
REVIEWS OF BOOKS

- A. BEN-DAVID, *Talmudische Oekonomie. Die Wirtschaft des juedischen Palaestina zur Zeit der Mischna und des Talmud*, illustrations, maps, George Olms, Hildesheim, 1974, Band 1, pp. 454.

To review a first volume of a major study is a thankless assignment. Ignorant of the contents of the yet unpublished parts, the reviewer is in the dark as to whether his comments on the available portion will be verified or refuted by forthcoming instalments. The present case is no exception and the reviewer may be frustrated as soon as the subsequent volumes appear. Bearing this in mind, the first volume of Ben-David's *magnum opus* raises high expectations for the promised second and third volumes in the first large-scale attempt at departure from the traditional religious and legal treatment of talmudic topics.

The first volume impressed this reviewer as a painstaking and comprehensive compilation and classification of economic data in the Jerusalem Talmud. After an extensive introduction on the objective of the study, the structure and history of the Talmud, and its historical and geographical scenario, the author describes the factors of production — natural and human resources — as they appear from the sources, then turns to a meticulous presentation of Palestine's agriculture, pre-industrial (artisanate) production, commerce, and service industries. The major part of the first volume concludes with a discussion of the country's rather low income and living standards.

The minutely detailed enumeration of the study's various topics evidences the author's familiarity with, and command of the source material — not a

minor achievement — as well as competence in marshalling and presenting data. However, modern economic history has come to consider recording and categorizing economic data of a past period as merely one first step in the pursuit of an economic historian's assignment. This first step has to be followed by quantification, or, in the absence of adequate quantitative data, at least by an attempt at conceptualization and analysis. In this respect, Ben-David's first volume does not meet expectations. Despite several attempts at quantification the author seems inclined to endorse the rather apodictic contention of his mentor, the late F.M. Heichelheim, that the conversion of ancient prices and wages into modern terms is to be considered as an idle speculation (p. 441). Although this subject remains controversial the author would have found Colin Clark's attempts at conversion and comparisons of cost of living standard helpful. The author apparently experienced difficulties in applying the tools of modern economic research to the overwhelmingly legal and didactic material of the "*Halakha*". It may well be that the talmudic preference for legal casuistry has led the author away from conceptualization and from paying sufficient attention to the difference between « case histories » and relevant events and developments. He seems to share with other historians a disinclination to distinguish between « significant historical » events and developments, and those which are merely significant historically.¹ As a result the reader is often at a loss to understand whether a specified casuistic narrative, of which the first volume abounds, represents a general rule, an exception, a normative example, or what the French call "*petite histoire*". A propos French — the footnotes and the bibliography (94 pages, or about 20 percent of the first volume) show a disproportionately small use of French secondary material; the author cites less than half a dozen items in French despite the considerable French contribution to many branches of Palestinology.

A major reservation of this reviewer refers to the author's statement (p. xix) that the Talmud « reflects a uniform economic picture that remained unchanged during six centuries in its foundations, structure, and important development ». Is this view tenable given the many changes in Palestine's political fortunes during an era almost as long as from the end of the Crusades to the present? It seems unlikely that such processes as the absorption of Palestinian Jewry first into the Hellenistic, later into the Byzantine orbit and the Persian interlude did not influence economic developments. Some initial research by Eijes, Gulak, and Lambert on money and banking during the Talmudic period does indeed indicate major changes in the Palestinian economy.

Another reservation refers to the author's contention (p. xix) that the economic data, developments, and conclusions in the Talmud « never con-

¹ The wording is from J.R. LEVENSON, *Confucian China and Its Modern Fate* (University of California Press in 1965).

tradict » the basic rules and laws and 20th century economics, whatever these may be. One cannot help asking for the reason of the gratuitously injected polemical note and the author's assertion that his study proves that « the Talmud is right » — as if anyone (except medieval or postmedieval religious pamphleteers) had doubted or attacked the genuineness of that source. Later in the text the author seems to reverse himself in stating that he believes it to be wrong to measure ancient economies with modern yardsticks, or to prove the modernity of ancient phenomena. Perturbing is also the author's repeated presentation of specified events as historical "firsts" (e.g. pp. 98, 231) which could be easily refuted.

It seems difficult to write on such an extended period as Ben-David does without falling into inconsistencies and contradictions. Confronted with a massive amount of raw material such as the Talmud represents the most conscientious author faces the task of selection. If not, he risks the consequences of the old adage « *taphasta harbeh, lo taphasta* » (*Qui trop embrasse mal étreint*). Among the contradictions is the ambiguous treatment of slavery. On the same pages, 71-72, the author states (1) that Jewish and Gentiles slaves are equally treated by talmudic law, and (2) points to preference Jewish slaves enjoyed and the reasons for them. Another contradiction refers to wine: On page 226 we are told the Talmud prohibits the use by Jews of wine which has been handled by Gentiles. He derives this prohibition — along marxist lines — from the need to protect local viticulture. Further on, on pages 226 and 229, we read of wine imported into Palestine for local consumption from Asia Minor, Italy, and Cyprus. This allows several conclusions: (1) That the wine from Asia Minor, Italy, and Cyprus was manufactured by Jews — a rather improbable assumption; (2) that the prohibition of gentile-made wine was kept more in the breach than by observation, or (3) that the author was discussing situations at different time-periods without telling the reader.

The most interesting sections of Ben-David's first volume are towards the end: the attempt to construe an (unquantified) balance of payments albeit without breaking down the entire talmudic period into sections during which the components of the balance must have varied; an analysis of income and living standards, and an attempt to arrive at an approximation of a national product.

From Ben-David's enquiry there emerges the picture of an almost permanently depressed subsistence economy whose precarious situation is aggravated by Palestine's strategic significance as a transit region. Some of the topics the author deals with in this last part of his first volume have quite a modern ring, such as the emphasis on austerity (because of excessive luxury practised by some?), the dependence on capital import, crime, emigration, low incomes and high price levels — phenomena which

required flexibility of the law and its interpreters to cope with permanent or recurrent emergencies.

The book is demanding and makes heavy reading. It is written in a professorial, and occasionally condescending and needlessly editorializing style. It could have been improved by more careful proof-reading and by seeking to achieve a more precise and concise presentation with fewer obscurities of which the concluding paragraph on page 330 is a good example.

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V. BERGHAHN, *Rüstung und Machtpolitik: zur Anatomie des "Kalten Krieges" vor 1914*, Mannheimer Schriften zur Politik und Zeitgeschichte, Droste Verlag, Düsseldorf, 1973, pp. 94.

Peace research and conflict resolution have become considerations of prime importance to social scientists. A great deal of the work done on these topics has been interdisciplinary in nature, and Berghahn's study, firmly anchored in historical analysis, well-written and cogently reasoned, is an excellent West German contribution to this type of enquiry.

Staking out for his study the armaments plans that were conceived during the Wilhelminian period prior to 1914, a period for which there is an abundance of documentary materials, Berghahn concludes that what took place at that time was in all probability a prototype of modern armaments production in support of a military-industrial complex. Indeed, what occurred in the Reich then seems to reaffirm a fundamental proposition of the students of peace research and conflict resolution; namely, that armaments production in the eyes of the establishment appeared to be essential for the maintenance of the economic position of the ruling caste. In the 1890's Germany was experiencing a socio-economic crisis, and the emperor and his advisors were of the opinion that one way of solving the grave problems was by means of governmental subventions for the production of armaments, for such outlays would keep the industrialists prosperous and the proletariat docile (p. 17). But, William II did not wish to increase the size of the army by conscripting large numbers of workers, since he feared that they would prove unreliable in the event of a need arising to quell social disturbances. In consequence, the enlargement of the navy seemed to be the only feasible route to take: it was much more elitist than the army, and in addition ship-building was a lucrative undertaking and the money spent on it would therefore be of great benefit to German industry and, as well, would provide thousands of jobs.

Economic and social considerations, moreover, dovetailed nicely with the climate of opinion, much influenced by *Mahanism* and Social Darwinism.

This climate helped to shape the plans of the emperor and his ministers, especially Tirpitz, and engendered an aggressive and competitive stance that was directed primarily against Great Britain. As they envisaged it, the forthcoming struggle with the British for overseas markets would spur on German industry and trade. Here was to be found the dynamic for German commercial greatness in the new century which had just dawned (p. 25).

To attain this goal the war fleet would need to be expanded at least to a point where its strength would only be about one-third less than that of Great Britain. To construct this so-termed "risk fleet" the government required the support of the Reichstag. For this purpose, therefore, Tirpitz undertook to form a communications bureau in the Reichs-Marine-Amt whose express goal was to "mobilize the masses" on behalf of the navy (p. 37). In effect, Tirpitz functioned as a minister for propaganda; perhaps, the first modern version of the type that has become so much a phenomenon of the twentieth century. His success was to be measured by the outpouring of anti-British sentiment unleashed by the seizure on the part of the British Royal Navy of two German postal steamers off Africa, and by the apt way in which he and the emperor used this incident to put pressure on the Reichstag to pass another naval bill in 1900 (p. 41).

In order to stimulate further the climate of hurrah-patriotism, Tirpitz encouraged the propaganda activities of the *Deutscher Flottenverein*. But within a relatively short period of time this organization had acquired so much power and influence that it operated independently of the Reichs-Marine-Amt. For example, Tirpitz had received substantial appropriations for naval ship-building in 1906, and, hence, he had decided not to ask the Reichstag for more funds the following year. Nevertheless, the *Flottenverein* mounted a drumfire of criticism against this decision and Tirpitz was forced to yield to it, for the organization reflected the economic desires of powerful industrial interests. In consequence, the Reichs-Marine Amt became the captive of these interests and finally wound up by abandoning its risk fleet concept, and by paying non-competitive prices for the new, and theoretically at least, unwanted ships it was ordering from the firms that controlled the industry; an industry that had attained this position due to previous governmental orders.

Given the fact of cost overruns in the building of the fleet, moreover, it soon became necessary for the government to consider the prospect of either curbing such expenditures and thereby jeopardizing the building programme, or increasing the amount of government revenue through higher taxes and thereby running the risk of having to implement tax reforms, including the direct taxation of incomes; such reforms, however, would cause the incidence of taxation to fall even more heavily than heretofore on the working and lower middle classes, and as well on the possessing classes, the latter having so far not borne the burden, since these levies had been in the form of consumption taxes. Under the circumstances, therefore, tax

reform might well have opened the floodgates of social unrest, and this was the very situation the emperor wanted to avoid (p. 68).

