'always and carefully avoided' (*ibid.*). It would be amusing to discover the astute calculation which led historians as different as Luigi Salvatorelli, Giuliano Procacci and Paolo Rossi (not to mention Nino Valeri and that other *History* which he edited) to avoid so dramatic a question. But after proclaiming the drama the editors of the Einaudi *History* at once succeed in extricating themselves from it — for if they desire to write the history of the 'country' they do not intend on that account to sacrifice the history of the 'nation'. The two unite whenever the national history incorporates 'anything which appears with sufficiently great historical consistency to achieve a real Italian character — from language to the particular forms of Catholicism, from cooking to the city... to geography' (*ibid.*). But against what yardstick can this consistency be judged sufficient to endow one or other of these factors with the quality of being essentially Italian? Should one not take diffusion, as in the case of the language, among one specific social class throughout the whole country, or the presence of supposedly common religious traits, or the insuperably divergent urban structures of the different regions or the even greater differences between the countryside of the Po valley and the Appennine hills which owes as much to man's intervention as to geological and climatic factors? But above all, what is now left in this type of inquiry of the Gramscian concept of hegemony and the attempt to interpret Italy's history in terms of the relations between classes and ruling groups, and the increasingly broad and more conscious demands of the masses to share in the common problems of the national society?

But there is no need to go on. The foundations used to sustain this massive work are clearly too feeble to be subjected to further criticism. One must conclude that the poor outcome of a work in which such great talent has been employed is essentially the result of the misguided, muddled or downright non-existent criteria used by those responsible for the general coordination of the undertaking. And if, as the press suggests, they on their own declaration limited themselves to giving the contributors 'an indication, an opinion' and then left them 'absolutely free' to carry on in their own way, the outcome could not have been otherwise. In this way they hoped to achieve 'a mosaic in which independent assessments were harmonized' (C. Vivanti, *cit.*, in G. Ferrieri, *Le nuove frontiere della storiografia*, in *Libri nuovi Einaudi, cit.*). As far as the mosaic is concerned there can be little doubt, but as for the harmony which was left to the whim of the fates one can only have the greatest reservations.

The possibility, then, that this work might, as one would hope, tell the Italians 'who we are and where we are going' (Vol. 1, p. 1, *Romano,* C. Vivanti, *cit.*) is to be discounted. Nor do the commercial sales tell us anything — it is one thing to sell a book and quite another to make people read it. Were we dealing with a real effort to reconsider the history of Italy we would be in doubt whether a comparable failure might not document a broader crisis in our culture, but in this case we are in fact dealing only with a failure of organization. So we may read with pleasure many of the excellent pieces among those published in these volumes (among others, the chapter by C. Vivanti, one of the editors, is extremely good) while rejecting the attempt to settle extremely delicate historical and cultural problems in so brutally quantitative a fashion by simply accumulating writings on different aspects of Italy's progress and then lumping them together side by side without further thought. With regard to the attempt to present the studies contained in these volumes as bold explorations of fields hitherto unknown, one must also point out, taking them as a whole, that in nearly every case they draw on an excellent range of previous works. And also, a century after the Jacini Inquiry, the claim made in public that here Italy's agrarian structures and land registers are examined for the first time is simply not true. These are really huge errors which no polemical or advertising zeal can excuse. It is no easy task to produce a history of Italy; it requires the commitment of the complete professional skill and civil consciousness of whoever undertakes it — but more than a miscellany of studies on various topics is required to accomplish it.

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T. Sowell, *Say's Law*, Princeton, Princeton University Press, 1972, pp. 247.

Say's Law, the macroeconomic proposition that « [aggregate] supply creates its own demand », has meant many things to many men. (Say himself was far from consistent in his interpretation). For example: Was the proposition supposed to hold only at an equilibrium level of prices, or at any level whatever? Did it hold *ex ante* (of supply and demand schedules) or merely *ex post* (in the sense that the amount sold equals the amount bought)? Did it hold only in the long run, or in the short run as well? And was the range of commodities encompassed in the aggregates to include only current production, or did it likewise include inventories of goods and money already in existence?

