
REVIEWS OF BOOKS

D. ALEXANDER, *Retailing in England During the Industrial Revolution*, London, The Athlone Press, 1970, pp. XI-282, £ 4.

That historical epoch labelled Industrial Revolution is generally recognized as an upheaval which permeated the entire structure of life in England. Nevertheless, scholarly investigation of the period has focussed principally upon changes in techniques by which society produced its material wares. David Alexander has extended the study of the Industrial Revolution in the area of distribution of output to final consumers. The distributional machinery of an agrarian, rural society was inadequate for a growing industrial society rapidly locating in cities. The evolution of the pre-industrial system of distribution into the modern one is the subject of *Retailing in England During the Industrial Revolution*.

The book is divided into four parts. Part I places retailing into proper social and economic perspective with a look at population, transportation, and the standard of living during the years associated with industrialization. The author provides evidence to support the expected urbanization of society as well as the evolution of medieval towns into cities anticipating the modern. The establishment of transportation facilities is outlined as their social and economic impact is estimated. Because the standard of living is a reflection of purchasing power, which in turn influences institutional machinery of distribution, Alexander examines the proposition that standards of living did rise during the Industrial Revolution. He concludes that « disposable incomes rose in the first half of the nineteenth century, and that the mass of the population enjoyed some of the fruits of economic growth ».

Part II deals with retailing beyond shops. The first of two chapters in this section presents an analysis of the role of fairs and markets in the changing society of the nineteenth century. Fairs failed to adjust to urbanization. They became less important as population became more dependent

on specialists for the goods consumed. Markets, on the other hand, evolved with social conditions, maintaining an essential position in the distributional structure where they were maintained and formalized in such a manner as to keep abreast of the demand for their services. Cities that provided adequate market facilities received substantial benefits in the form of orderly exchange operations, superior goods offered for sale, and satisfaction of consumer demand.

Apart from market selling there is an analysis of itinerant retailing during the period of nineteenth century industrialization. Alexander acknowledges the decline of peddling in the rural regions of England, but argues that itinerant merchandizing expanded in the growing urban centers. Shops maintained an advantage over street selling in most manufactured goods and high-cost items, but foodstuffs and products divisible into small quantities for resale were the strength of itinerant sellers. Alexander portrays the peddlers, hawkers, costermongers and their fellows as necessary and efficient segments of the changing marketing operation. As country people moved to the cities they found the street sellers both familiar and convenient. Long hours required of adult members of working class families left little time for searching out shops for necessities of life. The seller in the streets made such a search unnecessary. Further, itinerant merchandizing was more complementary to shop selling than competitive. Shop owners complained of these low overhead competitors and frequently secured legislation to control them. Nevertheless, they recognized that the street seller served more to attract customers to the shops than to detract from them.

The three chapters of Part III dominate *Retailing*. Shop retailing is the subject of the section, its growth the concern of the beginning chapter. The author presents extensive statistics (the bulk of which is placed in appendices) to support the conclusion that the number of retail shops in England grew during the first half of the nineteenth century. Statistics showing a rise in the number of shops per capita are offered as possible evidence of the growth in the share of the retail trade taken by the shops. The author is quick to point out, however, that such data, without figures on turnover, cannot be taken as definite support for such a conclusion. Moreover, the available statistics prohibit any certainty concerning the share of the market in the shops. A search for evidence on turnover does reveal the beginnings of very large shop outlets in the study period. These units, dependent on high turnover for success, were already seen as a threat to small shops by the mid-nineteenth century. Additionally, the employment of fixed prices and cash sales, the hallmark of the large shops, but coming of age in the small outlets as well, implies a growing reliance on fast turnover and small profit margins by nineteenth-century shopkeepers.

Following the analysis of shops in general is a chapter on specific types of shops. Each major type of operation is sketched out relative to size,

importance, peculiarities, and requirements of the operator. The food and household stores trades, presented first, include a breakdown into grocers and shopkeepers, butchers, provisions dealers and cheesemongers, and bakers and confectioners. The cloth and clothing trades are subdivided into drapery and haberdashery, tailors, dressmakers, and clothes dealers, boot and shoemakers, and hosiery and hats. The metal goods trades, and furniture trades are discussed under the heading of durable goods. Information from records of bankrupt firms are employed to provide a guide to the general trend of operations in each shop trade. The general conclusion drawn is that separation between production and distribution was becoming more definite over the period of analysis. Shopkeepers were becoming specialized retailers instead of production specialists also selling their output.