Berghahn's model, whose *point d'appui* is to be found in the theories of peace research and conflict resolution, lays bare the complex working of Wilhelminian Germany. Furthermore, by comparing it with Konrad Jarausch's study of Bethmann-Hollweg and Fritz Blaich's work on cartels, one can well comprehend why the attempt by the Reich to obtain a dominant position in the economic and political arenas of the world ended in a *Feblgriff*.

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J.-F. BERGIER, *Problèmes de l'histoire économique de la Suisse. Population, vie rurale, échanges et trafics*, Berne, Francke, 1968, pp. 95 (Monographies d'histoire suisse 2).

The *Monographies d'histoire suisse* are a new collection designed for the general public and launched by the *Société générale suisse d'histoire*. The aim of these monographs, which are under the direction of Walter Schmid, is to draw together specialists in the social sciences and scholars of the political, economic and social development of Switzerland. As it draws on the talents of experts in Swiss national history, the collection offers a good introduction to the main trends in contemporary historical research and draws attention to the main problems in Swiss history.

In the space of six years Professor J.-F. Bergier has made two remarkable contributions to the series which offer, for the first time, a comprehensive picture of the conditions of the industrial development of Switzerland from its beginnings to the present day. J.-F. Bergier was professor of economic history at the University of Geneva from 1963 to 1969 and now occupies the chair in the history of civilization at the *École Polytechnique Fédérale* in Zurich. He is also editor of the *Revue suisse d'histoire*, president of the Swiss Society of Economic and Social History, which was founded in June 1974, and Vice President of the International Association of Economic History. These are his qualifications for his study of Switzerland which he places in the European context. These two works complement each other, and it would be difficult to consider them separately.

The author has abandoned the traditional chronological frame-work which does not permit a comprehensive view of the original characteristics of Swiss economic history, and has chosen to concentrate on certain fundamental problems. These provide the structure of the work and take the form of three short essays on:

a) the growth and distribution of the population in the Alps, the countryside and the city;

- b) the essential aspects of rural life - cultivation and pasture;
- c) the development of trade and transport.

These three topics together explain the general development of the country and dominate Swiss economic life until the end of the XIXth century. The Alps emerge as an element of major importance, and Professor Bergier attempts to analyse their role and to measure their influence on the structures of the economy. The Alps were never an unsurmountable obstacle, and it has always been possible for men and trade to pass over them. The Swiss cantons, being in the heart of the Alps, were involved in commerce and profited from it as the guardians of the great mountain passes. But Switzerland was not only a mountain state, and its economic history concerns the difficult relationship between the population of the plains, towns and mountains. Demographic peculiarities are shown with the aid of tables and graphs, which clearly reveal the paradox of a relatively low, yet constantly over-energetic population. This is an essential, fundamental, feature in understanding the economic history of Switzerland, as is the urban equilibrium (towns increased in number, but remain small) which was to last until the late XVIIIth century. The main body of the work is taken up with problems of rural history, and the author examines in particular the concept of an equilibrium between town and country, between the plains and the mountains and between the different zones of production. Using selected examples, the author provides a clear and lucid picture of the development of agriculture and livestock-rearing which brings out the main phases of development — from a strictly demesne economy, to one that was mixed, then commercial until the last century when the rural economy began to be industrialized. Professor Bergier is not content simply to describe, but also attempts to explain certain peculiarities and unique features of the Swiss experience by drawing critically on a number of recent theories, the limitations of which he does not hesitate to point out. It is this constant search for information and for evidence which gives his pages their dense and weighty character.

Turning then to the subject of trade and transport, Professor Bergier describes the principal features of the Swiss situation and draws out certain major developments in this sphere which quite go beyond more commonplace and often fictitious paradigms. He distinguishes three main phases — the age of fairs and the opening of the Alpine passes in the late Middle Ages — the age of the merchants and of mercantile capitalism lasting until the mid-19th century — finally, and closely connected with early industrialization, the age of the railways which the Swiss cantons, federated only in 1848, found very difficult to accept despite their traditional function as a transit centre in the heart of Europe.

In this first study then the framework, the context and the background for the development of Swiss industry is described. It was another six years before a second and rather larger study, *The Birth and Growth of Industrial*

Switzerland (Naissance et Croissance de la Suisse industrielle) appeared from the press. In the meantime Bergier had published, among other works, an essay entitled « The Industrial Bourgeoisie and the Rise of the Working Class 1700-1914 » (London 1971) in which he examined the effects of industrialization on all levels of society.

In the second volume on Switzerland Professor Bergier seeks to avoid all ambiguity by stating explicitly the objective and limits of his study — the long-term development of the structure of Swiss industry. He tackles the following principal problems. How were the regions which make up present-day Switzerland, lacking as they do raw materials and access to the sea, able to manufacture and sell throughout the world products which have gained a high reputation for quality? How was it that Swiss industries succeeded in establishing themselves not only in Switzerland itself (where in 1970 they employed 51.4% of the population and provided over half the gross domestic product) but in virtually every other country in the world as well? What factors enabled this growth to occur and what obstacles had to be overcome?

To ask such questions is an easy matter — to provide concrete answers is much more difficult. The problems of the approach to adopt are numerous, and arise especially from the fragmentary nature of the documentation. The lack of the classical sources on which to base economic history is particularly serious for 19th century Switzerland, and the large number of gaps make it practically impossible to attempt a quantitative analysis of Swiss industry in this period as has been done for other neighbouring countries (p. 13). Yet such a study would be essential if the Swiss industrial effort is to be related with any degree of accuracy or precision to the international context.

The value of this study is further enhanced by the fact that in Swiss French language historiography there is no general study of the development of the Swiss economy from its origins to the present. This lucid and intelligent work amply fills this gap. It traces the entire history of the development of the economy from pre-history to the recent structural crises, and draws out its main, permanent, features — the natural conditions of the country, the problem of the mountains, the mainly poor soil which is incapable, now as in the past, of feeding the population even though it is small; the distance from the sea and the communication facilities; and, above all, the virtual absence of raw materials.

Switzerland produces barely any minerals; in the XIXth century it had no coal. It had not enough sheep, so that not even wool constituted a raw material; later it had no cotton and so on. Switzerland has always had to import all the raw materials, which is what forced her from an early period to develop to the maximum the other two essential factors of production; first capital, which she accumulated from her lively international trade and through a banking system that was established very early in Geneva, Zurich and Basle; and second manpower. In the second case quality and skill were

always more important than numbers, until the end of the XIXth century when large contingents of foreign workers were first called on for the construction of the great railway tunnels. The high level of education of the active population in that century was an important factor in the skill of the artisans and the first workers who established the reputation of the Swiss working classes. The Swiss response to competition from foreign markets was to offer a label of quality which dates back to the XVth century, when the letter G was to be found on cloths from St. Gall; now it is Tell's crossbow on numerous finished products which guarantees the label « Made in Switzerland ».

Having traced the main features of the industrial revolution in the Swiss cantons, Bergier stresses the profound changes that took place in the second half of the XIXth century. From about 1850 Switzerland lost the privileged position which early industrialization had given her, as most of her neighbouring countries were industrialized in their turn. She then found herself once again in the difficult situation which she had known before her take-off in 1800. It was the high degree of specialization in a very restricted range of goods that allowed her to surmount these difficulties. These were almost entirely luxury articles, in the production of which highly qualified labour and capital, rather than raw materials, were of paramount importance. But the circumstances which led Switzerland to the production of luxury goods also tied it to variations in fashion, attitudes and taste, and made it extremely vulnerable to shifts in the economic situation. The numerous crises of the time forced the Swiss entrepreneurs to integrate into the international system on which they were dependent. This led them to make fundamental steps in order to keep a place in foreign markets, and from these recent moves came the majority of the present-day national industries — textile products, watchmaking, machine tools and turbines, the food industry based on dairy products and certain pharmaceutical specialities which are growing in importance.

This policy called for constant perfecting of the means of production as a whole, with the double objective of continually increasing the quality of the manufactured products offered on the world market and of maintaining manufacturing prices at competitive levels. Professor Bergier describes the ways in which different branches of Swiss industry tackled these problems of survival from 1850 to the years after the Second World War. He points to the growing concentration of the major industries whenever business contracted after each period of crisis at the same time as new smaller enterprises came into being, so that although Swiss industry always appeared to be getting more concentrated it always remained reasonably diversified in fact. In the long term the problems facing entrepreneurs do not change much, and can be summarized as: markets to be won, raw materials to be acquired, fuel needs to be satisfied and the quality of the goods produced to be guaranteed. Even today these considerations remain equally valid, and the development of

Swiss industry since 1800 also gives rise to certain generalizations and themes: the degree of concentration of industrial concerns, their tendency to spill over purely national confines, the development of international companies, labour problems — both Swiss and foreign — and the varying degrees of specialization.

These two studies include a large number of tables and graphs which are accompanied by notes which help to make certain points clear. The bibliographical references are well organized, easy to use, and their analytical and chronological presentation together with the author's commentary mean that they provide an excellent introduction to the sources for a study of Swiss economic history. These two valuable studies fill a major gap in Swiss historiography, and one can only hope that they will be followed studies of living conditions and labour in Swiss industry in a future volume in the *Monographies d'histoire suisse* as Professor Bergier has suggested.

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F. BLAICH, *Kartell- und Monopolpolitik im Kaiserlichen Deutschland: das Problem der Marktmacht im Deutschen Reichstag Zwischen 1879 und 1914* [Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, Band 50], Droste Verlag, Düsseldorf, 1973, pp. 329.

In this craftsmanlike and methodical study, Fritz Blaich has explored the ways in which German industries prior to 1914 carved out for themselves, either singly or severally, positions of monopolistic and/or oligopolistic control over various segments of the economy. Cartelization of this type was not subjected to restrictions by the Reich. Indeed, in its decision of 1897, the *Reichsgericht* freely permitted cartelization. Meanwhile, however, the *Reichstag* became increasingly concerned about this situation, and, in consequence, began to formulate legislation to curb the *Marktmacht* of the cartels. This development soon led to interparty conflicts that were exacerbated even further by the activities of powerful lobbies which the industries retained in order to further their own interests. Such activities notwithstanding, it is the author's contention that the efforts of the *Reichstag* to control the cartels was at least partially successful, for by means of them, that assemblage did have a practical influence on economic policy decision-making. In consequence, the debates of its members about the matter were more than just mere "Gerede" (p. 21).