In its most controversial and now generally discredited form, Say's Law was asserted to hold *ex ante*, at any price level, in both long and short periods, and for current production only. The main thrust of Professor Sowell's re-working of his Chicago doctoral dissertation, in the best Jacob Viner tradition of careful, detailed scholarship, is, however, less Say's Law itself than the opposition thereto. This opposition covers a century and a quarter from the law's initial promulgation to the Age of Keynes.

It appears, according to Sowell, that the numerous dissidents had between them successfully repealed Say's Law, or reduced it to tautological « reformations » before Lord Keynes was born. All Keynes and the Great Depression did was to make their opposition respectable. From the vantage point of hindsight, Sowell wonders how and why the old wreck of Say's Law stood up so long and so sturdily as it did. He finds two answers: the authority of a few Great Names, notably Ricardo and John Stuart Mill, and the sloppy disorganization of the criticisms.[1] Only a few of the critics were good economists, or knew each others' work, or resisted temptations to « think with their blood », i.e., ideologically, in favour of the poor (or of the landed aristocracy)! The fact, however, remains that every significant point of the Great Depression criticisms seems to have been made at least once at least 50 years earlier.

The greatest of the « economic-underworld » dissidents was Karl Marx. Sowell devotes an entire chapter to Marx, taking the opportunity to restate certain of his own unorthodox interpretations of the Marxian economic system.[2] His principal heresies are denials that Marx, as distinct from the bulk of his followers (1) held any strict labour theory of value except as a first approximation, (2) expected capitalism to collapse or stagnate from either the falling rate of profit or any tendency to underconsumption, or (3) inter-

[1] In dealing with demand and supply, for example, few of the critics distinguished consistently between *quantities* supplied (or demanded), movements along supply and demand *functions*, or *shifts* in these functions. (But the orthodox opposition was little, if any, better!).

preted periodic « crises » as dress rehearsals for the collapse of capitalism, or as caused by anything more than disproportion between sectoral supplies and demands.

Aside from Marx, the best-known of the Say's Law dissidents are probably Sismondi and Malthus prior to 1850, and John A. Hobson later on. Sowell tends to downgrade all these authors in favour of lesser lights with clearer partial insights, and likewise in favour of Knut Wicksell, whose writings contain many of the criticisms without openly breaking with Say's Law as such. Sowell also translates many of the vaguer notions of Sismondi and Malthus, in particular, into the conventional algebraic and diagrammatic apparatus of contemporary economics, thereby rendering the exposition much easier for the economists in his readership, but possibly somewhat more difficult for the intellectual historians.

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L. Stone, *Family and Fortune. Studies in Aristocratic Finance in the Sixteenth and Seventeenth centuries*. Oxford, Clarendon Press, 1973, pp. XVIII-315, £ 4.50.

Eight years after his fundamental *The Crisis of the Aristocracy 1558-1641* Lawrence Stone has now published a study of individual family fortunes, partly in response to criticisms of the absence from his earlier work of detailed studies of specific families. This attractively written and elegantly produced volume offers a lengthy account of the Cecils, earls of Salisbury, followed by shorter analyses of the fortunes of four other families: the Manners earls of Rutland, the Wriothesleys earls of Southampton, the Berkeleys Lords Berkeley clock and the Howards earls of Suffolk. Although the chronological boundaries extend beyond those of *The Crisis*, from the mid-15th to the mid-18th centuries, by far the greater part of Professor Stone's discussion related to the period of his earlier analysis. The most substantial and fruitful additions are for the decades immediately preceding and subsequent to the years of *The Crisis* — 1530-58 and 1640-60 — periods in which Professor Stone is able to demonstrate in detail the significance of the distribution of the monastic estates, on the one hand, and of the Civil War and political alignments, on the other, upon family economies.