The final chapter of Part III presents a study of « The Economic and Social Organization of Work and Trade in Shops ». Bankruptcy records are carefully utilized to provide insights into the following aspects of retail operations of the study period: 1. turnover, margins and credit; 2. the role of the household in operating a retail outlet; 3. labor conditions and wages; 4. the premises employed by the tradesman; 5. furnishings and equipment employed in the shops; 6. miscellaneous trading expenses; 7. initial capital investment; 8. stocks and trade credit; 9. loan capital; and 10. contributions to commercial and industrial development.

Part IV is composed of a single conclusions chapter of eight pages. The general findings of the volume are condensed and offered up together at this point.

Although fault can be found with any publication, and every book could be improved, this reviewer finds no compelling shortcomings in *Retailing*. The volume is excellently done. Where data are weak the author acknowledges the situation. The book presents what the writer claims for it and does so in a scholarly but eminently readable fashion. The work is essential reading for historians of the Industrial Revolution and of the retailing revolution. Students will be reading this volume far beyond the time of its author or its reviewer.

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R. CAMERON (ed.), *Banking and Economic Development: Some Lessons of History*, Oxford University Press, 1972, pp. 267, \$ 8.95.

This volume which consists of an introduction by the editor and six papers by young economic historians, usually based on their dissertations, is a sequel to *Banking in the Early Stages of Industrialization* also edited by Cameron, and published in 1967. It adds five countries (Austria, Italy,

Spain, Serbia, and the U.S.A.) and one smaller territorial unit (Louisiana) to the six countries (England, Scotland, France, Belgium, Germany, Russia and Japan) and reverts to one country (Japan) already included in the earlier volume. The periods covered range from the half century before World War II (Italy, Serbia and U.S.A.), a period also covered in the papers on Austria and Japan, to more than 110 years (Austria). Neither the calendar period, nor the period's relation to the stage of the country's economic development nor the approach — conventional rather than cliometric — nor the type of statistics used or the style of presentation are uniform. Indeed there is not much that is common to the six papers except that they all deal with some aspects of the relation of the banking system and the country's economic development during the 19th century. Most of them also try to confirm or to refute Gershenkron's famous hypothesis about the banking system and economic backwardness, but come to different conclusions for their country, though those regarding it as inapplicable to their case appear to be in the majority.

The chapters on Austria, Louisiana, Spain and Serbia will be primarily of interest to the country specialists for whom they provide a good deal of new material and a few interesting hypothesis. They may raise as many questions as they answer but that is praise rather than criticism. The chapter on Austria, interesting because it deals with a case where the banks were following rather than leading industrial development, would have gained if it had brought the two other large components of the Monarchy — Bohemia and Hungary — into the picture. This would have provided an opportunity for a comparative study that is rarely available, *viz.* one among economic units for which many basic factors are the same because of their belonging to one political unit and which nevertheless show differences in their financial development. Since Professor Rudolph seems to have studied banking and industry in Bohemia there is hope that he may yet provide us with a study of this type. Tortella's chapter on Spain is welcome because much of the material is new — at least to non-specialists — and because it deals with the problem of the development of one sector — here the railroads — too far ahead of the others with the result that the economy as a whole does not develop: « The railways were built at the expense of the manufacturing sector they were supposed to help » (98).

Cohen's chapter is one of the most ambitious ones since it claims to « focus attention on the process of forced saving, the reallocation of financial capital and the reorganization of portfolios held by savers » (89), all rather intractable questions in the situation where relevant data are as scarce as in 19th century Italy. As far as I can see there is no systematic explicit quantitative discussion of these problems in the chapter, although it provides a good deal of interesting information out of the author's great store of knowledge about Italy's modern economic history. The chapter has obvi-

ously suffered from excessive compression, which has forced Cohen to make assertions about difficult problems without being able to document them, e. g., on distribution of income and wealth (59, 69) personal saving (67) and sources of industrial finance (64, 72). I am also unhappy about Cohen's presenting most of the figures in 1938 lire using the wholesale price index as deflator. If it is desired, very reasonably, to free the figures as much as possible from the influence of price-level changes and of the growth of the economy, the most appropriate way now available is to express them as fractions of national income. Even better, though requiring more space, is to give the original figures, so serious readers can work, or play, with them, as well as the ratios to national income.

