

---

## REVIEWS OF BOOKS

---

S. BELU, S. GOLDENBERG, *Die Epoche der grossen geografischen Entdeckungen*, Bucharest, Wissenschaftlicher Verlag, 1971, 440 pages, 5 maps.

This book by the two Romanian historians appeared in Bucharest in 1971 and represents the result of previous studies by the same authors, and in particular by Professor Samuil Goldenberg, on the history of the great geographical discoveries and their economic consequences, especially for European trade. The work is well documented, with an extensive select bibliography that is extremely useful both to the general reader and to the specialist; there are also five maps and numerous illustrations. In addition the compelling but strictly scientific style of the work is a further merit of the study.

The first two chapters deal with the socio-economic and political antecedents of the great geographical discoveries and describe the state of the development of medieval cosmography, science and navigational techniques in the early Middle Ages (pp. 9-42). The third chapter contains information on the precursors of this period, from the legacy of antiquity to the courageous men who took part in the Crusades and in the voyages of the 13th and early 14th centuries (pp. 43-78).

The following chapters of the work present the many facets of the history of the great geographical discoveries and the reasons and impulses leading up to them, to the birth of the Portuguese and Spanish colonial empires, and the consequences of these discoveries in various fields of history, particularly that of the European communities.

Professors Goldenberg and Belu's book is not a study of the history of geography, concerned with the geographical discoveries in themselves.

Although it examines these discoveries from the geographical point of view the work is presented to the specialist and the general reader as a historical work in which the stages in the development of the European communities, their geographical, astronomical, technical and navigational knowledge is examined, which taken as a whole facilitated the great ocean voyages. An important place in the work is occupied by the examination of the economic causes, the great trade routes and the socio-economic needs that prompted the efforts of brave men to discover new territories, as well as new sea routes for trade in areas already known. In our opinion the reader of this review will be most interested in the economic causes and effects of the age of the great geographical discoveries as they appear in this study.

The beginning of European expansion by sea and by ocean is considered to be the beginning of the predominance of European civilisation. The old continent established contact with the new territories, many European countries became 'workshops of the universe', while the territories that had been discovered became suppliers of raw materials to European industry (p. 79).

Some of the territories discovered by the Portuguese became bases for the cultivation of cereals and sugar cane. Consequently, after the discovery of the new world, the Spanish and Portuguese introduced the cultivation of sugar cane on to the new continent. Another important economic motive was the exploration of these territories in search of gold, at a time when there was a shortage of gold on the European market, mainly because some of this was returning to the East. For the men who set out for these new territories, which were for the most part unexplored, the prospect of finding gold was the greatest attraction.

Examining the causes leading to the great discoveries, the authors do not agree that the Ottoman conquests in the Mediterranean and the fall of Constantinople (1453) interrupted one of the most important trade routes between Europe and the East and so necessitated explorations along the African coast. Trade with the East continued and the Portuguese founded their Eastern colonial empire towards the end of the 15th century and the beginning of the 16th when the Turks conquered Eastern Anatolia (Syria and Egypt) and came into conflict with the Portuguese in the region of the Red Sea.

One part of the study deals with the promoters of the great expeditions, those who planned them for some decades and then carried them out. Here the civilisations found in pre-Columbian America by the Europeans are also studied (chapter 10, pp. 253-301). From this chapter on the authors dwell on the consequences of this epic voyage. The great geographical discoveries are seen as events of the type that have contributed to the 'changing of the structures of the world' in the various spheres of human activity, whether economic, social, political or juridical. The scope of European knowledge

was increased so the predominance of the civilisation it brought was guaranteed. Two colonial empires emerged, one Portuguese, one Spanish, traces of which are still visible today. Changes can be seen in the economic, as well as in the more strictly commercial, structure of the European states, because the new discoveries largely displaced the great trade centres and sea routes to regions that claimed need of them. They moved towards the Atlantic Ocean and engaged Spain, Portugal, England, the Low Countries and France in trade rivalry. Domestic sea trade became oceanic, while Europe's trading capital was in full expansion. The authors emphasise that trade now assumed global proportions, due to the reciprocal movement between Europe and America. Another consequence of the great geographical discoveries, which was in turn to have other consequences, as the price 'revolution' brought about by gold and the silver which was now mined in America and arrived in great quantities on the European market.

As far as structures and social relations were concerned the industrial middle class, and in particular that section of it that was based on commercial capital, was strengthened, which was a positive factor in the development of European society: the middle class was to be the force to break down the outdated feudal system.

In political terms there were changes within some of the European states (Spain, Portugal, England etc.) that became great powers and evolved towards a form of absolute monarchy, while at a European level the struggles for continental and colonial hegemony became more frequent. A modern spirit broke out in European society and contributed to the historical developments *à long terme* of the following centuries. In addition the authors study the innovations which the arrival of the European on the new continent brought about in the life of the Indians, in the socio-economic, political and institutional changes in America through contact with European society. Professors Goldenberg and Belu's work is a triumph of Romanian historiography and deals with a set of problems of European, and even wider, interest, that is of great significance and importance for human society.

NICOLAE EDROIU  
University of Cluj

G. FELLONI, *Gli investimenti finanziari genovesi in Europa tra il Seicento e la Restaurazione*, Università degli Studi di Genova, Istituto di Storia Economica, Milano, Giuffrè, 1971, 705-XXVII pages.

On July 1, 1739, in one of his letters to his family, Charles de Brosses wrote from Genoa: « Posturing as intellectuals we attempted to find some men of letters: nothing doing. This is not the right place; the merchants

are not amused by frivolities and bills of exchange are the only form of writing they know. These are used more here than anywhere else in the world and a public bank guarantee exists for them which is said to amount to 300 millions in solid cash. This strikes me as somewhat exaggerated ».

It has taken more than two centuries to show that this estimate, which caused the brilliant French magistrate and man of letters some perplexity, was not at all exaggerated. Felloni's foremost merit, in fact, is that he has succeeded in placing in perspective the size of Genoese private financial investments in the 18th century.

In 1725 the Genoese had 275.5 million bank lire invested in foreign securities and loans and by 1785 this figure had risen to 342.5 millions. These imposing and expanding assets show clearly that the city's financial power, far from being exhausted after the « Genoese banking cycle » of 1550-1630 described by Braudel, actually gained strength in the 18th century.

This topic is dealt with exhaustively in Felloni's book, the contents of which, based on very rich documentation, are presented with strict methodology. A total of 2,400 ledgers, account books and notaries' files in some 80 « fondi » preserved in 3 foreign and 20 Italian archives have been minutely examined and 260 historical publications and printed sources have been used — a truly impressive work of research.

The author first examines a sample collection of estates in order to ascertain, on the basis of the balance-sheets of concerns belonging to the nobility and the bourgeoisie and of private and public foundations (a total of 32 estates were analyzed and a general assessment was made of the prevailing patrimonial structure of the ecclesiastic bodies), what was the weight of financial investments in relation to other forms of capital immobilization.

Here the statistical research also provides an insight of the customs and behaviour of the rich aristocracy in a city once devoted to trade: Girolamo Fieschi had 290 kilograms of silver, while Marcello Durazzo had one ton of fine silver and 43 kilograms of fine gold in jewels; the 13 companies belonging to the nobility, which had made 29 million lire of financial investments and invested more than one million lire in jewels and precious metals and a similar amount in furniture, ornaments and clothing, had only 25,000 lire invested in shipping.

The examination of the bourgeois companies also reveals the slight importance of trading and industrial activities in the Republic, and so provides further confirmation of the widespread propensity of the upper classes to hoard, to have liquid assets for occasional speculations, but to invest above all in securities. This attitude explains the permanence in Genoa of the absolute predominance of an aristocratic oligarchy and the inadequate progress made by the middle classes in the century of Enlightenment.

Having established that investments in securities were the pivot around

which the economic activity of the sample companies examined revolved, Felloni analyses the two principal forms in which such investments were made: public debt securities and loans « for the use of Genoa ».