Blaich's book is divided into sections, each one of which treats of a major theme; for example, the problems of *Marktmacht* in theory and law; discussions about the problem of *Marktmacht* in the *Reichstag* from 1879 to 1914; the attitude of the political parties represented in the *Reichstag*

to the problem of *Marktmacht*; and an estimate of whether or not by its discussions and resolutions concerning *Marktmacht* the *Reichstag* was able to influence the country's *Wirtschaftspolitik*. Within this richly diverse offering of information and analysis, I found two sections of the book at once both fascinating and disappointing; namely, the one dealing with the problem of monopoly in the electrical industry, and the other concerned with the attitudes of the anti-Semites towards *Marktmacht*.

Cartelization in the electrical industry was an established fact before the turn of the century, Blaich asserts. Since electrical firms had contracted to build streetcars and municipal streetcar networks, both of which undertakings they had in every instance to finance for themselves, only those firms which were financially sound survived the crisis of 1901 which, at least temporarily, brought the construction boom to a halt. In consequence, the survivors formed the *Vereinigung deutscher Elektrizitätsfirmen*. In this conjunction, however, it was interesting to learn that one of the developments most devastating to a free market economy was the creation by the *Vereinigung* of a cartel for the submission of bids to municipal and state authorities desirous of constructing streetcar networks. Complaints by the smaller firms of the industry about the tremendous power which the giants wielded finally caused this cartel to collapse in 1909. Nevertheless, the three giants, AEG, Siemens & Halske, and Lahmeyer, concluded a secret agreement to control the market (p. 177). Although Blaich's account of these machinations is interesting, it would have been even more so had he underscored his thesis with examples based on the records of the giant firms. Why did he not use the resources of the Werner von Siemens *Institut für Geschichte des Hauses Siemens*? For that matter, why did he not cite some of the more recent monographs dealing with the electrical industry? One such work, based on the archives of the electrical firms themselves, and the first part of which contains a fine account of this industry prior to 1918, is that of Peter Czada, *Die Berliner Elektroindustrie in der Weimarer Zeit*. The examples which Blaich does cite in connection with this industry, unhappily, rely too much on secondary works and doctoral dissertations, such as that of Hans von Sothen (Freiburg, 1915).

It is regrettable, as well, that Blaich did not explore in greater depth the connection between anti-Semites like Otto Böckl and Herman Ahlwardt and the reaction of public opinion to cartelization. Certainly many of the assertions contained in Ahlwardt's *Die ertrustung Deutschland* fuelled the anti-Semitic attitudes of the middle-class, for here seemed to be ready to hand a persuasive explanation, couched in conspiratorial terms, for their own economic ills. The influence of the anti-Semites was considerable, Blaich states, for the two conservative parties made much use of this "Gedankengut" (p. 234). This is an intriguing observation, but the author does not tell us exactly how these ideas affected the discussion in the *Reichstag* about

Marktmacht, and in what ways the conservative politicians made use of these ideas. In any event, such notions must have received a friendly reception, for the conservative ideology before 1914 was well-grounded in fantasies about the nefarious actions of the *Alliance Israélite*.

This book constitutes a welcome addition to our knowledge about imperial Germany.

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M. BOGUCKA, *Nicholas Copernicus: The Country and Times*. The Ossolinski State Publishing House. Wrocław, 1973, pp. 201.

The author is professor of history at the Institute of History of the Polish Academy of Sciences, and in this study successfully portrays the great astronomer, economist and politician against the wider background of his age. She approaches this task with the assumption that the best way to understand Copernicus the scholar is through the times, environment and country in which he lived and worked. Professor Bogucka therefore discusses economic, political and cultural relations in Europe at the turn of the 15th and 16th centuries. She describes contemporary developments in detail: the new forms of the economy and government, the rise of the burghers and the increase in economic activity among the nobility in Europe. She describes in fact the economic relations which encompassed practically the entire European continent in the late Middle Ages since it is impossible to understand the economic relations of the Netherlands, England, Germany, Poland and Lithuania except against a general European background. Economic integration — as well as changes in the social and cultural structure — created a new situation in the history of the continent and was linked to the so-called « crisis of feudalism » in the 14th century. We know some of the reasons for this crisis and are even more aware of its various effects: depopulation, the decline of custom, changes in the political power structure. The existing social order was undermined. The growth of towns and a money economy as well as the appearance of a market for labour went hand in hand with the financial crisis of the feudal class thereby threatening its existing social position. The feudal class concentrated its attempts to preserve its status around the time-tested activities of war, plunder and raising the peasants' dues, or else attempted new initiatives, which included changes in the existing forms of production and also commercial activities in markets that were new in both the geographical and social sense.

Under conditions of ever-increasing political and economic chaos only successful experimentation offered a chance of success for the members of the different social groups. In my opinion, such experiments were

characteristic aspects of Europe in the late medieval and early modern period, the Europe of the Copernican age.

The search for more successful forms of action and the break with existing practices was evident in all spheres of life. This was the basis for the overseas expeditions of the Portuguese nobility, the enclosure of common fields carried out by the English gentry, the financial system applied by the merchants of Holland, the Florentine manufacturing industry and, furthermore, the formation of *folwarks* (manorial farms) on labour rent.

The author correctly draws two basic conclusions from her material. The decline of the Middle Ages was accompanied by the breakdown of values which had formed the basis of life for earlier generations. With the extension of geographical horizons, previous conceptions of the role of various states and peoples were re-assessed. This can be seen clearly in the state of which Nicholas Copernicus was a citizen. In the 14th century Poland was still a secondary European power playing a role of little significance in the political and economic life of Europe as a whole. The union with Lithuania, the recovery of Pomerania and the expansion of the Jagiellonian dynasty created a completely new political situation. The Polish state, which until the middle of the 14th century was merely a remote backwater of Europe, began to play a rather important role in European politics. Poland became embroiled in conflicts between the Hapsburgs and Jagiellonians, the Christians and Turks, the Lithuanians and Russians and between the Scandinavians and the Hanseatic League. At the turn of the 15th and 16th centuries Poland even became involved in the Franco-German conflict. Since many of the gentry participated in political decisions, in the 15th century there obviously had to be a widening of intellectual horizons in Poland. This is the significance of the fact that Copernicus spent more than half of his life in the small episcopal duchy of Warmia. Warmia was closely involved with all the activities of the Polish Crown and took part in all the decisions relating to the wider problems of contemporary European politics. One cannot discuss Warmia without reference to Prussia, Prussia without reference to Poland, or Poland without reference to Europe. In a wider sense, the land of Copernicus was Europe as a whole. The great astronomer emerged from the changing climate of his time as one of those giants of the Renaissance who cannot be historically measured in terms of one single country.

Here it would be worth mentioning Copernicus's activities in the realm of economic theory, a topic to which Professor Bogucka has, perhaps, dedicated a somewhat meagre chapter. Poland's participation in international trade led to the spread of the initial effects of the «price revolution» to the Baltic. The prevailing monetary chaos was increased by the devaluations practised by the Prussian towns, by the state of the Teutonic Knights and, finally, by the Silesian mints. The work on monetary reform was carried

out with the active participation of Copernicus (his main works were: *De estimatione monete*, 1522; *De monete cudende*, 1528) which advocated monetary revaluation (i.e. raising the value of silver coins) and the stabilization of better money. The effects of the deterioration of the quality of coins were analysed in a very interesting manner, revealing that their consequences were social polarization. In his economic works, Copernicus formulated the principle that if monomial currency of superior and inferior value were in simultaneous circulation, the latter would begin to squeeze out the "better" (i.e. superior) coins since these would either be reworked or exported abroad. The same principle was later formulated by T. Gresham. As supporter of the "substantial" (or metal-based) theory, and an opponent of nominalist views, Copernicus proposed the regulation of the number of mints, the establishment of a single, state value for currency and the withdrawal from circulation of old currency which only caused economic losses to great masses of the population.

Professor Bogucka's book does however deal comprehensively with the cultural changes which took place during this period and which created a suitable climate for Copernicus' basic discovery concerning the heliocentric arrangement of the solar system (the last version: *De revolutionibus orbium coelestium*, 1543). During the 15th and 16th centuries it became necessary to seek new and more beneficial forms of activity, one of which was a reliance on science. The intellectual turning point which the author emphasizes was tied to a real advance of science in Poland. The development of parish schools (it is estimated that in the 15th century the Crown had one school per 1,000 persons) provided a base for the formation of a unified national culture. In the 15th century Poland found herself in a situation in which science became a necessity for everyday life.

This general situation provided the background to Copernicus's discovery. He was a genius superior to the average level of the intellectual elite of his time. But one must also remember that this elite was numerous and, more important, grew out of the wider ranks of educated men. Thanks to science, the 14th and especially the 15th century brought opportunities for social advancement. Throughout the country this social advancement was made possible by the most prominent representatives of science who were highly esteemed and recognized by the ruling elite. The rise of the Cracow Academy is closely related to the participation of its professors both in public and court life and in the politics of the entire state.

The Cracow Academy was also the centre through which scientific and cultural innovations from Europe were transmitted to Poland, although such innovations also reached the country to an even larger degree through professional and diplomatic contacts. New fashions and new ideas ceased to be the domain of a select few. The innovations were accepted and modified by numerous people in Poland who sometimes derived direct

benefit from them. The flowering of science and culture in the 15th and 16th centuries was a result of this development.

Can we derive any conclusions about Copernicus's cosmopolitanism from Professor Bogucka's book? He was a member of the international republic of scholars, but he was also a son of Royal Prussia whose political, economic and cultural situation defined both the limits and interests of this great scientist.

Professor Bogucka, by portraying the life of Copernicus against the background of the complicated changes of the age, has afforded us a better understanding of this great astronomer's work. Through the prism of Copernicus's life she also shows us the origin of modern European society. Published to commemorate the Year of Copernicus which was proclaimed by the United Nations, this book is one of the more successful works dedicated to the great Copernican anniversary.