The two chapters, apart from Cameron's excellent summing up in the Introduction, that I found most interesting — *de gustibus...* — are those on Japan, 1868-1930 and on the United States 1863-1913. Both deal with important problems in financial history and propose solutions that are either novel or have been disputed in the literature. They thus add the spice of controversy to the discussion.

Yamamura's thesis is that, contrary to common opinion and in particular to Hugh Patrick's chapter on Japan in *Banking in the Early Stages of Industrialization* of 1967, the Japanese commercial banks resembled until the 1920's more closely British than German banks in that their connections with industry were not very close; their portfolios of industrial securities were small; that most of their credits were made for the short term. This thesis has its corollary in the assertion that internal sources rather than bank borrowing or security issues to the public provided in many cases the bulk of financing, particularly in the early phases of development of industrial enterprises. It was only from the 1920's on with the rapid expansion of the *zaibatsu* that the banks, both those affiliated with a *zaibatsu* and those remaining independent, provided industrial enterprises with what were in effect medium-term credits and acquired substantial portfolios of industrial stocks. It would be presumptuous for one who is not a nipponologist and is unable to read the Japanese language to take issue with authors who meet both conditions. I am, however, impressed by Yamamura's careful documentation, particularly his use of the accounts and reports of individual enterprises. Most of this material, both that used by Yamamura and that as yet unpublished, is unfortunately available only in Japanese. It is very much to be hoped that some of it will also become available in English. In view of Yamamura's own attitude that his thesis should be regarded as tentative and that a great deal of research must be done before it can be accepted or rejected, all a reviewer can say is that it looks to him that the ball is now in the court of the defenders of the traditional wisdom.

Sylla's iconoclasm is a re-evaluation of the National Banking System that stresses its positive contributions to economic growth, and to industrial

development in particular, and questions some of its negative aspects that have dominated much of the previous discussion, e.g., its alleged aggravation of cyclical downturns. Here he has a *prima facie* case since the American economy grew as fast in the half century after 1863 as ever before or after. The defenders of received opinion are therefore forced to resort to a counterfactual assertion, *viz.* that with a different banking system, e.g., one of a small number of banks with a regional or even a nationwide net of branches, or of mixed banks of the German type, growth would have been even more rapid, or at least would have been better balanced in one sense or another, or would have been more equitable. As far as I know, nobody has attempted to test systematically this or other counterfactual hypotheses.

Three economic effects of the National Banking System stand at the center of Sylla's argument. The first is the channeling of funds from the provinces to the central money markets by means of bankers' deposits, a flow which greatly increased the ability of the metropolitan banks to finance industry directly, or indirectly through the stock market. The second effect is the facilitation of monopoly or oligopoly in local bank credit markets resulting from the limitations of entry, a limitation which was most effective in smaller markets. The third is the transformation of national banks « into an important accessory of federal budgetary policies ». On this point Sylla, it seems to me, overstates the case, at least as far as the second half of the period, say after 1890 is concerned when the national banks' holdings of U. S. Government securities had become a rather small proportion of their assets — by 1900 they were down to less than one-tenth of total assets and they hardly increased in absolute amount between 1867 and the mid-1890's — so that they are not likely to have influenced the national banks' policies.

There is only one point of statistics on which I would disagree with Sylla, *viz.* his assertion that the assets of commercial banks increased more rapidly between 1850 and 1910 than most, if not all, American « industries ». Limiting the comparison to financial institutions the assets of life insurance companies increased by about 200 times between 1860 and 1912 — a period close enough to Sylla's comparison — and those of mutual savings banks by 20 times compared to 27 times for all commercial banks and to 67 times for non-national banks (1870-1912). The point is that the extremely rapid growth during the half century before 1913 was not specific to national banks but can be observed for most types of financial intermediaries and therefore must be explained in good part not by factors specific to individual types of financial institutions but by forces raising the rate of growth of the financial superstructure beyond the rate of expansion of the economy as a whole as reflected in the trend of national product.