From the city the author passes to Europe. He does not confine himself to describing the destination of the investments in securities, but accurately examines, at a continental level, the mechanism of the formation of the demand for capital, reviewing the origins, the duration, the rate of interest and the repayment terms of 591 loans issued in Europe between 1686 and 1810 in which Genoese financiers were involved. He also reconstructs the vicissitudes of the public debts of 12 states, from the Arch-Duchy of Austria to the Kingdom of Spain, from the Kingdom of Great Britain to that of France.

As regards in particular the Italian states (the Republics of Venice and of Genoa, Grand-Duchy of Tuscany, Legation of Bologna, Austrian Lombardy), interesting information is provided as to how the « reforms » took effect in re-ordering public finance, unifying the various loans in a single « monte » with the consequence that interest rates, maturities and appropriations were also unified. The 350 pages into which this part of the study is condensed will be found of valuable assistance by students of public and private financial events in 18th century Europe. The behaviour of a group of financiers that proved even more powerful than the « Protestant bank » of Geneva, provides a first-class topic for analysis. It is interesting also for researchers working on contemporary economic conditions which differ from those of Genoa to observe the « fundamental turning-point » reached around 1765: a change of trend, documented uninterruptedly between 1725 and 1764, led investors to get rid of their securities and increase their investments in foreign loans. It became increasingly difficult to obtain interest, and an atmosphere of distrust began to surround public debts: the wars of succession and the Seven-Years war made their effects felt. Money changed route and flowed to medium-term loans « for the use of Genoa », for which particular care was taken in the pertinent deeds (always drawn up in Genoa) to state the real guarantees clearly and to control interest rates and procedures for the repayment of capital according to a fixed monetary yardstick.

This was perhaps the last important attempt made by a class of financial operators, with several centuries of experience in the struggle to protect themselves by every means against their powerful creditors, to try and adjust their strategy to the changed conditions of the economic environment. In a certain sense, perhaps, we are here in the presence of the « defensive » technical evolutions already adopted by the Genoese bankers at the time of Charles V and his immediate successors to survive the bankruptcies that caused the ruin of lenders in Southern Germany.

In the chapter devoted to the « technical aspects » and « human protagonists » of the lending operations, attention is drawn to all the devices and

mechanisms used in the attempt to provide an adequate safety margin for investments. But all this was not enough: the upheaval caused by the French revolutionary legislation, Napoleon's European measures and the decisions taken by many European governments after the Vienna Congress were far more deleterious than the Spanish insolvencies. In fact Genoese capital losses on investments in securities alone exceeded 200 million bank lire, without counting the failure to pay interest for some twenty years. The abundance of financial resources in the 18th century gave place to the misery of the early nineteenth century, which was to condition all the vicissitudes of Liguria's industrial development.

Felloni's book, which is complete as regards the subject it deals with, has also the further merit of posing a number of stimulating queries.

Let us mention only one of them. From where did the growing availability of resources that fed Genoese investments in securities in the 18th century come? Was it from saving on incomes and especially on incomes from securities, inside a single banking cycle which, starting at the beginning of the 16th century, lasted for three hundred years? Or were part of these resources provided by the liquidation of other types of investments the Genoese had made in the 17th century in Southern Italy, Spain and elsewhere?

GIORGIO DORIA  
University of Genoa

R. M. HARTWELL, *The Industrial Revolution and Economic Growth*, London, Methuen (University Paperback), 1971, pp. XXI-423, £ 2.—

Over sixty years ago Clapham described the history of the Industrial Revolution as a 'thrice squeezed orange' in which there still remained an astonishing amount of juice. In recent years the squeezing process has certainly been intensified but, in Dr. Hartwell's view, the juice still remains stubbornly unforthcoming. In the present volume the author attempts to discover — or rather illustrate — why this is so, and in so doing provides one of the most provocative and important reviews of contemporary British economic historiography. Consisting of seventeen essays and articles, six of which hitherto unpublished, the volume is not intended as either an exhaustive or complete study of the British Industrial Revolution but as an exposition of the author's approach in anticipation of a more complete study which he intends to write. It contains rather an 'agenda for research' in which, by means of extensive analysis of traditional and current economic historiography and of methodological proposition, Dr. Hartwell argues that the time has

come to pause, to examine the bases on which economic history research has been conducted, to discover why this research has been so inconclusive in its findings, and to formulate new and more fruitful analytical directives.

Dr. Hartwell claims that his approach is 'sufficiently different and novel to interest economic historians' and it is important to understand precisely the nature of this novelty. The thesis put forward is that the Industrial Revolution has hitherto lacked definition, that the need for such a definition is fundamental, and that it can be provided only from economic theory. On this basis the Industrial Revolution is defined as a phenomenon — or the phenomenon — of economic growth; as such it can be analysed rewardingly solely in terms of an economic history informed by economic theory. Dr. Hartwell does not claim uniqueness for such a methodological formulation and throughout the influence of current American economic history writing is evident. It is also clear that the emphasis on the need for quantitative factor analysis, in view of the work of such contemporary economic historians as Deane and Cole, Habbakuk, Flinn, Mathias etc., is now less novel than when first propagated by the author; a fact which permits him often to adopt the unusual dual stance of innovator and voice of orthodoxy. What is novel however is the formulation which derives from this initial definition — that is of the Industrial Revolution as a phenomenon involving totally both economic and social structure and one which can only be analysed quantitatively and in terms of that totality, of the aggregate of complex and interdependent variables of growth. It is this duality of emphasis, the combination of quantitative methodology with a global framework of application, that contains the novelty of contribution in the author's approach and which allows him to investigate meaningfully fields relatively neglected by other economic historians, in particular the so-called 'Non-economic Inputs' — the growth contributions of investment in human capital, of organization, or of increased productivity in the tertiary and service sectors. There is no paradox in this, for only on the basis of a systematic quantitative methodology is it possible to attempt to assess the growth contributions of such non-economic variables. But while the author's investigations in this field are of the greatest interest and provide one of the most important constructive contributions of the present studies, it is also true that they are as yet preliminary and it is not always clear, for example, that the sources for a more precise quantification of problems such as general education in the XVIIIth century exist.

The most distinctive aspect of Dr. Hartwell's approach however is the extensive use of historiographic criticism both in forming methodological propositions and in theoretical analysis. The author convincingly argues in favour of these exercises on the grounds that 'thinking is research' just as much as the spoliation of archives. By and large Dr. Hartwell's critical thinking with regard to the work of contemporaries and predecessors is highly constructive and it is no incidental value of the present collection of

essays that they contain the most complete critical appraisal available of the historiography of the Industrial Revolution from Toynbee to the present; this also makes this volume a valuable and stimulating introduction to the central problems of the subject in addition to its value to the researcher. The criticism often levelled at the author, that in many respects he fails to provide clearer conclusions where he has criticized the failure of others, is largely irrelevant; the value of his critique lies not in whether he succeeds or otherwise in providing conclusions but rather in the fundamental and repeated insistence on the need for broader and more theoretical bases for economic history research and its application to broader and more inclusive fields. In this respect the place given to historiographic analysis is highly significant of Dr. Hartwell's own method, for it illustrates how closely he is dependent, if critically, on the corpus of traditional history, and shows clearly his definition of 'new economic history'. It is not in this case a random jump into the world of hypothetical contrafactual alternatives, but much more an attempt to revise and reorganize traditional studies on the basis of more specialized quantitative-functional techniques. In his interesting chapter on the relationships between economics and history he shows keen awareness of the danger of sacrificing the historical sense for the sake of quantification. It was after all the historians who identified the various factors and variables of growth, even if they then did not have much idea what to do with them, while the economists were prone to unrealistic over-simplifications; now it is the economists who must help the historians out of the *impasse*, but this they can do only after themselves acquiring that historical 'feel'.