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Translated by *Saulius Sužiedelis*

C. M. CIPOLLA (ed.), *The Fontana Economic History of Europe*, Volume III, *The Industrial Revolution*, 1973, pp. 624; Volume IV, *The Emergence of Industrial Societies*, Parts 1 and 2, Collins/Fontana Books, London and Glasgow, 1973, pp. 845.

These are the third and fourth of a planned six-volume series on the economic history of Europe from the Middle Ages to contemporary times. Each volume, with an introductory chapter by the editor, Carlo M. Cipolla, is composed of a series of essays written by established scholars specially commissioned. In addition, the publisher is bringing out each essay as a separate pamphlet. The editor chose the subject matter of the chapters so as to cover adequately the major areas of traditional concern.

Volume III, *The Industrial Revolution*, covers approximately the period 1700-1914 and contains Professor Cipolla's introductory overview and chapters on Population in Europe (André Armengaud), Patterns of Demand (Walter Minchinton), Technological Progress and the Industrial Revolution (Samuel Lilley), Banking and Industrialisation in Europe (Bertrand Gille), The State and the Industrial Revolution (Barry Supple), The service Revolution: The Growth of Services in Modern Economy (R. M. Hartwell), The Industrial Bourgeoisie and the Rise of the Working Class (J-F. Bergier), Agriculture and the Industrial Revolution (Paul Bairoch), The Emergence of Economics as a Science (Donald Winch), and Industrial Archaeology (M.J.T. Lewis).

Volume IV, published in two parts, is *The Emergence of Industrial Societies* and, except for an essay on the international economy, focuses on specific countries. In addition to Professor Cipolla's introduction, it contains chapters on Belgium and Holland (Jan Dhondt and Marinette Bruwier), England (Phyllis Deane), France (Claude Fohlen), Germany (Knut Borchardt), The Hapsburg Empire (N.T. Gross), Italy (Luciano Cafagna), Russia (Gregory Grossman), Scandinavia (Lennart Jörberg), Spain (Jordi Nadal), Switzerland (B.M. Biucchi), *The Emergence of an International Economy* (William Woodruff), and a Statistical Appendix (B.R. Mitchell).

Taken as a whole, these are welcome additions to our stock of literature on Europe's economic history; in general, the individual authors have written their pieces with care and have maintained a high level of historical scholarship. However, the work should be considered as supplemental material for teaching purposes rather than as a basic text. As with all anthologies of such nature, this series lacks the necessary internal cohesion, that unity of vision and necessary continuity of analysis, to stand alone as an explanation of Europe's economic past. Each author quite inevitably does his own thing without reference to, or consideration for, the arguments of other participants in the venture.

While the chapters themselves remain analytically unconnected, so the individual essays often too largely consist of descriptions of conditions and events rather than analytical, empirically supported explanations of events.

As an example, André Armengaud's essay on Population in Volume III (Chapter 1, pp. 22-73) is an informative description of demographic changes; but, other than suggesting that population growth increased the number of producers and consumers, the author does not employ economic theory to interpret the data and circumstances he presents. It is trivial to say that a larger population will lead to larger output and consumption — such must be the case, unless the marginal products of enough of the added workers are negative. If indeed we are concerned about the economic impact of changes in population (aside from explaining why the rate of change of population varied), we should pose our questions in terms of effects on per capita output. What are the implications of an absolutely larger population? (E.g., population rises relative to other resources, and, barring changes in technology or an increase in the quantity of land or capital equipment, average wages of labour are adversely effected.) What are the effects of a change in the rate of growth of population? (E.g., a faster rate has certain implications with regard to maintaining or increasing the amount of capital equipment per worker.) And what are the effects of changing age distributions of a population? (E.g., if the birth rate increases, and there is no change in death rate, the proportion of the population in the pre-labour force age bracket rises, and this fact has implications for economic change.) Such kinds of questions one would like to see discussed in an economic history.

While description of economic circumstances is necessary, it is not sufficient, for our understanding of the economics of Europe requires, in addition, the careful application of our profession's analytics.

Another example of explanatory deficiency can be found in Bergier's essay, « The Industrial Bourgeoisie and the Rise of the Working Class 1700-1914 » (Chapter 7, pp. 397-451). Bergier discusses the ascent to political power of the bourgeoisie and the formation of the working mass — that is, the creation of the capitalist class and the proletariat. Objective, precise application of economic analysis is lacking. Several quotes will illustrate. « Woman and child labour all the year round in conditions almost identical to those *inflicted* on adult men remains the *great moral scandal* of the industrial revolution — most workers' wives worked, which was a *pretext* for the employers to lower wages, considering the family was no longer kept on a single wage... As for the children, who were tied to a working day of ten to twelve hours from age of three (sometimes), or five or six (often), they could not receive any schooling likely to befit them for responsibility (p. 431) ». « A few entrepreneurs (Wedgwood, or the Mulhouse spinning magnates, and others) tried to treat their workers more *humanely*. They paid *decent wages*, built better houses and schools, encouraged certain collective leisure pursuits, etc. This was paternalism. But at the outset... paternalism was the nearest the bourgeoisie got to an enlightened social attitude. The motives of its practitioners were not wholly *pure*, in any case, and evolved as time went on, even if the end result remained the same. In the early days... paternalism drew its inspiration from the humanitarian and progressive outlook of a minority of clear-sighted entrepreneurs; at the same time, it did not exclude the profit motive... (pp. 435-36) ». The italics above are the reviewer's. The imprecision and emotional tone of the language and the passing of poetic judgement on the actions or conditions of participants are frail substitutes for dispassionate economic inquiry, if one has in mind the imparting of a better understanding of events.

Volume IV, which concentrates on the experiences of various countries, suffers less from this defect. Particularly well done are the essays of Gregory Grossman (« Russia and the Soviet Union », Chapter 8, pp. 486-531) and Jordi Nadal (« Spain, 1830-1914 », Chapter 9, pp. 532-627).

In sum, these two volumes contain useful essays on European economic history from about 1700 to 1914; but often they lack economic analysis and, as a whole, do not provide a coherent account of the economic experience of the period. Rather, they represent a series of independent, fairly specialized essays (not, however, without overlapping elements) strung together without a central interpretative theme for guiding the reader through, and making sense of, the substantial description of economic facts and circumstances.

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R. B. DAY, *Leon Trotsky and the Politics of Economic Isolation*, Cambridge: Syndics of the Cambridge University Press, 1973, pp. x, 221.

As a revival of interest in the diverse socialist heritages, several volumes examining Trotsky's political and economic thought have recently appeared in print. Professor Day's¹ book stands out among them as an outstanding appraisal of Trotskyism. In this thoroughly researched monograph (based on previously unknown papers in the Trotsky Archives deposited at Harvard University), the reader will find a challenge to both the "Trotskyite" and "Stalinist" interpretations of the events in USSR from 1917 to 1927. The author argues that in their attempts to interpret the Trotsky-Stalin dispute concerning the question of "Permanent Revolution and Socialism in one Country", historians have raised the wrong questions and offer inadequate interpretations (p. 4). He argues that a reappraisal of Trotsky as a politician and a Marxist theorist is essential, and advances his own thesis that the operative question for Trotsky was not *whether* Russia could build socialism in advance of international revolution (as interpreted by Deutscher, Lichtheim, Nove and others) but *how* to devise an optimal planning strategy, taking into account both the existing and the future international division of labour.

In this reviewer's opinion, despite its controversial nature, the merit of Professor Day's hypothesis is that it stresses empirical aspects rather than theoretical postulates. Likewise, his analysis of War Communism (1918-1921) and the NEP system when the debate erupted between Trotsky and Stalin over the concept of Socialism in a separate country are very original.

Trotsky's feud with Stalin, says Professor Day, was only tangentially connected with the theory of Permanent Revolution. This theory, in his view, was irrelevant to the economic debates of the mid-1920's on two grounds. In the first place, it was concerned with the seizure and maintenance of political power and not with the pragmatic problem of building socialism. Secondly, the theory claimed the inevitability of an international revolution. Trotsky did not argue the impossibility of socialism in one country, but merely that "Russia was prepared for the revolution and that proletarian aid from the West would eliminate the danger of counter-revolution—nothing more" (p. 10). Yet in 1930, after his exile, Trotsky claimed that he had anticipated Stalin's errors and that he had not believed Russia was ripe for socialism without international revolution. According to Professor Day, this latter Trotsky's position was an attempt to create a political myth for purposes of anti-Stalinist propaganda (pp. 10, 11). Although these and many other

¹ Professor Day is an Assistant Professor of Political Economy at the University of Toronto. Published by the Cambridge University Press (in its Soviet and East European Studies Monograph Series), the book was written under the auspices of the Centre for Russian and East European Studies at the University of Toronto.

disputes are examined mainly from Trotsky's viewpoint, extensive references are made to Lenin, Bukharin, Sokol'nikov, Preobrazhensky, Zinoviev and Stalin.

In the main, the book concentrates on the economic implications of the restructured empire's isolation from Europe. The coverage is broad, as may be seen from the topics the book is made up of two parts, together eight chapters. Part I — The Dilemma of Economic Isolation (chs. 1 through 3), treats such topics as the Myth of Trotskysm, Isolation and the Mobilization of Labour, and Integrationism and the New Economy Policy. Part II — The Politics of Economic Isolation (chs. 4 through 8), deals with such topics as The Search for a New Faith, Socialism in One Country, Trotsky's Attack on Socialism in a "Separate" Country and Integrationism in Defeat and Exile.

The first three chapters examine the isolationism of War Communism and the integrationism of the early New Economic Policy (NEP).² These two opposing international outlooks with their complex economic implications were at the base of political conflict during the early years of the USSR. This reviewer is in agreement with Professor Day's assertion that although a number of excellent works have been devoted to the political history of the 1920's, none has analysed the material in terms of this economic dichotomy. For that reason none have managed to explain satisfactorily either the origins or the meaning of Trotsky's conflict with Stalin (p. 69). In a sense, Dr. Day confirms the Bolsheviks' own dictum that "politics is concentrated economics." Thus he argues on the basis of extensive empirical evidence that "if Trotsky's behavior during War Communism had been consistent with the interpretation normally placed upon the theory of Permanent Revolution, he would have fallen into the integrationist category. Instead, he emerged as the central theorist of economic isolation. Apparently, Trotsky saw in War Communism a coherent system of policies intended to achieve a direct transition to "Socialism in One Country", and only during NEP he became a consistent proponent of integrationist policies. The book analyses Trotsky's position on the policy of the new socialist state towards the capitalist West in relation to his theories of the market and imperialism. The volume concludes with a brief account of the evolution and rationalization of Trotsky's thought during his life in exile.