Now that we have before us the attempts in the two volumes, to disentangle the relationships between banking and economic growth in the

early stages of industrialization of about a dozen countries, all with the exception of Japan, in Europe or North America, can we say with confidence what the typical relationship is or can we identify the economic and non-economic factors that determine the character of the relationship? I am afraid the answer must be negative, although anyone having read these two volumes will be much more aware of the nature of the problem and of the difficulties of an answer. There are at least three possible reasons for doubt. First, it is possible that there is just not enough regularity in the relationship between the development of the commercial banking system and economic growth since the industrial revolution to permit the formulation of hypotheses that are valid for all, or even a large number of countries. Secondly, even if broadly applicable statements about regularly observed relationship could be formulated, this would not yet answer the basic question about the direction of causation. Thirdly the material of which we now dispose, which is primarily macroeconomic, and the analyses that have been made, may not be sufficient to decide about the existence and character of the relationship, let alone about its direction. What is now needed, it would seem, is first, the extension of the investigation to a considerably larger number of countries, particularly those in the Third World; secondly to apply to these countries, as well as to some of those covered in the two volumes, a common statistical framework, a framework that, of course, will be more nearly complete for some countries and periods than for others; and thirdly, and possibly even more urgently, the study of the relationship on a microeconomic level, i.e., from the point of view, and on the basis of the records, of selected individual financial institutions or of selected business firms; particularly, of course, firms that have played an important role in their country's industrialization. The data with which we usually attack the problem — essentially combined balance sheets and operating statements of large groups of financial institutions or business firms — simply are too crude to do the job, even though they are essential to enable us to formulate appropriate hypothesis and tests.

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F. W. CARTER, *Dubrownik (Ragusa). A Classic City-State*, London-New York, Seminar Press, 1972, pp. 25-710, £ 9.50.

In recent years young Yugoslav economic historians have shown great interest in the social and economic problems of the Republic of Ragusa, as the numerous studies by Dinic, Tadic, Vinaver witness. But the Republic's history — and especially its economic history — has also attracted the attention of scholars of other nationalities, due doubtless to the unique richness and

continuity of the documentation contained in the historical archive at Dubrovnik.

The present volume is by an English author. Carter was drawn to the history of Ragusa by the desire to clarify the factors which kept a State of such modest proportions in existence for so many centuries. In fact, even in its period of greatest prosperity, the Dalmatian republic did not exceed an area of 1,029 sq. km. or a population of 80,000. The work sets out to identify the factors in Dubrovnik's social and economic life between 1204 and 1806 which gave it independence and also, for a certain period, wealth.

Mr Carter's study is divided into three parts. The first is devoted to a brief history of Dubrovnik from its foundation to the end of the 'Venetian' period (1204-1330). The second examines the period 1358-1500, which was characterized by antagonism towards Venice and by the Turkish occupation of the Balkans. Finally the third looks at the period from 1500 to 1806, the year in which the republic finally lost its independence. Altogether six centuries are covered, during which, and with varying success, the Dalmatian Republic played an important part in the commercial life of the Mediterranean.

The Venetian period (1204-1358) constituted an important moment in the foundation of the Republic's commercial fortune. Contact — as yet without antagonism — with Venice aided Dubrovnik's initial attempts to establish itself in the commercial sphere of the Eastern Mediterranean, especially in the Adriatic and the Black Sea. Dubrovnik's expansion in this period was exclusively on land, towards its Bosnian and in particular Serb and Bulgarian hinterland. The author examines in detail the rich network of roads by which the merchants of Ragusa travelled throughout the Balkans, as well as the Republic's colonial trade which was established there. By studying the composition of the caravans — the number of horses used, the goods carried, the stops made etc. — Mr Carter has been able to reconstruct not only the itineraries but also the nature and composition of this trade. Dubrovnik's function at this stage was to supply Venice with agricultural goods from its Balkan hinterland.