Historiographical criticism inevitably entails throwing down a number of gauntlets and in this Dr. Hartwell is not renowned for timidity. The essays in the present collection are divided into three sections — Methodology and Background; Causes and Process; Social and Economic Consequences. The distinctions between the first two are often unclear, mainly because methodological criticism and proposition and theoretical analysis are closely interwoven in the author's own method. In general terms however these sections follow the three focal points of the author's thesis — the definition of the Industrial Revolution in terms of economic growth, of total economic and structural transformation; the insistence on the 'discontinuity' embodied in the process of growth in reaction to a very long term and generalized process of economic change (in this sense the term 'take-off' is often employed, although the stage theory of Rostow and the German Historical School is explicitly refuted); the quantitative analysis of the variables of growth — capital formation, population increase, technology, organization and human capital — with the insistence on their analysis in the framework of a 'complex relationship interacting to cause aggregate change'; and finally the examination of the social consequences of industrialization and economic growth, in particular the effect on the conditions of the working classes.

Put forward in the context of a historiographic critique this thesis lays three fundamental charges against the traditional studies of the period and the process. In the first place the failure to establish an economic definition of the Industrial Revolution has caused the neglect of fundamental problems, in particular the problem of *why* the Revolution occurred. This neglect is typified by the lack of general studies — Dr. Hartwell will admit only three, those of Toynbee, Mantoux and Ashton — and by a preference for sectarian studies of individual industries or factors, which have the general tendency of ‘explaining away’ the problem of how growth occurred by assuming the influence of exogenous stimuli in particular sectors; assuming thereby the general causes of growth. Part and parcel of this deficiency is the similar preference for ‘evolutionist’ causal explanations in which, the author argues, the crucial distinction between economic change — a very long term process evident and generalized throughout Europe from the XIIIth century onwards — and economic growth, which was the phenomenon exemplified for the first time in the Industrial Revolution, are disguised by historically unhelpful (or at least economically unexplanatory) concepts such as the rise of capitalism. The evolutionist and sectarian theories often go together, and Dr. Hartwell argues convincingly against both in favour of a process of balanced growth, which alone provides an explanation of the responsiveness of the economy to economic growth, to technological diffusion and innovation. This conclusion is supported by an important discussion (Ch. 9) of the theory of growth strategy in which an effective attack is made on the influence in favour of unbalanced growth exercised by Rostow, Innis and Schumpeter.

It is in the debate over the social consequences of the Industrial Revolution — Dr. Hartwell’s contributions to which are here reproduced at length but not unfortunately the replies — that the element of controversy becomes fiercest, most clear-cut, and probably also most limiting. Without doubt the debate is fundamental. Against the ‘pessimists’ position that living standards and the quality of life among the poor deteriorated as a result of industrialization — a stance that can be traced back to Toynbee and the Hammonds but which finds its clearest and most forceful contemporary champions in E. J. Hobsbawm and E. P. Thompson — Dr. Hartwell argues for at least a minimal relative improvement in living standards from the earliest period of growth. Again his basic position is theoretical; economic growth is coterminous with increased *per capita* income and in this case, with the absence of distribution trends prejudicial to the poor and with the probability of a low savings to capital input ratio in the process of capital formation, it is illogical to assume a deterioration. Economic growth in underdeveloped countries is after all stimulated to improve, not aggravate, social conditions. On the basis of extensive research on consumption indices, savings statistics, and government investments in welfare — detailed methodological criticism of which can be found in Dr. Hobsbawm’s various rejoinders — the author establishes

a minor but tangible improvement from the beginning of the century, increasing after 1815 and then again very rapidly after 1840. In its second phase the debate has shifted — mainly due to the directives of E. P. Thompson — to concentrate more on the quality of post-industrial life, the human sacrifices of the industrial regime, of squalid urbanization and of alienation. On such a plane the discussion is clearly much less conclusive with both sides lacking adequate methodological means for analysis. Dr. Hartwell, while careful to point out that conditions were not good if still relatively better, stresses the humanitarian aspects of early XIXth century government and factory masters in response to the economic logic of the need to invest in human capital; but here the obstacles to change and to humanitarian intervention — which could also summon economic logic to their assistance, as the author's novel essay on British factory children and American slaves suggests — are almost certainly underestimated. Conducted almost exclusively in terms of value judgments, the debate is open to a degree of polemic which almost certainly mars its usefulness. On balance, however, Dr. Hartwell's contribution has the value of questioning relentlessly accepted and modish dogma. In particular his attack on the myth of the ideal bucolic society of the XVIIIth century — in which is included a blissful quotation from the Webbs illustrating succinctly the historical idiocies to which such a myth can lead (pp. 64-5) — and his description and research into pre-industrial agrarian life, providing a picture much closer to Carlo Levi's Lucania than to Hardy's Wessex, is both credible and gives a much more realistic framework within which to examine the changes effected by industrialization. It would be interesting to know, however, from when the popular diffusion of the pastoral myth can be dated — authentic history after all is not always the most influential.

It would be wrong however to stress too predominantly the element of polemic and pure historiographic criticism in these essays at the expense of Dr. Hartwell's more independent research contributions. The importance of the studies of the contribution of the service sector to growth has been mentioned. In what are preliminary analyses the author examines the problem of the extension and advancement of general education in the XVIIIth century, the particular structure and flexibility of the English legal system and Common Law, and the development of entrepreneurial and managerial techniques. As far as is practical — and lack of research knowledge in these fields at present creates severe limitations — Dr. Hartwell attempts a quantitative-functional assessment of the influence of the various components of the tertiary sector on the process of general growth, concluding — again in a preliminary way — that there was a positive relationship, that productivity in the service sector was increasing at a rate probably faster than that in either industry or agriculture, and that this growth was essential for the Industrial Revolution. These essays certainly leave as many questions open as they answer, in particular any conclusions in these fields need to be set

in the type of comparative perspective explored, for example, by F. Crouzet and P. Mathias. Dr. Hartwell's initial forays indicate clearly, however, the importance of these sectors and the validity of his methodological equipment, even though the state of present research does not allow its proper application. Precise questions of cause and contribution may be set, and if an equally precise answer cannot, and could not, be found, at least vague generalizations can be given some quantitative expression or else be dispensed with.

To conclude it is not difficult to assert that these essays on the Industrial Revolution are among the most important that have appeared, and from them the author emerges clearly as one of the most original, lively and iconoclastic of British economic historians. It is a book to recommend both to the student and the researcher — and also to those in fields far removed from the Industrial Revolution — for the author's restless and provocative independence of judgment coupled with a well-defined methodology, and for his ability to study the particular always in terms of, and in the framework of, the general. It is unfortunate that in the present collection this is not always matched by the presentation; as in any collection of this nature repetition is inevitable but the length of the book does make this something of a problem for the reader. It is often difficult to extract with certainty the author's first or last word on a given topic and at times the reader is borne down by sheer weight of attrition. In this case however even that is a small price to pay, but one looks forward all the more to Dr. Hartwell's projected 'large and systematic book'.

J. A. DAVIS

St. Antony's College, Oxford

M. LACHIVER, *La population de Meulan du XVII<sup>e</sup> au XIX<sup>e</sup> siècle (vers 1600-1870). Étude de démographie historique*, preface by P. Goubert, Paris, S.E.V.P.E.N., 1971, pp. 339.

Since its definition some fifteen years ago in a precious handbook<sup>1</sup> which has since been repeatedly revised and reedited Henry's methodology has provided a durable guide for demographic research in France. It provides a method which is both disciplined and exacting and which is applicable to both the positive and the negative aspects of the only direct documentation for the pre-statistical era — parish registers — the documents that is of the age prior

<sup>1</sup> LOUIS HENRY, *Des registres paroissiaux à l'histoire de la population; manuel de dépouillement et d'exploitation de l'état-civil ancien*, Paris, Institut National d'Etudes Démographiques (I.N.E.D.) 1956; reedited under the title *Nouveau manuel...* in collaboration with Michel Fleury. The principal village monographs other than that of P. Goubert on the Beauvaisis (*Beauvais et le Beauvaisis de 1600 à 1730*, Paris, S.E.V.P.E.N., 1958) have been published in the series of the I.N.E.D.

to the introduction of the '*Etat-civil*' during the Revolution. The exhaustive and critical extrapolation from these registers (of births, marriages, and burials) of schematic indices provides the means for the 'reconstruction of the families'; on the index of the individual families are noted indications of kin — date and place of birth, filiation, profession where possible, marriage date together with that of widowhood and possible remarriage — and of off-spring — the dates of birth and death. From an initial examination then the ages of marriage and death, of the child-bearing age of the mothers, the generation intervals, the length of the union, the descendants of each family group, the interval between widowhood and remarriage can be established. The subsequent elaboration of these statistical calculations permits the estimation of various rates (fertility, legitimate or otherwise, according to the mother's age, infantile and juvenile mortality, etc.) and of long term trends which accompanied social evolution; that is the estimation of a certain demographic structure and its transformations.