Professor Day's mastery of the relevant literature, his sharp eye for the revealing detail, and his vivid and lucid style deserve high praise. The monograph will be of value to those interested in the history of Marxism, Trotskyism

² "Isolationists" tended to look upon USSR as an exile from the world economy and "integrationists" believed that despite the country's unique political order, Russia must resume her previous position in international affairs.

and the USSR, as well as in political economies of socialism in general. Nonetheless, the reader should beware of incorrect and at times confusing use of the term Soviet Russia instead of Soviet Union; similarly, his term "New Economy Policy" rather than "New Economic Policy" is clumsy.

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J. DE VRIES, *The Dutch Rural Economy in the Golden Age, 1500-1700*, New Haven and London, Yale University Press, 1974 (Yale Series in Economic History).

The spectacular revival of the northern Netherlands and particularly of Holland at the end of the 16th and during the 17th centuries has always strongly fascinated historians. Until quite recently, however, most of the research was essentially concentrated on the secondary and the tertiary sector. Exceptionally, some attention was paid to the agricultural changes during this period. J. de Vries fully deals with these changes and shows that they were inherent in, and in many respect even at the basis of, the economic revival.

We can consider the first chapter an innovation since it deals with two theoretical models ("peasant model" and "specialization model") investigating the extreme values of the developing process in rural areas considering a very sharp population growth. Throughout the following chapter these models are thoroughly tested by means of financial, judicial and notarial archives. The problems is dealt with in detail, especially concerning the maritime provinces of Holland, Friesland and Groningen and the western part of Utrecht (characterized by an alternation of clay and peat soils). In the introduction the social contrasts at the end of the Middle Ages are exhaustively discussed. It was very typical that in these provinces farmers owned a very important amount of land (especially in Holland where the average was 42%). However, some important regional contrasts were taken into account. On the North Holland isles of Texel and Wieringen almost the entire area belonged to the farmers, whereas on the South Holland isles only 8 to 10%. In the latter case the soils were of significantly poorer quality (« poor sand and peat soils and little of the more valuable clay soils »). This also contributed to marked social distinctions even within the lower class. Therefore, the author can state, concerning the situation around 1500, that: « it is the pronounced inequality in economic resources that impresses itself on the observer » (p. 67). A further increase of the social differentiation can be related to the sharp population growth in the 16th and the first half of the 17th century and especially in the Zaan area, central Holland, Ablasserwaard-Krimpernerwaard, the "vlekken" in Friesland and the "veenkolonieën" in Groningen.

Important adaptations in the rural sector seemed the more necessary if one considers the enormous import of grain from the Baltic countries. Furthermore, it should be noted that urbanization influenced the growing demand for vegetables, meat and dairy products. This is clearly shown from the evolution of prices during the period of the so-called price revolution. In contrast with other areas, we note here the highest rises in price for meat and dairy products!

Finally, the author deals with the sharp rise in rents and the increasing taxes, a consequence of frequent dyking-in and draining works. This caused the commercial extension of the primary sector. From all this the author concludes: « the rural economy moved more rapidly away from autarchy. Developments in the rural economy imposed a "pro-trade bias" ». Moreover, these changes caused maximal employment in a number of allied trades ("specialization model"); this situation was reinforced by good prospects causing the start of new projects (mostly with speculative ends, and amongst others financed by the bourgeoisie).

This favourable trend in the trade cycle eventually came to an end in the late 1600s and certainly died after 1660-1670. Subsequently, there were no more investments in the primary sector and a number of farms were even abandoned. In this connection one could ask whether some generalizations, as proposed by the author, (e.g. the consolidation of trades) are not rather the consequence of the new period which then occurred? As a matter of fact insufficient quantitative data were included and, when present, referred to the late 17th and the early 18th centuries, when the unfavourable economic trend was already felt and when especially farming had reached its low-point. The few indications for the 16th and the early 17th centuries seem already to show a (temporary) parcelling of farming land.

However, these remarks are of minor importance when considering this study. We would stress once more the tremendous merits of the author, who completely succeeded in situating more clearly the basic role of agriculture in the so-called "golden age".

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R. FLOUD (ed.), *Essays in Quantitative Economic History*, Oxford, Clarendon Press, 1974, pp. 250.

Although the use of quantitative methods in economic history is as old as the discipline itself and has increased dramatically in the last two generations, economic historians continue to have an arduous and haphazard task introducing themselves to these valuable tools. This is especially true for those economic historians who receive their professional training in history,

rather than economics. First, there are practically no texts which focus on the unique problems of quantification with historical evidence. Second, examples of the proper use of quantitative methods in a historical setting are quite scattered; finding an essay utilizing a potentially helpful technique is often akin to a game of roulette at Monte Carlo, past or present. Recently, several volumes have appeared attempting to rectify the first problem, including a fine introductory text by the editor of the present volume. Now, in the volume under current review, Floud has assembled for the Economic History Society an excellent selection of essays on quantitative aspects of British economic history with the essays chosen explicitly to illustrate quantitative methods with powerful and wide application.

The volume begins with a lucid introduction to the tools of quantitative analysis written by the editor. As should be the case with quantitative methods in a historical context, stress is placed on descriptive statistics. Brief, clear explanations are given of frequency distributions, measures of central tendency, measures of dispersion, measures of association, simple and multiple regression analysis, and measures of change over time. A section on statistical inference concludes the introduction. Importantly, Floud stresses that the assumptions which must be met in order to utilize statistical methods to infer population characteristics from a sample are quite restrictive and not always met by samples of historical data. Another neglected point noted by Floud is that historians often have data on entire populations. Thus, use of the theory of statistical inference to assess the "significance" of their descriptive statistics is inappropriate. What makes this introduction especially valuable is the extensive cross-referencing between its topics and the passages in the assembled monographs where the various techniques are illustrated. The topics of the introduction contain frequent citations to the monographs and each monograph is preceded by references to Floud's introduction and other texts where the fledgling quantifier can find a fuller treatment of the monograph's methods.

The principle guiding the selection of the eleven monographs was that they illustrate clearly the application of important quantitative tools. For the most part it must be judged that Floud's choices serve this goal admirably. Ames's monograph on the 1337-9 sterling crisis neatly demonstrates some simple uses of frequency distributions, contingency tables and statistical inference. Ohlin's essay on medieval demography affords an excellent introduction to some of the pitfalls of sampling. Two measures of association, Spearman's rank-order correlation coefficient and Pearson's product moment correlation coefficient, are displayed well in Schofield's piece on the geographic distribution of wealth in England, 1334-1649. Cooper's essay on the social distribution of land and men in England, 1436-1700, and Felix's critique of the relationship between early modern profit inflation and industrial growth, illustrate effectively the old adage that « an average is no better

than its variance », or perhaps more directly, averages often hide a considerable variety of experience which historians ignore at their utmost peril. Soltow's brilliant essay on long term changes in British income inequality is included in the volume, offering one of the best introductions to the measurement of income dispersion available anywhere. The benefits of separating trend from cyclical behaviour in time series are demonstrated in an essay by Adie on English bank deposits before 1840. A monograph by Sandberg on the diffusion of ring spinning in America and Britain, and one by Matthews on British post-WWII economic growth in the light of its economic history since 1870, serve as good examples of measurement in combination with the tools of economic theory.

Two monographs are offered to introduce the reader to correlation and regression analysis but, at least in this reviewer's opinion, they are inadequate examples. In Olson and Harris' essay on prices and production of British wheat, 1873-1914, and Ford's on British investment in Argentina, 1880-1914, correlation and regression analyses are employed to determine the relative strength of a number of independent variables in accounting for the variance of a dependent variable. For Olson and Harris the dependent variable is British wheat acreage and for Ford it is the flow of British investment to Argentina. In both monographs the strength of each independent variable appears to have been judged, either explicitly or implicitly, by its addition to R^2 , a correlation coefficient which measures the extent to which a given set of independent variables account for the variance of the dependent variable. The difficulty with this technique is that a great deal depends on the order in which the independent variables are added to successive regressions. A better measure of the relative contribution of an independent variable is its beta-coefficient, that is, its regression coefficient normalized by its variance and the variance of the dependent variable.

For example, Ford first regresses Argentine railway profits (P) on the flow of British investment to Argentina (A) and in one version obtains an R^2 of .650. He then regresses both Argentine railway profits (P) and new overseas issues of the London Stock Exchange (F) on the flow of British investment to Argentina (A) and the R^2 rises to .788. Now it may be inferred from this progression that F was less important than P in accounting for the variance of A, especially because Ford then introduces other independent variables in which the increment to R^2 declines progressively and these are claimed as weaker influences on A. Such an inference, however, would be incorrect. Using the data supplied by Ford, it can be shown that if Ford had regressed F alone on A, the R^2 would have been very close to .650. If beta-coefficients had been employed instead, thus avoiding the pitfalls of which variable to introduce, first, second, and so forth, it would have been immediately obvious that P and F possess roughly equal « explanation power ». In sum, incremental R^2 's are often poor guides to an independent variable's

relative contribution and Floud seems to overlook this. Beta-coefficients are probably a superior instrument for this purpose. Despite this problem, both essays contain interesting insights into their respective subjects.

Floud deserves considerable thanks from economic historians. His tightly organized introduction is a pleasure to read and the illustrative monographs he has selected are, for the most part, excellent examples of quantitative technique and outstanding in their own right.

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A.C. KELLEY, J.G. WILLIAMSON, J. CHEETHAM, *Dualistic Economic Development: Theory and History*, University of Chicago Press, Chicago, 1972, pp. xi, 399.

The authors, later referred to as KWC, describe themselves as « teachers and students of growth economics, economic development, and economic history (who) have been continually frustrated by the lack of an integrated treatment of the sources and determinants of economic development », and their purpose in writing this book was « to provide a treatise on economic dualism in theory and history ». The task is basically well conceived, with different aspects of the dualistic economy described in the framework of a series of related and progressively complex neoclassical models in the tradition of Uzawa and Jorgenson. The principal models are tested against the Japanese post-Restoration growth experience, and discussions about the choice of initial conditions and parameters for these tests provide the opportunity to draw extensively on the literature on development in other countries.