A limiting feature of these studies is that they concentrate on the most wealthy and (often) politically important families, the equivalent of the French ' Les Grands '. Professor Stone justifies this in his introduction because of the greater completeness of the documentation, and because « the speed and the

[2] Sowell's original statement of his position is *Marx's* Capital *After 100 Years*, « Canadian Journal of Economics and Political Science » (February 1967). This reviewer disagrees with all these interpretations, but this is not the place for more than simple recording of our disagreement.

scale of the gains and losses are at their most dramatic and the story is therefore more interesting than it would be for more modest aristocratic families less exposed both to opportunity and temptation ». But all conclusions must be tempered by an awareness that profits from office played a significantly greater role for these than for lesser families.

A feature of the present book that will be appreciated by readers of Professor Stone's earlier work is the clarity with which he explains the problems and complexities of estimating family income, particularly the need to separate current income and expenditure from capital relating to dowries and jointures. This is an issue of central importance to the study of individual family finances, for — as Professor Stone rightly stresses — « the central concern of the individuals under study was — or was supposed to be — the prosperity and continuity of the family, and... the preservation of the family as an entity from generation to generation is very strictly determined by economic and biological considerations ». These factors (« sheer chance and individual personality as individual variables in their own right », as Professor Stone describes them) loom very large in the discussion of all five families. But a sample of five is obviously too small to allow the reader to judge whether Professor Stone is justified in denying that the cumulative effect of chance and personality could modify the « fundamental findings » of *The Crisis of the Aristocracy.*

The main conclusions that may be drawn from these interesting studies would seem to be, firstly, that the consolidation of landholdings remained the prime concern of families, both positively in the sense of investment in land of profits from other sources, and negatively through the disposal of land only as a desperate measure or as an act of exceptional irresponsibility. Secondly, for these major families the Court acted as a constant point of reference, even in such periods of notorious parsimony as Elizabeth's later years. Plentiful years, such as the 1530's and 1540's with the distribution of the monastic estates, or after James I's accession, offered opportunities of accumulating fortunes that could transform the rank and power of a family. Indeed, an element that emerges with striking force was the degree of corruption in the administration, accepted by all parties, but raised to new levels by James I's ministers Salisbury and Suffolk. As against this, the seeking of office could prove a disastrously expensive business, while its loss might mean (as in the case of Southampton) an inability to reduce living standards and life style to a level commensurate with reduced income. Thirdly, compared to income from land and profits of office, other sources of income — such as exploitation of mineral resources, privateering or investment in colonial companies — remained at best risky and usually ended in loss. The only really successful example given by Professor Stone is the Rievaulx ironworks exploited by the Manners of Rutland, and even in this instance of technical innovation and direct management production and income ceased to grow after 1600, possibly because of market limitations or shortage of wood supplies. Urban development in London offered greater long-term possibilities, and after the Restoration represented probably one third of Southampton's income. But in the 16th century urban profits were by no means so sure, and throughout the period under study the element of chance — in the sense of the location of property in London — remained crucial.

In the continued absence of detailed studies of this nature for Continental countries, it remains difficult to make comparisons. It is clear that the wealth of an aristocratic family constituted the essential base for political influence and power, and that the cost of launching and maintaining a career impinged heavily, and sometimes dangerously, on family resources. A noticeably distinctive feature of such successful families as the Cecils and Manners is the efficiency of their estate management by the later 16th century. Even though they took no part in stimulating technical change in agriculture, their concern to consolidate their estates, to carry out some enclosing, and above all to limit the length of tenures and modernise their systems of accounting, would appear to be at least half a century in advance of their Continental counterparts. On the debit side, the cost of building — albeit the examples of Hatfield and Audley End represent extremes of grandeur — always appeared to endanger the economic stability of a family: Robert Cecil's building sprees in the early 17th century represented over 5 years' income; in Turin in the later 17th and early 18th century, palace building cost some four to six years' income.