M. Lachiver's study then forms part of an illustrious tradition which is represented most notably by the study of Crulai (E. Gautier and L. Henry), by that of the parishes of Beauvaisis (P. Goubert), by a group of village monographs dealing in particular with Normandy, the West, the Paris region, and Aquitaine. But it also goes beyond the limitations of these studies. In chronological terms the extension of the study up to 1870 gives both a more complete picture and greatly reduces the number of 'incompleted', and so unserviceable, indices. In geographic terms although the town of Meulan was small, having perhaps some 1500 to 2000 inhabitants around 1600, hardly more than 1000 at the death of Louis XIV, and again some 2000 under the Revolution and under Napoleon III, it provides a statistical basis infinitely wider — allowing the reconstruction of at least a thousand families — than is the case with rural parishes which rarely contained more than 500 souls and in which demographic rates have to be deduced from too restricted a number of families (in the case of Thézels-Saint Sernin studied by P. Valmary the group is as small as 100, of which the exact age of the wife was only known for 42). The examination of the 32 parishes surrounding Meulan further widens the statistical basis reducing the number of wastages — indices made unusable because the date of marriage is unknown — and also allows an initial examination of the phenomenon of urban immigration.

It is indeed a town which is treated in this case, certainly with close ties to the surrounding countryside, to the cereal growing Vexin, to the wine producing villages of the vallies and to the country around Chartres. Meulan's socio-professional structure alone — which can be traced over a century (1765-1866) thanks to no less than six census registrations — is sufficient to demonstrate its particularity. Confronting a small skeletal primary sector (between 5 and 10%) the secondary and tertiary sectors were mutually self-

balancing prior to the subsequent supremacy of the latter in the XIXth century. Its particular precocity is also emphasized by low illiteracy rates; in 1670 nearly 65% of the men and 40% of the women were capable of signing their own marriage documents. The interest of this initial urban monograph lies then in its confirmation of the findings of the village studies and in the distinctions which it draws in these key aspects.

Extremely pronounced was the demographic increase of the XVIIIth century — the population doubled, partly as a result, it is true, of simple recuperation — which was three times greater than the national average, and derived from two major demographic waves of thirty years duration, terminating respectively toward 1730/40 and 1790. The employment for the first time of shifting averages reveals clearly the solidarity of the three curves even when in 1670 and 1695 that of mortality is uppermost. The structural solidarity emerges; more births and so at a distance of twenty-five years more marriages, when there was no murderous crisis such as that of 1693/4 and that of 1738/43 to decimate the infantile population and produce abnormalities in the age pyramid at a distance of thirty years. But the conjunctural solidarity, as R. Baehrel rightly observed, needed to be more clearly established: the more marriages consequently the more births, and so, with 40% of all children dying before the age of five, the more deaths. Resulting from a much greater geographic mobility than that to which we are accustomed in the village examples, immigration — and in particular that of young adults — helped reinforce the motive influence of this trend in marriages. This in itself was precocious and became evident from 1785/90; the percentage of men marrying who had been born within the town throughout the XVIIIth century remained around 40% — thereafter it fell to 34% (1790-1814), then to 23% (1840-49), while the area of origin simultaneously widened considerably. The percentage for women resisted better (dropping from 60 to 56%), but an examination of the census registrations reduces the value of these figures; there the women of non-Meulan origin are even more numerous than the men, but the tradition of celebrating the wedding in the parish of the spouse was still maintained.

While this mobility is confirmed by an examination of the origins of the deceased and by the particularly rapid and widespread renewal of family names, it also closes certain doors to M. Lachiver. It becomes impossible, for example, to study adult mortality, while that of infantile mortality is made extremely difficult due to the double movement occasioned by putting children out to wet-nurses both from Paris to Meulan and from Meulan to the countryside. What remains essential however is to examine the way in which the structures which were typical of the 'demography of the Ancien Regime' — scarcity of bachelors and celibates, relatively late marriages, high fertility rates and high infantile and juvenile mortality — began from about 1740 to be transformed. If in terms of infantile mortality the advances made

before 1790 were extremely slow (some 50% of all children born between 1740 and 1789 did not reach the age of twenty), a reduction in the fertility rate is definitely evident before this date; this itself constitutes a transformation which is even more striking than are the fertility rates of the beginning of the century which had made possible the first demographic spurt and which were higher than any others known, even higher than those of Canada. The town was not always a devourer of men.

The evolution however was effected gradually as if a series of 'brakes' had been applied both successively and in parallel. The first of these was traditional; an increase in the age of marriage. Rather than leading to a 'growth in immorality' — which is too often put forward and contains too simplistic an explanation — this caused an increase in illegitimate births, which is evident from 1760. It put a halt, in any case, to the 'multiplicative' influence which the arrival at marriage age of the large groups born since 1715 should have exercised. If then a demographic revolution took place, it began in an entirely classical fashion, that is with the adoption of the first means of controlling expansion known to and employed by all traditional societies; limiting marriage by delaying the age of marriage. But subsequently other breaks became apparent with the gradual establishment of a voluntary limitation of births; in the first case this occurred after a certain period of marriage and the family remained static at the third or fourth child, but subsequently the same attitude became evident even in the early years of marriage without there being any significant variation in attitudes between social classes. Contraception was already practiced by a small number of families and became more widespread; births were spaced out first in those families where they were already separated by 'median' intervals and then in those in which the intervals were closer. From 1790 the use of contraception which some half-century previously had been the practice of a minority became that of the majority. The first 'brake' then became redundant and the years of the Revolution and the Empire saw a decrease in the marriage age which in the case of men tended to be short-lived being motivated by the desire to avoid conscription, but for women proved more long lasting.

Anticipating then by some half a century the evolution of the countryside, the town — and in particular the small town such as Meulan — takes on the character of both a model and an intermediary; the '*funestes secrets*' were reproduced on the pattern indicated by Moheau. Demographic Malthusianism which appeared in France earlier than in other parts of Europe gradually established itself before spreading to the countryside. It brought with it a total change in attitudes toward the family and marriage, toward life, birth and death. Would Meulan in turn follow the example of nearby Paris? The question is worth asking. It seems then that the example of Meulan is valid for northern France, on which the majority of demographic studies

have been concentrated as opposed to the largely uncultivated Midi; is the precocious Malthusianism of small villages such as Quercy, Thézels-Saint Sernin really applicable to the southern regions of France as well?

MAURICE AYMARD  
French Institute, Naples (Italy)

- J. LOPEZ YEPES, *Historia de los Montes de Piedad en España. El Monte de Piedad de Madrid en el siglo XVIII*, Madrid 1971, 2 vols in 4°, pp. XX-645 and 626, with 39 tables.

Señor José Lopez Yepes has devoted an exhaustive study to the Mont-de-Piété (pawn-bank) of Madrid in the XVIIIth century, which appears in a sumptuous edition due to the assistance of the Spanish Confederation of Savings Banks. Of some 1.300 pages the text proper occupies only some 561. The end of the first volume is made up of appendices, and the second is a collection of documents.

The author has been able to produce so extensive a work by exploiting the riches of an archival deposit hitherto virtually untouched, the archive of the Savings Bank and Mont-de-Piété of Madrid. He provides in addition a very detailed description of the sources for the XVIIIth century. In so doing he has been able to revive the study of the Madrid Mont-de-Piété formerly known only through the very old book of Braulio Antón Ramirez (1856).