The view of production in the dualistic economy differs from the earlier neoclassical description of Japanese growth by Jorgenson in two respects. First, the authors use CES production functions, and secondly, both sectors are dependent on inputs of efficiency capital and labour, thus allowing for intersectoral differences in the elasticity of factor substitution. The elasticity of substitution of efficiency labour for efficiency capital exceeds unity in the traditional sector, and is less than unity in the modern sector, assumptions that are well supported by the empirical literature.¹ On the other hand, in view of Nakamura's estimates of the contribution of land to the growth of agricultural output in Japan, its omission from the inputs to agriculture might seem inadequately defended and the implications of this choice to be insufficiently explored.

¹ See also, C.K. CLAGUE, « Capital-Labor Substitution in Manufacturing in Underdeveloped Countries », *Econometrica*, 37 (July 1969), pp. 528-537.

Attempts to explain intersectoral flows of capital and labour are an important feature of the work. The migration of labour is explained via the customary wage differential model and a cost of migration. A similar model is used in conjunction with putty-clay assumptions to explain intersectoral capital flows through the agency of a capital market (or other financial intermediaries). Relative wages and rentals, and hence the distribution of income and the flows of labour and capital between sectors, are determined by the factoral marginal products in the traditional neoclassical manner. These assumptions might be questioned on at least two grounds. First, is there a measure of capital stock such that its marginal product is related to its price? The recent position of the lengthy Cambridge (U.S.)-Cambridge (England) debate is that although the anticipated marginal product of capital explains desired increases in the capital stock at the micro level we cannot meaningfully construct a long-run aggregate stock series for this purpose since this procedure involves valuation of capital stock and hence circularity in the argument. The question of why aggregate capital seems to explain distributive shares is still open.² Similar problems are met in relation to the labour stock. Secondly, even if profit-maximizing equilibrium factor prices are determined by the marginal products of stocks somehow defined, do entrepreneurs truly maximize profits with respect to factor payments, or do they do so only subject to ancillary constraints on the levels of such payments? As described by Lockwood, the Japanese labour market was characterized by a rather narrow spread of wages between sectors and chronic labour shortages in the modern sector, associated with poor working conditions and a consequent high turnover of the labour force.³ This evidence suggests that actual wages,

² FRANKLIN M. FISHER, « The Existence of Aggregate Production Functions: Reply », *Econometrica*, 39 (March 1971), p. 405.

³ MIYOHET SHINOHARA (*Structural Changes in Japan's Economic Development*, Tokyo: The Kinokuniya Bookstore Co., Ltd., 1970, p. 24) has also noted the late development of the dual structure of wages in Japan, largely following reforms in labour legislation, with monetary differences appearing only during the 1920s. Even then the dual structure was associated with low wages in the modern sector (large firms), but greater security, guaranteed salary progression, and some compensatory real fringe benefits in the form of housing and the like. JAMES C. ABBEGGLEN *et al.* (*Business Strategies for Japan*, Tokyo: The Voyagers' Press for Sophia University, 1970, ch. 1) have emphasized the cost benefits which accrue to rapidly growing industries through progressive salary scales based on seniority and starting from a low base. It is not clear, therefore, that at any time during the Meiji and Taisho eras, it could be said that gross wage and non-wage compensations in the modern and traditional sectors were related to differences in labour productivity. KOZO YAMAMURA (« Wage Structure and Economic Growth in Postwar Japan », *Industrial and Labor Relations Review*, 19 [October 1965], pp. 58-69. Also see SHINOHARA, *op. cit.*, ch. 8) who has indicated a recent erosion of the dual wage system which may possibly be leading to a more neoclassical structure.

at least in manufacturing, were substantially below the marginal product of labour and were in fact determined by the "institutional" wage level of the traditional sector. As the proportion of the labour force in the modern sector grows, the implied distributional bias would affect both the structure and magnitude of demand, and cause the share of the modern sector in total output and the rate of growth of output to be biased downwards.⁴ Thus, while I do not reject the neoclassical distributional assumptions as a basis for model building, having, indeed, made use of them myself, I do consider that both analytical and historical perspectives require a more extensive discussion of these issues than is provided.

Chapter 4 contains the results of test simulations on the simplest of the simulated models, based on a set of parameters and initial conditions considered to be representative of a developing economy. The simulated rates of growth of total output are in good agreement with actual rates except for the case of Japan. KWC infer that the discrepancy in the Japanese case is due to the use of a population growth rate much higher (by a factor of three) than actually occurred; and draw the conclusion, in agreement with the earlier findings of Lockwood for Japan and Robbins for the United Kingdom, that economic growth is stimulated by low rates of population growth.⁵

Later chapters contain developments of the basic model, including the descriptions of capital and labour migration, and simulation results for these versions. Since these models are evaluated against the Japanese growth record one might expect that the parameters and initial conditions used in their simulations would have been chosen accordingly. Unfortunately, the authors chose not to do this but, instead, to retain the original parameters and "adjust" the time scale of the simulations to achieve the desired agreement between actual and simulated rates of growth of output, making two simulation periods equal one year of real time. In doing so, KWC have implicitly doubled all rate-of-growth parameters in the model, for population and both capital and labour augmenting technological change, and this in spite of the earlier diagnosis that the rate of population growth used in the simulations was too high. In consequence, it is difficult to make any meaningful interpretation of the simulation results. One can sympathize with the authors' desire to have a complete set of models simulated with common parameter values. But a better procedure would have been either

⁴ WILLIAM W. LOCKWOOD, *The Economic Development of Japan*, Princeton: University Press, 1954, p. 273.

⁵ WILLIAM W. LOCKWOOD, *op. cit.*, p. 161. LIONEL ROBBINS, «The Dynamics of Capitalism», *Economica*, 6 (March 1926), pp. 37-38, cited by Lockwood, *ibid.*

to choose another country for the quantitative comparisons throughout the book or to add a section to chapter 4 based on a resimulation of the model with parameter values appropriate to Japan.

The reader may be disposed to accept, with qualifications, the general qualitative results described in these later chapters, but if so must do so circumspectly. For example, in the discussion of the simulated capital markets, the intersectoral flows of savings are found to be quite small and insensitive to other parameters, a result « somewhat at variance with much of the literature dealing with the role of the capital market in development, where great stress has been placed on the advantages to be gained from "improved" capital markets » (p. 281), and with apparent policy implications. But the parameters that control the intersectoral flows of savings were chosen to achieve precisely this result (p. 364), which is therefore a consequence of the choice of parameters and not of the economy described by the model. Further, while it may be true that intersectoral flows of savings from the traditional to the modern sector, measured as percentages of total savings in the traditional sector, have not been large in most developing economies, the absolute amounts may be important at an early stage of development. Since the model only describes the effects of capital markets in terms of facilitating intersectoral flows of savings, and then only for an economy in a relatively advanced state of development, it is not equipped to provide useful policy guides with respect to the overall utility of financial intermediaries for developing countries.

The underlying purpose of this book is a good one. Unfortunately its implementation has been less satisfactory. In addition to the problems already covered there is, for example, no discussion of steady-state paths for any of the models. In fact this topic is explicitly rejected as uninteresting by the authors, who nevertheless provide a very detailed discussion of the existence, uniqueness, and stability of a solution to a preliminary model which is nowhere compared with actual data. The balance, integration and organization of the material in the book leaves an overall impression of too-hasty preparation. Typical of the organizational problems is the inclusion of equation summaries and parameter estimates of the less important models in the text, while the corresponding material for the other models is relegated to appendices or not included at all. There are in addition a number of typographical errors and at least one inaccurate bibliographical reference, though I have not made an exhaustive search in this direction.

After considerable revision, this book could be an important contribution to the development literature either as a text or as a research monograph. Two of the authors (Kelly and Williamson) are currently engaged in preparing a new book Japanese economic history. Hopefully, they will overcome some of the problems of the book under review.

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CH. P. KINDLEBERGER, *The World in Depression, 1929-1939* (History of the World Economy in the Twentieth Century, volume 4), Berkeley and Los Angeles: University of California Press, 1973, pp. 336.

Professor Kindleberger inquires why the depression of the 1930s was so wide, deep, and long. He finds the key in his specialty, the international monetary mechanism. Specific disturbances were not the main trouble. Rather, the system was basically unstable; and no country was furnishing the necessary stabilizer. Doing so would have involved: (1) Maintaining a market for distress goods (instead of warding off cheap imports); (2) Providing counter-cyclical foreign lending; and (3) Discounting in crises; i.e., giving ample financial aid to countries whose currencies were in danger of coming unhinged from their fixed parities. (Kindleberger does not consider whether the gold-standard façade of pegged exchange rates was itself an element of instability.) Before World War I, Britain had underwritten the international system, in part by expanding its foreign loans when recession at home was tending to cut imports (and cutting back on lending when investment was booming at home). In the United States in the 1920s, however, foreign lending boomed along with domestic investment; and when depression cut down on imports, the earlier outflow of America capital stopped and shifted into reverse. Experts debate the size of the resulting shock, but « If the system is basically unstable, this issue is downgraded in importance ». By that time, anyway, the British were unable to continue their earlier central role; and the United States was unwilling to take over. (They silver-purchase programme serves as « a brilliant example of world economic irresponsibility on the part of the United States ».) Kindleberger likens a financial system with two or more leaders, or with leadership shifting, to a car with two drivers, one growing weak at the wheel while the other lacks enough interest to take over. « The main lesson of the inter-war years . . . [is] that for the world economy to be stabilized, there has to be a stabilizer, one stabilizer ».