Two final points. It would be useful to investigate to what extent urban development profits in 17th century London reflected the more rapid growth of that capital city compared to Paris. More generally, it may be that the centralised nature of English administration necessitated concentration on the Court for an ambitious English noble family, whereas on the Continent local administration (not merely in the form of venality of office) offered alternatively profitable opportunities, even if on a smaller scale. But too little is still known about the economy of Continental aristocratic families, even in the 18th century (let alone in earlier periods where Profesor Goldthwaite's study stands in almost splendid isolation), to do more than hazard suggestions.

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A. C. Sutton, *Western Technology and Soviet Economic Development - 1945 to 1965*, Hoover Institution Press, Stanford University, Stanford, California, 1973, pp. 482.

« All along, the survival of the Soviet Union has been in the hands of Western governments. History will record whether they made the correct judgment » (p. 421).

« Every new Soviet purchase of a major Western technology is *pari passu* evidence for a central lesson of this study: Soviet central planning is the Soviet Achilles' heel » (p. 423).

After 423 pages of detailed « documentation » and « objective » analysis Sutton arrives at the above quoted simplistic conclusions.

Throughout the entire book it is evident that the biases, premises, and assumptions of the author represent a cold war capitalist mentality which conveniently overlooks critical aspects of this phenomenon called « technology » and furthermore it neglects to consider the impact of the geo-political aspects of a nation as large in geographic area as the U.S.S.R. — whose contiguous land mass covers 1/6th of the earth's surface and on which the sun literally never sets. The implications of this fact alone cannot be ignored and cannot be detached from technological innovation. Also, to ignore the traumas the Soviet people have suffered from external invaders deletes a factor which must be considered in any analysis of the Soviet Union, including especially an analysis of technological development in that country. For example, in World War II alone, it has been well established that the Soviet Union lost upwards to *30 million people — dead* — as a direct result of that war. If one refers to history one cannot ignore the millions of people who died during the Revolution and the Civil War and its aftermath in the Soviet Union. How much intellectual and creative human talent and energy was destroyed through this process — energy and talent that could have contributed to « technological » development in the Soviet Union? There are no data in Sutton's book showing a measure of this loss or an interpretation of the implications of such data.

Considering the « documentation » included in the book, and it is substantial in quantity to the point where it is overburdened with charts, tables, numbers, et cetera, Sutton seems to be playing a « numbers game » which becomes tedious since all his « numbers » seem to be selected in a fashion which is designed to lead to the same conclusions: that the creative people in the Soviet Union are not capable of innovative technology because of « ...lack of personal profit incentive and disciplinary market forces... » (p. 65) and that because of the « inability hypothesis » (p. 423) the « ...Soviet central planning has not fostered an engineering capability to develop modern technologies from scratch... » (p. 423). The contentions projected in the above quotations again focus on the author's biases and lack of appreciation that it is an extremely rare occurrence when any modern technology is developed from scratch!

Grave reservations must be placed on much of the « documentation » used (and their interpretation) by Sutton, especially when material from the « House Un-American Activities Committee » (HUAC) is cited (p. 210). To use HUAC as a source for justifying some of the author's conclusions is ludicrous, knowing the record of HUAC which includes distortions, falsifications, character assasinations, the use of paid informants and infiltrators, et cetera, to pursue its « Cold War » ends. The dichotomy raised by Sutton in conceding the innovative technological ability of the Soviet Union in the military field, while stressing its supposed « inability » in engineering in the civilian domain, is ineptly explained and can be viewed as a « tactic » designed to promote continued heavy military expenditures by the « West » to meet the implied « threat » of Soviet innovative weapons technology. These huge expenditures on weapons and weapons development have important implications for « Western » economies which are largely based on the profit motive. This dichotomy introduces confusion in Sutton's conclusions: if central planning is the « Achilles' heel » of the Soviet Union in the civilian sector of the Soviet economy why does not central planning have the same effect on one of the most centrally controlled and planned of all areas of human activity — which is the military? To say, as Sutton does, that this is due to the fact that « military technology developed toward a specific objective can be pretested to determine whether it fulfils its objective » (p. 361) does not even begin to explain the reasons for the existence and application of creative-innovative technology in this area of endeavour in the Soviet Union. Is it just possible that the Soviet Union actually does have indigenous creative engineering and scientific talent perfectly capable of innovative technology at any level or in any domain that any modern technological society possesses? And is it not possible that the Soviet Union (unfortunately) is capable of developing perverted priorities equal to the « West »?