The greater part of the book is taken up with a highly detailed monograph, the reading of which is not facilitated by the weight of chronological and statistical detail with which it is laden. It would certainly have been possible to have condensed this. The author desired however to relate his austere study to a more general historical perspective. It is for this reason that some 80 pages are devoted to the history of the Monts-de-Piété in Italy and Spain. Without dwelling on what the author says of Italy, for the subject has been treated frequently, we would mention one important work that seems to have escaped Señor Lopez Yepes' attention; the *Archivi Storici delle aziende di credito*, Rome 1956, 2 vols. One should also note that the statutes of the Madrid Mont-de-Piété were inspired directly by that of Rome.

More to our present concern are the antecedents of the institution in Spain. For this the author draws on the classical *Historia de la Previsión Social en España* by D. Antonio de Armas (1944), on the works of Señor Nuñez de Cepeda for Navarre, of García Isidro and Gonzalo Anes for the *pósitos*, and of Voltes Bou for the savings banks of Barcelona. It emerges from these that it is necessary to distinguish between various types of institutions all inspired by the same charitable ideal. In the Mont-de-Piété of Madrid

was the first institution of any importance to have existed in Spain (1702), it was antedated by others of which we have little information, that of Saragossa dating from about 1626 and that of Cuéllar in the province of Segovia founded in 1636. It was already known that both Barcelona and Granada followed the example of Madrid in 1740 and 1741 respectively, but Señor Lopez Yepes' researches have brought to light three other Monts, those of Salamanca (1728), Jaén (1750) and lastly Mexico (1775).

Much older than the Monts-de-Piété were the corn banks which appeared from the late fifteenth century. In Navarre they were called *arcas de misericordia* or *vinculos*; their function was to lend grain to farmers for sowing. The oldest known is that of Goyano (1480), but by this date there already existed a *pósito* at Molina de Aragon and an *alhondiga* at Seville which appears to have been operating for some time. These institutions restricted themselves to assisting peasants in difficulties. They predated the Italian *Monti Frumentari*, the first of which was at Rieti (1488). Under the Catholic Kings the *pósitos* spread. In 1789 they numbered 7,813. Their number however decreased markedly in the XIXth century but, although very much in decline, even to-day they have not totally disappeared.

If the *pósitos* were corn banks the *Montepiós* corresponded rather to redraft deposits. Originally they were designed to assist widows, orphans, and invalids. They increased in number in the XVIIIth century, principally due to the minister Esquilache.

To come to the main subject of the book, the Mont-de-Piété of Madrid, it was founded due to the initiative and tenacity of an Aragonese priest D. Francisco Piquer, almoner of the convent of *Descalzas Reales*. His plan had a double aim, that is to provide masses for the souls in Purgatory and at the same time to aid the poor. Hence the highly characteristic title *Sacro y Real Monte de Piedad de las Benditas Animas del Purgatorio de Madrid*. Don Francisco Piquer began boldly enough by depositing one real in a coffer. He collected alms by placing *caxitas* throughout the city. He had the merit however of understanding that his project could attain the desired scope only with royal patronage. This was not denied him. In 1713 the Mont-de-Piété was placed under royal patronage, and was conceded a large estate near the convent. In 1716 it was granted the right to perceive a portion of the benefices of the bishoprics vacant in America, and in 1723 a remittance of 70,000 reals to meet the wages of its employees. During the same period its statutes were recognized. The institution began to function fully in 1724 and was directed by Father Piquer until his death (1739).

One part of its finances then were provided by the state. The remainder came from alms and legacies, voluntary payments made by borrowers — for no interest was exacted on loans — and finally by deposits made free of charge. The expenses consisted of the cost of the great *novena* for the souls in Purgatory, of the masses, but, above all, of the loans on pawn. In 1725

3,077 persons were also assisted; in 1799 12,683; between these two dates a total of 587,473 persons.

Señor Lopez Yepes studies the working of the Mont-de-Piété, providing a great wealth of detail. Its administration was controlled by a General Junta which normally met once a year and on which sat, in addition to the *corregidor* and the *vicario* of Madrid, the Protector of the enterprise, who was normally a person of considerable standing, as for example the Count of Campomanes (1775-1783), and the Administrator General. Day to day affairs were dealt with by individual juntas at the end of each month.

The personnel was headed by the Administrator General who bore the title of first deputy. This office remained in the hands of the family of Father Piquer until 1794. He was assisted by a second deputy. The functions of the accountant, the depositary, the treasurer, the director of the exchange office and of the assistants were more specialized.

The procedure was relatively simple. Someone wishing to pawn an article went first to the *tasador* who valued it, then to the accountant who registered it, then to the depositary who took consignment of it, and then finally to the treasurer who gave payment. For reimbursements the procedure was reversed. Loans were made on jewelry and possessions furniture being excluded, and great care was taken not to over-value the pledges. The borrowers had in theory a year and in fact eighteen months in which to make recovery. Thereafter the pledges were sold. If the sum realized exceeded that of the pawn, however, the borrower might claim the difference.

It is clear then that the Mont-de-Piété of Madrid was first and foremost a charitable institution and shewed no trace of that 'banking character' that Signor Garrani has illustrated with regard to the Italian Monts-de-Piété.

HENRI LAPEYRE  
University of Grenoble

P. MASSA, *L'arte genovese della seta nella normativa del XV e del XVI secolo*, Genova, 1971, pp. 307.

Derived, as the author tells us, from a study of account books and documents referring to the silk guild and individual entrepreneurs, this study by Paola Massa, which has appeared as 10th volume in the valuable new series of « *Atti della della Società ligure di Storia Patria* », fits into a firmly established tradition of studies on the Medieval and early modern origins of the Italian guilds and corporations. In addition the author attempts to follow the development from these origins over the two centuries — the 15th and the 16th—, which she has taken as chronological boundaries, in connection with the development of the European economy and with the particular

economic and trading problems of Genoa in this period, upon which light has already been shed in the studies of Borlandi, Heers, Sieveking, etc.

The volume is divided into two parts and covers a total of 198 pages; the first part deals mainly with the structure and features of the guild and the second with its relations with other groups of craftsmen and wage-earners involved in the general process of production, and there is a copious documentary appendix (pp. 199-300) to complete the rich collection of footnotes. It is however a rich and complete record, which is well presented and arranged, of original sources for the regulations of the guild found by the author in the Archives and Libraries of Genoa.

The study contains a number of noteworthy points of interest as is only natural in view of the object of the research. The silk industry flourished in Genoa at the time of the decline of the city's trading power, attracting large amounts of capital formerly invested in shipping and trading activities and influencing the whole economic life of the city, which quickly became one of the principal silk producers in Europe. It was therefore only natural that the varying fortunes of the guild came to be identified not only by the silk manufacturers but also by the city administrators with the fortunes of the whole Genoese economy. This was particularly so because, as the author points out, it «was not a guild of craftsmen, but of merchants and capitalists, sometimes even of nobles» (p. 27) who controlled the craftsmen and wage-earners engaged in the various stages of production, and endeavoured — often successfully — to prevent them organizing themselves in guilds of their own which would have given them more power. The guild is then a microcosm in which are reflected economic and social problems which involved much wider areas, and in which the fundamental difficulties of the economic history of the period emerge.

The author describes some of these, which need only be briefly mentioned here: relations with the city authorities, the tax privileges the guild succeeded in obtaining, relations between capital and labour, technical processes, the increasingly «protectionist» character of the regulations as competition increased, the drain — which increasingly severe sanctions proved unable to stem — of craftsmen possessing a wealth of skills and technical capabilities accumulated through centuries, who were attracted by the privileges offered them by rival European cities; all this with pertinent and rich documentary references point by point. It is true however — although this is a risk hard to avoid in works of the kind — that the abundance of details sometimes becomes dispersive and the weight of purely descriptive material often make it difficult to identify the lines of development of the regulations and of the production process as a whole. But it is to be hoped that Mrs Massa will give us a clearer picture of the latter in her future works.

BIAGIO SALVEMINI  
University of Bari (Italy)

F. MELIS, *Documenti per la Storia Economica dei secoli XIII-XVI*. (Istituto Internazionale di Storia Economica « F. Datini », Prato - Pubblicazioni, Serie I, Documenti, no. 1), Florence, Olschki, 1972, 628 pages.