Kindleberger contrasts his diagnosis of the 1930s both with the view (attributed to Paul Samuelson) that the depression stemmed from a series of historical accidents and with any dominant-cause interpretation such as the monetarist theory of Milton Friedman and Anna Schwartz. He notes that the decline in the U.S. money supply was mild until March 1931, whereas velocity fell 13 percent between 1929 and 1930. The decline in prices and relapse of long-term foreign lending in the spring of 1930 were critical, he says, because they led to the international crisis of 1931; and although he cannot explain them, he is confident that « they had nothing to do with the money supply ». He notes the differential behaviour of net excess and net borrowed reserves at central-reserve-city, reserve-city, and country banks from 1929 to 1933, implying but not really arguing that the figures somehow discredit « simple monetary theory suggesting that business conditions ought to follow the money supply . . . ». One wonders just what facts, figures, dates,

and countries he had in mind when writing that if Friedman and Schwartz had « not focused so exclusively on monetary conditions in the United States, [they] might have noted the accentuation of the depression which came with the transfer of the presidency from Hoover to Roosevelt (occurring after the money supply had been greatly enlarged); and the still more significant (in my judgement) transfer of leadership in the world economy from Whitehall to the White House ». After mentioning the steepness of the 1937-38 recession, Kindleberger commends to monetarists a passage quoted from the *Federal Reserve Bulletin* of June 1938. It is not clear whether he expects the monetarists to take the passage as evidence against their theory or as reinforcement of their dismay at the theoretical notions then underlying monetary policy. Anyway, according to the Federal Reserve, « 1937 taught us that an abundant supply of gold and a cheap monetary policy do not prevent prices from falling ».

While Kindleberger mentions a number of facts and episodes that are compatible with and that even appear to illustrate what he says about inherent instability and lack of leadership, he presents little evidence and reasoning to rule out other explanations of the depression. Nor does he even argue explicitly that the others stand in rivalry with his own. Quite conceivably, historical accidents and lack of leadership might have contributed to cumulative monetary contraction. He does note that « Everywhere except France money was tight » in 1929 and, promising to « cite instance after instance of what, with hindsight, appears as economic illiteracy », does note that « Deflationists are found everywhere . . . ».

Kindleberger mentions many interesting or amusing facts and episodes, including « puerile central-bank quarrels » involving the heads of the Banks of England and France and of the Federal Reserve Bank of New York. German Chancellor Brüning, « with tears in his eyes », had to turn down (presumably in 1931) a foreign rescue loan for the Danat Bank for fear — as later revealed — that a mysterious bad loan from the bank to the son of President Hindenburg would come to light. In April 1933, on hearing that President Roosevelt had decided to accept the Thomas (Inflation) Amendment then pending in Congress, Budget Director Lewis Douglas commented, « This is the end of Western civilization ». Kindleberger makes good use of memoirs and other original sources in English, French, and German. The book has a valuable bibliography.

Emphasis and arrangement are uneven, however. Unavoidably, perhaps, the book falls between the two stools of a chronological narrative and an analysis arranged by topics.

The closing page makes recommendations. Instability would result if the United States and the European Economic Community were to compete for economic leadership, or if one proved unable and the other unwilling to lead, or if each retained a veto over the other's programme without pushing one of its own. Any of three other outcomes could be stable: continued

or revived U.S. leadership, European leadership, or the lodging of effective economic sovereignty and authority with international institutions. Kindleberger evidently prefers the last solution but sees difficulties in the way.

The most charming part of the book, in my opinion, is its autobiographical preface. The depression is where Kindleberger came into economics; his book is largely « *a recherche du temps perdu* ». While a college student in 1930 he got a summer job (through nepotism, engagingly admitted) on a freighter plying between Copenhagen, Leningrad, and three Finnish ports. From that experience dates his lifelong interest in international trade and taste for Tuborg beer. As a Keynesian, he looks back ruefully on serving in the spring of 1932 as office boy with the National Economy League. In preparation for graduate study, he took an evening course in money and banking in February and March 1933, when the instructor, a financial editor, gave day-by-day accounts of the collapse of the American banking system. A term paper of 1933-34 on competitive exchange depreciation between Denmark and New Zealand resulted in his first professional publication and in his lifelong « prejudice against flexible exchange rates ».

One can question the main thesis and some minor aspects of the book (including excessive personification of countries, as well as some editorial oversights). Still, the book does enhance one's impression of Charles Kindleberger as a humane person and a scholar of broad competence.

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J. R. MANDLE, *The Plantation Economy: Population and Economic Change in Guyana, 1838-1960*, Philadelphia: Temple University Press, 1973, pp. xv, 170.

These precise, highly condensed 144 pages offer a clear and provocative interpretation of the economic history of one colony in the modern British Empire. It begins at a moment of crisis for the sugar-exporting, slave-based plantation society of British Guyana: British abolition of slavery in 1838. Between then and 1850, an estimated 75 per cent of the former slave population either moved off the plantations or died. Such an exodus called into question the very ability of the plantation colony to survive.

The concept « plantation economy » comprises a basic reference point throughout the book. Professor Mandle makes two essential points about plantation economy insofar as it was exemplified in colonial Guyana. First, as a mode of organizing productive activity, plantation economy fits neatly into neither the conventional « feudal » nor « capitalist » models. Directly

dependent upon and geared toward the international market, the plantation owners organized the larger society to provide labour cheaply and plentifully. The resulting social relations approximated, in Mandle's words, to those found in mental institutions. « Inmates » and « staff » are the analogues of plantation workers and plantation owners/managers.

Second, Mandle demonstrates that the plantation economy survived several profound threats to its existence. The owners' close links to British colonial authorities won official support for the plantation as the basic element of the Guyanese economy. Furthermore, the plantation, as a productive unit, proved remarkably adaptable to changing circumstances. Thus, Mandle organizes Guyana's economic history as a sequence of types of plantation economy.

Plantations survived abolition by substituting indentured labourers who were recruited chiefly from India. Thus began the socio-economic differentiation between African ex-slaves, who either struggled with peasant proprietorship or moved to Guyana's towns, and the East Asians, who replaced the slaves on the sugar estates.

The plantations' next crisis involved two consecutive blows: the late nineteenth century depression in world sugar prices and the end of Indian immigration in 1917. Responding to the first blow, sugar cultivation was sharply curtailed producing unemployment among estate workers. Mandle indicates that the colonial authorities reacted with some temporary and marginal assistance to peasants to grow rice as an import-substitute. The second blow paralleled abolition in its seriousness. The colonial authorities responded by developing a sufficiently large labour pool domestically to ensure that a free labour market would continue to supply cheap and plentiful workers.

Professor Mandle devotes a relatively large proportion of this volume to analysing demographic trends in Guyana. His conclusion, supported by evidence from the history of health and sanitation facilities in the colony, is that « the effect, if not the cause, of the investment in public health facilities, which occurred before 1940, was, to some extent, to offset the loss in estate population and labor force, caused by the termination of indentured immigration ». Thus, population growth was stimulated, and the plantations survived.

Finally, depression and war induced price movements and general economic dislocations which set the stage for the current and seemingly insoluble crisis faced today by a « neo-plantation » economy in independent Guyana. First, nearly three fourths of the sugar estates were progressively purchased, merged and rationalized by the firm of Booker Brothers, McConnell, Ltd, involving considerable substitution of capital for labour. Second, increased labour productivity on the sugar estates, together with rising

population, produced mounting unemployment which the long neglected and undeveloped non-plantation sector of the economy could not possibly absorb.

Mandle's research confirms, then, the growing conception of colonial economies as suffering poverty, backwardness and current crisis because of the specific economic consequences of their colonial status. British policy in Guyana sustained the plantation economy despite the extreme poverty it meant for Guyana's majority, despite the costliness and uncertainty of its source of labour, and despite its inability to cope with clearly visible population and other trends. (And as a touch of tragic irony, the different economic roles which British policy assigned to black and Asian Guyanese eventually produced in the 1960's the competitive conflicts which permitted the neo-plantation economy to survive even into a politically independent Guyana).

In many ways, Mandle's is a careful, excellent study. He is aware of several unfinished points, where further research is essential to confirm or alter his interpretation. On three points, some criticism may be ventured. The detail of his demographic analysis, excessive in a book of this length, is also beyond what is necessary to support his conclusions. More important, the notion of the plantation as a « total » environment akin to a mental institution is an analytical device which, while helpful, easily leads toward according too much influence over events to one among several parties in a complex set of interactions. This leads to the third and most important criticism.

The sugar estate workers — slaves, then indentured immigrants, then free wage-workers — and also the small peasants appear in Mandle's work in simply too passive portrayals. In many ways, direct or subtly indirect, they probably exerted pressure on the plantation economy that derived from their needs in the situation. Mandle indirectly brings this factor into his analysis when arguing that one cause for the substitution of capital for labour on the sugar estates after World War Two was the « unreliability » of estate workers which he then ascribes to trade union organization and similar factors. Even if we were to believe (and I do not) that this was the first occasion when the workers exerted a profound influence on economic events, this occasion would need its own historical explanation, which Mandle does not provide. Mandle might quite properly have delimited the subject of his book to the colonial economy viewed from the perspective of the dominant decision-makers, but then he could and should have clearly so indicated just to avoid the implication of nearly unrelieved passivity on the part of the workers. The « world the slave-holders made » is one the slaves also made.

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E. VAN DE WALLE, *The Female Population of France in the Nineteenth Century*, Princeton University Press, Princeton, N.J., 1974, pp. xix, 483.

This is a deceptively titled book. It is really the foundation work for a study of the decline in the birth rate in France in the nineteenth century — a study that Van de Walle is preparing as one in the series of related studies of fertility decline in Europe being carried out by a group centring on the Office of Population Research at Princeton, University. Volumes in this series on Portugal by Massimo Livi Bacci and on Germany by John Knodel have already appeared.

A central element to the approach of the Princeton group is to compute and analyse indices of fertility for numerous political sub-divisions of a country. In every case the job of data-assembly is a herculean one; for France especially so. As a consequence we have the French data offered to us without interpretation in the volume under review. Of the volume's 483 pages, 241 are sets of tables for 82 *departments*. This is therefore primarily a book for specialists, a work of reference for those who work on nineteenth century France. Yet the book deserves reading by all those who work systematically with historical demographic data.

Van de Walle seeks to estimate reliable indices of fertility for France, by *departements*, using census and civil registration statistics. He covers the period from the supersession of the parish registers of the Church by civil registration and the initiation of regular, national censuses up to the early twentieth century. This requires an evaluation and correction of the census counts of population by age and marital status and their reconciliation with the flow of events recorded in the registers of births, marriages and deaths. Because this is to be done for each *departement* of France it has to be pursued in a fairly mechanical way. For his purposes Van de Walle needs to work only with the female population for which the census counts are less disturbed by wars and migration so by focussing only on females he both simplifies the task and gains some prospect of greater reliability.

One of the author's first tasks is to demonstrate that his effort is worth it — that the census and registration are unreliable in their raw form. This he does quite effectively in his second chapter which will make useful reading by all users of historical French population statistics. The census counts, especially for particular age groups, are shown frequently to be dubious. Although there is evident improvement in coverage over time the birth registration statistics are argued to be more reliable than census data.