To categorize technology in nationalistic or geographical terms in today's world is to imply that innovative technology develops in a vacuum — isolated from other influences. Obviously, the opposite is true. A good engineer, in solving any technological problem, always looks to what others have done in order to learn from their experiences, both positive and negative, and in the process collectively develops technology in a creative manner, instead of continually starting from scratch. To utilize the developments of others and to apply them to specific goals and conditions requires a basic knowledge of engineering art and science. Again — this cannot be accomplished in a vacuum. To reiterate, technological innovation is largely a *cumulative process* which includes using the information and data and devices developed in the past and adopting, and inventing on the basis of past and present experiences. In other words, technological development is an *international phenomenon* and it requires ideas from many sources — and ideas are not restricted to any one political or economic entity or philosophy.

Categorizing technology into « Western » or « Eastern » domains is an artificial device which ignores the history of science and technology and their development in this world.

Sutton has written a book which, in effect, develops a self-fulfilling prophecy.

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J. B. Williams, *British Commercial Policy and Trade Expansion*, Oxford, Clarendon Press, 1972, pp. X + 514.

The manuscript for this book had received its final revision shortly before Judith Blow Williams died in a car crash in 1956. Due to the persistence of her friends and the support of Ralph Davis at Leicester University, it has now been published together with an excellent bibliographical chapter by David Williams which brings together material published since the book was written.

The book is concerned with the role of commercial diplomacy in bringing about the expansion of British trade during the crucial century when the British economy surged forward on the basis of the commercial and technological triumphs of the industrial revolution. It begins with the role of pioneering and exploration in advancing trade. The material here is purely descriptive of activities in colonies, frontiers, island trade centres, newly discovered lands and is essentially a background to the central section which follows. There are interesting passages on remote areas such as Muscat, the Yemen, Abyssinia, but the longer sections on the West Indies, North America and Australia offer little that is really new.

The core of the book deals with British commercial diplomacy in Europe, the Americas and the Far East. We are shown the merchant at work searching for new materials and other products to import, pushing into new markets for his exports, though the connection between all this and the vast economic changes going on in Britain at the time is left to the imagination. Governments intervened to help him out when he got into difficulties and this is what Miss Williams recorded in most detail. As time went on particular interventions of this kind became less important than the drawing up of treaties on the most favoured nation principle. It is detailed work, conscientiously done, and for anyone interested in the details of negotiations over tariffs, the material is indispensable.

On the broader plane one must be careful not to judge the book by the standard of present day analytical techniques. Nevertheless it has to be admitted that it was out of date in that regard even in 1956. There are no tables, no attempt is made to indicate the magnitude and relative importance of trade flows and above all there is no analysis of the degree and timing of changes in the direction of such flows. Consequently, although we are told a lot about commercial diplomacy, we have no means of assessing its effectiveness. It is particularly unfortunate that there is no study of the interaction of trade flows through multilateral patterns, for these were vital to much of the trade under discussion. New trade in the Far East, for example, was almost entirely dependent on the establishment of entrepots and other mechanisms for stimulating just these kinds of patterns and in this book all that is lost. Furthermore we get no indication of the extent to which trade emerged from economic changes within the countries concerned themselves, rather than from the operations of outsiders.

That part of the third section on war and smuggling which deals with the Revolutionary and Napoleonic wars bears no comparison with Crouzet's great work. Even so the book contains masses of detail drawn from a wide range of sources in many languages. For this alone it will be valuable for a long time to come. It is old fashioned but it also brings with it the old virtues of thoroughness and integrity.

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