According to the author this work is of a 'principally didactic character'. This is true, in the sense that as a scientific work it instructs. To review it is not an easy task; one must limit oneself to stating that it consists of a long introduction (pp. 1-133), two hundred documents, mainly unpublished, transcribed and annotated, and all reproduced, some in facsimile, a paleographic appendix by Elena Cecchi and four indices compiled in such a way that they can be used as glossaries. To this one would add an invitation to read the work, as this is the only way to appreciate the extraordinary wealth of the contents.

In the 17th or 18th century the study would have been given the title of '*Amplissima Collectio*', in the 19th of '*Chrestomathy*', today it would be called an '*Exempla Scripturarum*' for it is destined to remain a classic model of a certain type of documentary collection, and to herald a new series of publications badly needed in economic history. The study is chiefly concerned with the merchant, not trade, it has a humane, or humanitarian basis linking it with the noblest Italian traditions. In fact the work can be categorized only by the least restricting term 'Anthology', meaning that it is the result of a selection, is a set of examples, of economic documents of a certain category, a selection made according to new criteria, even if the author intended his work to be associated with another anthology, by R. S. Lopez and I. W. Raymond, who in 1955 published a collection of documents, translated into English.

The origins of this anthology lie far back in time, for it is to be associated with the course of lectures given by Melis at the University of Florence during the academic year 1963-1964 on the *Fonti della Storia Economica*.

The author distinguishes between direct and indirect written sources; the first are those of the economic operators themselves and therefore the most reliable, genuine and truthful; these are to be consulted first, while the indirect sources are only substitutes with which the scholar must be content when the direct sources do not exist. Among the direct sources there is a further category of derived sources which are consulted as an expedient, for example the '*quaderni dei cambi*' when bills of exchange have been lost, as is too often the case.

This work contains examples only of direct sources, as a didactic contribution, so to speak, to the highly specialist courses held at the *Istituto « F. Datini »*.

A document says nothing unless it is examined and interpreted correctly and the reader is capable of extracting the juice from it; making the document

speak is both an art and a science. Melis teaches us the science of making it speak, but as for the art... this unfortunately cannot be taught.

The documents are divided into seven groups, general, specialized and private papers, accounts, banking papers, textile manufacture and handbooks for preparation and consultation. For each group there is an individual and systematic introduction indicating the sub-groupings, so that the selection is perfectly methodical; for example the group of specialized papers falls into statements of accounts, cargo papers, invoices, lists of goods, price lists etc. As it would be impossible to analyse each individual introduction, we can pick out the one concerning banking, which deals with three topics of great interest — bank discounts, cheques and bills of exchange. The author shows how the bill of exchange can be discovered, even when the original has been lost, in a company's ledgers. The long paragraph on this subject is a precious guide to the interpretation of other documents.

As for the bank itself, the following definition is important: 'we can talk of a bank only when a number of undertakings habitually favour, back, support and facilitate financially the management of other undertakings, allowing them to overcome the limitations of their fortunes whenever a favourable opportunity for investment occurs, or equally to avoid a forced release'. Consequently the concept of the medieval bank should exclude those loans which were so called even if the lender knew perfectly well that he would never see either the interest or his principal capital again, in other words those exactions made to governments or individuals that went by the name of loans but were in fact payment for favours or privileges. Melis observes that the banking function of a given concern, as defined above, can be examined more easily through the debtor's accounts than through those of the creditor, and this is true even in fields far removed from those considered in the work; in fact from debtors' accounts I have been able to ascertain the credit activity of some Palermo banks in favour of retail trade in the first half of the 15th century and of the *Tavola di Palermo* in favour of the wholesale trade in the second half on the 16th century.

Setting aside questions of method, the documents selected are also important in and for themselves and introduce the reader to fascinating topics such as weaving techniques, Flemish, Portuguese and Andalusian shipping in the Mediterranean from 1300, the development of trade practices, mercantile arithmetic — here represented by Leonard Fibonacci — weights and measures, prices, Europe's relations with the mysterious Levant and with America...; a whole store of facts, with bibliographical notes, references and introductions, which surpasses the limits of its didactic purpose and assumes wider significance; it is an extremely useful opportunity for those already concerned with economic history to keep abreast of developments and a lively and fascinating introduction to those approaching it for the first time.

One of the letters is in code (p. 228), written from Lyons to Filippo Strozzi in Venice and giving political news; it is one of the common cipher codes in use throughout the 16th century, also for diplomatic purposes. The code is composed of the numbers from ten to a hundred, some substituting single letters and some, pairs of letters (though many denote the same sound), while others indicate cities or persons; the numbers from eighty-one to a hundred are those called 'zero' in cryptography and are rather ingenuously placed at the beginning of the paragraphs.

The paleographic appendix, the result of remarkable labour and interpretative insight, is very useful when one enters the field of individual calligraphy which, like abbreviation, ignored the classical rules of paleography, perhaps because the writers had abandoned Latin but had not yet mastered the rules for writing in the vernacular.

Our thanks are again due to those who have played the part of Maecenas in the production of this work, which does great honour to the Italian academic tradition.

CARMELO TRASELLI  
University of Messina (Italy)

- O. PICKL (ed.), *Die wirtschaftliche Auswirkungender Türkenkriege*, Graz, Selbstverlag der Lehrkanzel für Wirtschafts- und Sozialgeschichte der Universität Graz, 1971, 366 pages, no price indicated.

This volume contains the reports submitted to the 1st International Symposium on the Economic and Social History of South-East Europe held at Graz from October 5th to 10th, 1970 under the sponsorship of the regional government of Styria. These are 18 contributions by scholars of different nationalities (French, German, Austrian, Polish, Hungarian and Yugoslav), the object of which is to outline aspects and problems relating to the economic and social consequences of the wars fought between the Christian and Turkish armies in this part of Europe. The period considered is the 250 years between 1450 to 1700, that is to say from some years before the fall of Constantinople to the beginning of the Turks' retreat to the Balkans, which can be set at the time of the second siege of Vienna (1683).

The subject of the Symposium was touched upon only a few years ago, in the summer of 1967, at the Conference organized by Ingomar Bog (who was also one of the relators at the Graz Symposium) at the Seminar of Economic and Social History of the Philipps-Universität of Marburg and der Lahn of which he is the Rector, on the foreign trade of Central Eastern Europe between 1450 and 1650 (see the *Proceedings* in « Der Aussenhandel Ostmitteleuropas 1450-1650. Die ostmitteleuropäischen Volkswirtschaften in ihren Beziehungen zu Mitteleuropa », edited by I. Bog, Köln-Wien, Böhalu

Verlag, 1971). Later the attention of scholars was called more specifically to the importance of the Turkish occupation of the Balkans — and of Hungary in particular — by Makkai, who stated that this occupation had the dual function of mobilizing human resources along the military frontier on one hand — just as, according to the author, was to be the case later on the American West « frontier » — and on the other hand of upsetting and delaying the economic development of the Balkan peninsula (see L. MAKKAI, *Turner's Thesis and the « Balkan Course » of Agricultural Development in Hungary during the Turkish Occupation* in « Ricerche storiche ed economiche in memoria di Corrado Barbagallo », edited by L. de Rosa, Napoli, E.S.I., 1970, II, pp. 275-290).