Van de Walle bases his reconstruction of the female population of France at each census date essentially upon the registration of births. He follows each cohort of female births from census to census (every five years) taking account of loss through mortality and out-migration along the way. Such a procedure requires a starting point. Van de Walle arbitrarily assumes that the population total of 1801 is distributed by age according to a model

stable population consistent with the observed birth and death rate of the time and an assumption of stability demographic processes. In a regime of fairly high mortality the arbitrariness of the assumed initial population washes out after a few decades. Most of the discussion of patterns of change looks at the period between 1831 and 1906 and will be little affected by the arbitrariness of the assumed age distribution in the initial year.

Starting with the female population of 1801, distributed by age according to a reasonably stable population, the numbers are augmented by births, corrected for inferred changes in the degree of registration and diminished by deaths generated by a model life table that is selected to be consistent with the population characteristics and to generate the numbers of deaths over the intercensal interval recorded in the civil registrations. The results of such a computation are compared with the enumerated population of that cohort in the next census and a residual attributed to migration inferred. The implied migration total is then allocated by age class on the basis of a single, standard age distribution of migrants based on immigration to Paris. The calculation is then repeated for the intercensal period to take account of net-migration by age and a reconstruction of the female population at each census date thus arrived at. A similar procedure is applied to the flow of marriages to determine the number of married women at each census date.

As the author makes quite explicit, this procedure requires in addition to the numbers of women by age enumerated at each census and the intercensal flow of birth, death and marriages the assumptions that:

- 1) the 1801 age distribution conformed to that of a model life table stable population,
- 2) the intercensal projection of mortality by age conformed to the schedule of a model life table that would generate the registered numbers of aggregate deaths, and
- 3) the pattern of migration by age was the same for all *departements*.

The last is surely the most doubtful assumption. Migration proves to be such a complication that the reconstruction procedure does not work adequately for the *departements* containing the major urban centres: Paris, Marseille and Lyon — hence the limitation of the study to only 82 *departements*. However, the age pattern of the strongly positive net migration into Paris is essentially what is used to establish the standard age distribution of net migration for all reconstructed *departements*. Thus part of what is called census error may really been peculiarities in local age patterns of migration.

Model life tables are effectively used in this study although the author is fortunate to have reasonably accurate aggregate totals of deaths to act as a guide to selection of the appropriate tables. For many applications of the model life table approach we are not so fortunate. One might wonder

why Van de Walle starts with a stable population age distribution in 1801 and works forward, especially when it is clear that at least some *departements* were already well into the destabilizing process of fertility decline, rather than working backward from some reasonably reliable twentieth century census. As the author shows, though, the influence of his assumption is effectively washed out (through mortality) by mid-century and for the latter half of the nineteenth century his reconstructed populations depend entirely upon the assumed mortality and migration patterns and the corrected series of birth registrations.

The methodology of this book deserves careful attention by all who are working with imperfect measures of population in times past. Half the book, though, is given over to tabular results. For each *departement* there are five tables which present the reconstructed total female population, every five years from 1801 to 1906; the reconstructed age distribution of that population by five-year age groups; female birth registration corrections, final birth estimates and the resulting crude birth rates for each quinquennium; the reconstructed numbers of married women by age; the standard Princeton indices of overall fertility, proportion married, marital fertility and illegitimate fertility. The last is the principal input to Van de Walle's analytical volume, the publication of which we await with eager anticipation. Some readers are now in a position to go ahead with analyses of their own thanks to the appearance of the present helpful volume.

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W. WOODRUFF, *America's Impact on the World. A Study of the Role of the United States in the World Economy, 1750-1970*, London, The Macmillan Press, 1975, pp. xv, 290.

Excuses, if such be needed, for reviewing a work with the title of this one in a journal devoted to European economic history are, one, that it treats primarily of America's economic impact on the rest of the world and, two, that what American history the book does contain is designed essentially, but by no means exclusively, for non-American readers. The book has such merit, furthermore, that it should be required reading for the contemplation of learned men everywhere. Its themes are the epic one of cross-cultural influences, especially of an economic nature, and the development of the interrelatedness of the world economy since 1750.

Its story is told in such a fast-moving way — in only one hundred and eighty-five pages of text — that the book is a model of historical writing.

Professor Woodruff, of English ancestry, but with long residences in Australia and America to say nothing of service during the War in Africa and Italy, has long been interested in the reciprocal influences of one culture

on another and has experienced many of them. He has already produced *The Impact of Western Man* (1966), which treats Europe as the focal point of world history and plans to write a third *World Encounter*, which will be a study of the impact in the world of those Europeans who colonized Australia. He focusses his attention in these studies upon the economic activities of man, but he does not eschew such considerations as basic values of cultures, population problems, political pressures, and intellectual and aesthetic accomplishments.

The book before us contains a wealth of insights into the American historical scene from the conquest of a continent to the extension of the American economy, at least as a model to be realized, to the far corners of the earth. Professor Woodruff believes that America's expansion westward had a great influence on American basic values, although he does not carry this idea so far as Frederick Jackson Turner did, in that it made the American convinced that with enough "faith" one could accomplish any task and that "progress" consisted in extending man's control over his environment, which to many meant increasing man's supply of material goods and services. The author emphasizes the role that America played in being a "safety valve" for absorbing Europe's "excess" population; he stresses the willingness of immigrants to work hard to improve their lot to prove that they had made a wise decision to leave their native lands; he places importance upon the opportunities that America provided for the investment of European capital, which seemed to many, including Marx, as essential for the preservation of the capitalist system; and he contends that World Wars I and II were turning points in America's place in the world, for with them it became primarily an exporter rather than an importer of "influences" — of the various things that it had received originally from Europe and now had to offer the world, modified by the process of passing through the New World.

The story told by Professor Woodruff is supported by an abundance of specific evidence of both a qualitative and quantitative nature, although one should quickly add that no attempt is made to follow the so-called Cliometricians in trying to measure the immeasurable. Furthermore, over one hundred pages are devoted to bibliography and statistical tables for those who want to pursue various subjects or who want more details than can be introduced into the text.

Undoubtedly, however, the most astounding impression which this book leaves on the reader is the speed with which the American economy developed and attained a position of primacy. One is forced to wonder, as the author does, under what conditions and for how long this position can be maintained. One is led to contemplate the proposition that material well-being can lead to the self-destruction of both individuals in society and to the society itself, if improperly used. Here is the great challenge to America of the present day.

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R. D. WOLFF, *Economics of Colonialism: Britain and Kenya, 1870-1930*, Yale University Press, 1974; pp. 203.

The economic history of East Africa has been long neglected hence a book with the title this one bears has the potential of being a large increment to our knowledge of that part of the world. One's excitement quickly subsides on examining this small book, however, for it is merely reinterpretation — and a thoroughly flawed one at that — of the old theme about how the white settler stood in the way of significant economic advance by the indigenous population of Kenya.

This book's claim to originality is its thesis that the impediments to greater economic achievement by the African population can be traced to policies stemming from the economic needs of British imperialism. This may well be so, but this book is not the one that demonstrates it.

In part the book's failure to achieve what it sets out to do may result from circumstances beyond the author's control — he apparently had to limit himself to secondary data — but the suspicion that the difficulty is more than that is aroused by the author's inadequate documentation of his facts and his carelessness in handling data. For example, the first chapter devoted to the economic significance of the part of the British Empire added after 1870 provides statistics which lump all these colonies together making it impossible to really determine whether Kenya was an essential part of the Empire, in economic terms, or whether it was, in fact, of little significance. In discussing economic changes over time Wolff repeatedly (e.g., pp. 5, 7, 8, 9, 10, 11, 15, and 137) takes a single year from one decade and compares it with a single year in another decade as sufficient to show that there has been a general upward or downward trend in whatever he is discussing during the intervening 10 years. The particular two years he has chosen do in fact show changes are in the direction he claims, but one is left with the uncomfortable feeling that much of the trend he presents may result from the choice of atypical years, a doubt he could have easily removed by presenting averages of several years instead.

There is also a disturbing number of instances in which no documentation is provided for critical facts (e.g., pp. 58-60, 63, 68, 86, 96-98, 101-105, 108, 112, and 117). Even more common is presentation of data which only go part way in establishing convincingly his points. For example, he claims that the development of the coffee industry in Kenya «demonstrates some dimensions of the process whereby the structure of a colonial economy is determined by reference to economic needs of the metropolitan power» (p. 73). Yet he merely asserts that Kenya's coffee was important to Britain by making her less dependent on non-Empire sources. He does present total exports of coffee from Kenya and Uganda for many of the years between 1907 and 1930, but neglects to provide statistics showing the

destination of those exports. We are thus left with the possibility that the expansion of Kenya's exports may have been to a large extent in response to the segment of the world market represented by consumers outside the British Empire rather than being part of a grand scheme directed from London as he would have us believe. At another point (p. 141) he comments on the impact of colonial policies on the Kikuyu, Kenya's most populous tribe, in saying « this . . . points to the consequences of British policy on the land, labor, and taxation of Africans; it prevented the accumulation of capital needed to achieve even a minimally efficient agriculture within the reserves ». Here, again, he provides only part of the evidence that one would need to establish this generalization. He does show that the Kikuyu lost land; that there were policies designed to force them to join the wage labour force; and that they were taxed. However, he nowhere establishes what the accumulation of capital actually was or what it would have had to be to achieve a « minimal efficient agriculture » (a term he never defines). (My own work suggests economic changes over roughly a third of this period were such that there, may have been considerable capital accumulation among the Kikuyu.¹

In sum, the contribution of this book is at best slight. While it does establish that economically as well as socially Africans were not treated as well as the white settlers in Kenya before 1930 — a fact that is surely not in dispute in scholarly circles — it does not even begin to establish convincingly its mayor thesis concerning the links between the economic needs of British imperialism and the welfare of Kenya's Africans.

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¹ See my « Myths About the Behavior of Kikuyu Labourers in Early Colonial Period », Institute for Development Studies (Nairobi), Working Paper No. 157, April 1974, pp. 1-11; and my « Economic Change Among the Kikuyu, 1895-1905 », Institute for Development Studies (Nairobi), Working Paper No. 158, April 1974, pp. 1-32.