The scholars present at the Graz Symposium devoted their attention above all to the repercussions of the Turkish wars on trade relations among the Balkan countries, and between the Balkans and the Ottoman Empire; at least half the reports deal more or less explicitly with this subject. Many of the contributions are devoted to the direct consequences of the Turkish wars on Balkan trade. First among these is Pickl's study of the effects of the Turkish wars on trade between Hungary and Italy in the 16th century, in which he outlines the nature of Italo-Hungarian trade along the principal trade route uniting the two countries (the *Laibacherstrasse*), which was based on the exchange of cattle for textile manufacturers. He shows how the volume of this trade, after increasing continuously up to the 1580's, began to shrink beneath the pressure of military events affecting the peninsula. The situation in the following decades is described by Prickler in his study of the volume of Hungary's export trade to the West from the 16th century to 1700; the author succeeds, in giving a fairly clear picture of the difficulties besetting Hungarian trade in the 17th century when, as a result of the continual wars, it suffered frequent and considerable variations of fortune which only began to level out in the last decade of the century, following the Ottoman « retreat ». On the other hand Kubinyi, in his study dealing with the effects of the Turkish wars on the central Hungarian cities up to 1541, describes the pattern of production (mainly agricultural) in some dozen of the most important of these towns between the mid 15th and the mid 16th century. He outlines clearly the contrast between the difficulties of economic life on the one hand, due to the fighting and ensuing insecurity, and the cultural and social vitality on the country on the other which, up to 1529, the year of the siege of Vienna, showed a progressive increase in the number of its students at that University. Closely connected with Kubinyi's paper is Szakaly's which deals with the problem of the continuity of the economic structure in the Hungarian villages during the Turkish domination. The author examines the whole of the period during which Hungary was subject to Ottoman rule, showing how the pattern of production remained unaltered in the decades between 1541 and 1699, although it obviously registered all

the effects of the military events in that period. Further proof of this is provided by Ruzsas's paper which provides an analytical study of the development of the trans-Danubian villages under Turkish dominion in the 17th century.

A second group of reports deals more widely with the consequences of the Turkish wars on the trade and economy of the Balkan regions in general, examining the geographical boundaries in which these effects were felt. The papers of Malecki, Braunstein and Lerner, deal respectively with the changes undergone by the trade of Cracow and of Poland in general at the time of the Turkish wars of the 16th and 17th centuries, with relations between Venice and the Turks (1480-1570), and with the effect of the Turkish wars on the trading centre of Frankfort on Main. These studies show clearly the importance of the market of the Balkan regions on the principal trade routes of the period. Above all they show how even distant markets, such as Frankfort on Main, were affected by the lack or interruption of Balkan trade at the time when south-east Europe was confronted by the Ottoman danger. That the economy of this part of Europe was an integral part of the economy of the continent at the beginning of the modern age is moreover fully demonstrated by Kellembenz in his valuable paper on this subject. He shows clearly enough, in fact, that in spite of the virtually permanent state of war between Christian and Turkish armies in the Balkans, the rest of Europe could not do without a number of its products, especially cattle, which reached the West through Ragusa and Venice.

Of particular interest are the papers of Bog and of Vilfan, both of which examine the financial aspects of the Turkish wars. Bog analyzes especially the problems of the financing of the Christian army and of the war expenses in general: in 1677 alone, the author tells us, the Christian army cost more than 4 million florins (p. 18), a huge figure, far beyond what the imperial exchequer could collect through direct taxation, so that private financiers played an important role. Vilfan, for his part, describes the function of raids and looting in the financing of the armies facing each other in the Balkans, in addition to the amount of revenue from direct taxation in Slovenia-Carniola in the 16th century.

To these papers dealing with the problem of financing war expenses must be added Tremel's study of iron production in Styria in the 16th century; this production was of considerable importance in supplying arms and war equipment to the imperial armies in particular due to the advanced stage reached by the Styrian iron industry. Lastly, Roth's and Cervenka's papers are devoted to monetary problems. The former in particular examines the Turks' financial and monetary needs, while the latter, on the basis of archaeological reports and making use of modern processing techniques, describes the breadth of the area monetarily affected by the Turkish wars.

A final group of studies is devoted to the problems of society and the

army in the Balkan region. Kiss's and Moacanin's studies in particular illustrate respectively the particular character of the soldier-peasant in the military border areas, and the nature of the landed property of the military population on the Croat-Slovene border in the 16th and 17th centuries. Vasic examines the nature of society and of the economy beyond the «military border», that is to say on the Turkish side, in about the same period, while Bieniarz endeavours to examine the Turkish influence on customs and culture in Cracow in the 17th century which was illustrated through the use, in Poland, of certain objects, such as carpets, ceramic, etc., or descriptions, diaries, etc., concerning Turkish life and customs.

From the different aspects and problems dealt with, there emerges from the papers collected in this volume a factor that is of particular importance for an understanding of the economy of the Balkan peninsula during the Turkish wars. For while it is true that the economy and trading relations in this period were upset and disturbed in varying degree, on the whole they were never completely destroyed. As has recently been stressed by Darby, economic interests were always considered no less important than political interests by the Turks.

ANTONIO DI VITTORIO  
University of Bari (Italy)

P. SPUFFORD, *Monetary Problems and Policies in the Burgundian Netherlands. 1433-1496*, Leiden, E. J. Brill, 1970, £ 7.80.

Mr. Spufford, having already established himself in the journals as a notable authority on coinage, has now produced one of the very best fusions of numismatic and monetary history that I have ever read. In that respect he is indeed a *rara avis*, since so many quite able numismatists are lamentably ignorant of economic history in general, thus failing to perceive the true significance of their observations, while many economic historians who occasionally delve into monetary affairs are prone to make often egregious errors because of their inexpertise in numismatics. I myself cannot lay claim to the same degree of numismatic excellence as Mr. Spufford — at best, to an amateur status; but I do feel confident in stating that, having myself researched much of the same documentary sources (to the 1470s), I could find no significant errors in either his numismatic or economic analysis. On the contrary, I am much indebted to Mr. Spufford's book for revealing a few blunders that I myself had made in a study on Anglo-Burgundian economic relations. (Fortunately I was able to rescue my manuscript from the publisher in time to rectify them).

The one minor defect worthy of at least passing note that I did detect

is the statement that the county of Namur's coinage had been unified with the Flemish before the monetary reform and general unification of 1433-4. In fact, however, both the gold and silver coinages which Duke Philip the Good struck in Namur, from June 1425 to May 1433, differed significantly from his Flemish coinages. Indeed, Philip's purchase of Namur in 1423 proved most opportune in permitting him free rein to conduct extensive and very profitable debasements there during these years of his French and Dutch wars, while the Flemish Estates had forced him to maintain monetary stability (except for the silver debasement of November 1428) in that county. Curiously enough, the years from Duke Philip's accession to power in 1419 to the coinage unification of 1433-4 have generally and quite unjustly been relegated to the shadows of Burgundian monetary — and economic — history. Yet this was the very period of Duke Philip's most active mint manipulation (at Dordrecht and Brussels as well), the consequences of which help to explain why Philip had to adopt the monetary reform of October 1433, and thereafter to maintain a « sound » and hard-money policy, apart from the defensive revaluation of gold in 1454, for the next thirty-three years.

Mr. Spufford's book, despite its title, does not purport to present either a chronological narrative or a complete and concerted analysis of Burgundian monetary policies for the period 1433-1496 (ending as it had begun with a major coinage reform); rather it consists of six essays on particular aspects *des affaires monétaires* over this period in general, each on the whole very fascinating and most valuable for numismatists and economic historians alike. The first study deals with and, I trust, will finally resolve that old hoary, bristly debate over the nature of medieval « monies-of-account ». In the main he supports with quite conclusive evidence Prof. Van Werveke's contention, one disputed by even the great Marc Bloch, that almost all the various monies-of-account used in the Low Countries were tied directly or indirectly to the silver penny or *groot*, though not as he rightly points out to any specific *quantity* of pure silver. The few exceptions were those based upon gold coins, either those in current circulation with variable silver values, or « ghost » coins with permanently frozen values. As an added footnote to Mr. Spufford's essay, the municipal accounts of Leuven are worth citing as a curious example of a transformation from a « ghost- » gold money-of-account to a variable-gold to, finally, a purely silver-based system. From the earliest extant account of 1345 to c. 1405, the tax receipts were recorded in £ s. d. *assise geld*, tied directly to the gold *schild* (*écu* or *shield*) struck by Emperor Louis IV (1328-46), weighing 4.53 g., as follows: 1 *schild* = 16½ d. *oude grout* = £1. 13s. Od. *assise geld*, with £24 *assise geld* equal to £1 *oude groot*. Thereafter, to mid-1436, the Leuven accounts were recorded in *Rijns gulden* (1 Imperial florin = 1.408 *oude schilden*), which were evaluated quarterly by their current market rates in silver *plakken* (Brabantine pennies, each subdivided into 24 *miten* = 24s. *payemente*, as another money-of-account):

thus, a receipt of 12 *gulden*, 32 *plakken*, 16s. 5d. *payemente*. In 1436, when Brabant finally acceded to the Burgundian monetary unification, the accounting value of the Leuven accounts' *Rijns gulden* was arbitrarily fixed, for the next sixty years, at 54 *plakken* (= 18 *stuivers* = 4s. 6d. *groot* Brabant = 3s. Od. *groot* Flemish); and thus this artificial *Rijns gulden* had in effect become a silver *groot*-based money-of-account. Finally, in 1497, the Leuven treasurers adopted the Burgundian-Hapsburg Receiver-General's favourite money-of-account, the pound of 40 *groots* Flemish (= 3s. 4d., or one-sixth *pond groot* Flemish, the other contender as the most used money-of-account).<sup>1</sup>

In his next two essays, on « Coinage » (struck) and « Currency » (in circulation), based upon astute analyses of both the mint records and extant coin hoards, taking good account of the limitations of each, Mr. Spufford establishes five monetary transformations of considerable significance for the period 1433-1496: (1) from a pro-gold to a pro-silver mint policy (but in fact a *reversion* to the traditional, pro-silver policy, after having for almost half a century followed the suit of other north European countries' pro-gold policies); (2) from a foreign to a Burgundian preponderance in the circulation of gold coins: (3) from an English-French to a Rhenish-German dominance in that foreign gold circulation; (4) similarly, in minting the Burgundian gold, from using the English and French to taking the Imperial German coins as the models; and finally (5), from Flemish to Brabantine leadership, or at least Brabantine equality, in the aggregate mint outputs. All together these changes would seem to reflect, as Mr. Spufford himself suggests in part, the South German commercial expansion to leadership of the European economic revival from the mid-fifteenth century, based largely on the Central European silver-copper mining boom; and more especially the Rhenish-South German propulsion of the Brabant Fairs to ascendancy over the traditional, long-established Flemish commercial towns. Certainly these conclusions and the significance of the monetary changes seems to be well borne out by his fascinating, heavily-documented account in his next chapter (« *The Guerre Monétaire* ») of the competition between the Burgundian and Rhenish-Imperial mints during the fifteenth century. (The other three parts of this chapter are not so successful: those on Tournay and Calais do not break much new ground; and that on the French-disputed mints of Amiens and St. Quentin, though indeed new, cannot be described as being vitally important).

I am not as convinced as Mr. Spufford that the fluctuating currents of Burgundian foreign policies and political ambitions had as much influence upon, particularly, the types of Burgundian coinages struck — though quite admittedly an apparently close correlation does exist. I cannot agree, for

---

<sup>1</sup> Stadsarchief Leuven, nos. 4986-5124. Most of this may be verified from material presented in Raymond van Uytven, *Stadsfinanciën en Stadseconomie te Leuven van de XIII<sup>e</sup> tot der XVI<sup>e</sup> eeuw* (Brussels, 1961).

example, that Philip the Good had struck his gold *nobles* just because of the Anglo-Burgundian alliance (1419-1435). Rather I believe that he had had them struck as deliberate and inferior counterfeits of the English *nobles* to «facilitate» his subjects' purchase of English wools at the Calais Staple; and that this deceitful practice led to monetary conflicts that contributed as a strong irritant to the breakdown of that alliance — and to the more rapid decay of the wool trade and of the Flemish economy, indirectly benefiting the more German-based Brabantine economy.<sup>2</sup>

For economic and especially monetary historians Mr. Spufford makes perhaps his greatest contribution in his fifth chapter on «The Profit of the Duke», by demonstrating that the *seignorage* revenues, in effect that percentage of the bullion retained by the prince as a feudal tax on minting, did not constitute a pure profit, as so many historians have wrongly assumed. For, from these revenues (plus the *remèdes*, or excess metal left from deficient minting, and occasional bullion confiscations), the prince had to meet several costs, some fixed and some variable: the salaries of those monetary officials attached to his court (e.g. the *maître général des monnaies*), his mint wardens or inspectors and assayers; the maintenance of the mint itself, and depreciation of its capital installations. In Appendix VI, Mr. Spufford provides from those fully complete accounts still extant estimates of actual, *net* profits (though unhappily *just* the net profits, without the corresponding revenues and expenditures), showing that for long periods of monetary stability and thus of relative inactivity the mints generally operated at a deficit. The major exception was the period of Archduke Maximilian's war-inspired debasements of the 1480s, when the mint profits rose sharply to account, perhaps once or twice, for as much as 20 percent of total ducal revenues (which themselves are generally most difficult to calculate accurately from the Receiver-General's accounts). I myself have attempted to calculate net mint profits from some of the pre-1433 accounts (of Namur and Dordrecht), but cannot claim much success: for, so different were medieval from modern accounting procedures that it was generally most difficult, if not impossible, to distinguish capital from current expenditures and disguised loan repayments (and interest) from genuine salaries. But finally, to have better clarified his most valuable presentation, Mr. Spufford ought to have noted explicitly that the other, direct, and more important costs of minting — the wages of the mint artisans, the tools and coin-dyes, the coinage alloys, etc. — were met from yet another charge on the bullion supplied to the mint, the mint-master's *brassage*, also a fixed percentage, the residual balance of which thus constituted his own payment or profit.

The final chapter, «The Interest of the Estates», probably the most interesting and useful one for the *general* historian, has already appeared in

<sup>2</sup> As I have argued in *An Economic Aspect of the Collapse of the Anglo-Burgundian Alliance, 1428-1442*, «English Historical Review», LXXXV (1970), pp. 225-44.

expanded form as an article in *Standen en Landen*, XL (1966). Unlike a few of my colleagues, I myself accept and would willingly defend his major thesis that conflict over the coinage played the major if not sole role in the development of the so-called Estates-General of the Burgundian Low Countries as a constitutional organism from the mid-1430s. Once Duke Philip had unified the coinage in 1433-4 — and after much consultation with his subjects, argues Mr. Spufford, the Burgundian and then Hapsburg rulers had no choice but to convoke joint assemblies of all their provincial estates to secure « advice and consent » for any further coinage changes. Clearly the Estates had a vital interest in asserting control over the coinage: to curb debasements which, through elastic minting profits, permitted the dukes much greater financial independence, while wrecking inflationary havoc upon the economy. Nevertheless, it does not really appear that the Burgundian Estates ever managed to acquire as much power over the mint, and the « purse » in general, as did the English Parliament.

Mr. Spufford's study concludes with six appendices (in 50 pages), the most important of which presents tables of the Burgundian coinage production from 1433 to 1496.<sup>3</sup> He has chosen to do so exactly as presented in the accounts: the number separately of each of the several gold and silver coin denominations struck at each mint for the period of the mint-accounts, which ran variously from a few months to occasionally two years or more. Then *monthly* (estimated) averages of each of these coin denominations, consolidated for the principalities of Flanders and Brabant Mechelen (but curiously omitted for the others), are presented in bar-graphs. Certainly numismatists and many monetary historians will be most interested specifically in the monetary data as thus presented; and if other economic historians would prefer to have annual and aggregate estimates of the amounts of pure gold and silver bullion struck, they will soon hopefully be satisfied by the publication of such data, at least for the period 1384-1480.

Monetary let alone numismatic history is notoriously difficult to write, as many of us can well attest with blood, sweat, and tears. One should therefore not only congratulate Mr. Spufford on a most able and valuable performance, but express the ardent hope that he will continue to pursue this field, and not become too attracted by parliamentary history, in which I see he has the most recently published. The laws of comparative advantage should dictate for him the former field, which so many before him have attempted and abandoned.

JOHN H. MUNRO  
University of Toronto

---

<sup>3</sup> The other appendices are on monies-of-account, coinage hoards and records of coinage collections, mint profits, and « monnaie noire », or *billon*-coins, petty-coins. No tables were presented of the mint-indentures or ordinances prescribing alloy, weight, value, bullion-prices, seigniorages, or brassages.