Between 1204 and 1358 the basis of Ragusa's commercial expansion in the Balkans was laid: the expansion took place between 1358 and 1500 as did the rise of the city-state which was maritime as well as commercial, being at the same time a sovereign and independent state. During this phase Ragusa freed itself from subordination to Venetian economic interests and took on an independent function first in Adriatic and then in Mediterranean trade. This was the cause of the antagonism with Venice, but this was not the sole problem of the period for Dubrovnik. Momentarily Turkish occupation of the Balkans threatened to destroy the rich network of commercial posts which the Republic had created during previous years, and threatened the city's autonomy. A 'modus vivendi' was reached between Ragusa and the Porte, based on payment of a yearly tribute by the former in exchange for

its independence, and this allowed the Dalmatian Republic to develop fully the function which it had enjoyed during the Venetian period as agent between the Balkans and Adriatic, and later the Mediterranean. Mr Carter states that Dubrovnik's expansion was part of the general expansion of the Mediterranean which involved Venice and Genoa also. The basis of this expansion was exchange between developed and underdeveloped areas, of which certain Mediterranean centres became promoters (p. 550).

For the years 1358 to 1500 the author examines the reasons for the progressive expansion of Ragusa's commercial relations, comprising trade in materials, animals, slaves, hides, wax, paper and linen from the Balkan hinterland in exchange for textiles, wine, oil, salt, paper, glass, and ornaments from various Italian states. Mr Carter notes in particular that in this period domestic industry developed in Ragusa, textiles (especially wool), ship-building and jewelry being the leading sector. Artisan activity developed greatly in these years, and this led to initially small but subsequently more conspicuous capital formation, which from the early 17th century went with greater frequency into the deposits of Italian banks and *Monte di Pietà*. The author sees this as an indication of the limitations to the Republic's possible economic development. Its expansion was tied ultimately to geographic and demographic factors even, though it had been these same factors which had initially favoured the Republic in preserving its independence for so many centuries.

The third phase of Dubrovnik's history, the years from 1500 to 1806, covers the period of the Republic's greatest economic power, the 16th and early 17th centuries. 1667 is generally held to be the start of Dubrovnik's decline, being the year in which the city was half destroyed by an earthquake. But many sectors of the city's economy were in difficulty before this date.

The 16th century was, then, the city's great golden age. It was in this century that commercial expansion, both by land and sea, was at its height, extending throughout the Balkans south of the Danube and the whole Mediterranean area. In this period the volume of trade, and the size of the Ragusan fleet, reached their highest levels.

This economic development, accompanied by expansion in tertiary activities, also saw a conspicuous cultural flowering as a natural corollary, which Mr Carter discusses at length. Despite this extraordinary economic expansion, however, the Republic was already in decline by the late 17th century, and more markedly so during the 18th century. The political disaster of 1806 was preceded by a long economic decline, which in many ways paralleled that of Venice. What were the causes of this? Mr Carter argues that we must look first at Ragusa's economic isolation in Adriatic trade, following the rise in the 18th century of new ports such as Trieste and Fiume which were closer to the centres of Europe. In addition to this, one must remember that the formation and broadening of national markets in Europe did not benefit

Ragusa. Being unable to count on a significant national market, and as the demand for its commercial services declined, Ragusa was forced to charge increasingly high rates for ships, charters. Consequently this meant a reduction in the size of the fleet and hence also its part in first Mediterranean and then Adriatic trade.

Mr Carter's book is well documented and rich in appendices which will further increase its value; it is the most useful and important study of the Dalmatian Republic in recent years. It is, however, strange that the author should have neglected the Republic's financial problems, which in a study of this nature, even if they are deliberately excluded, cannot be left totally unmentioned, due to the close relationship between the State's commercial and financial life. But this, perhaps, is a further reason for others to be drawn to the often surprising economic history of the Dalmatian Republic.

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P. N. CHRISTIERNIN, *Lectures on the High Price of Foreign Exchange in Sweden* (1761), edited with an introduction by Robert V. Eagly, *Memoirs of the American Philosophical Society*, vol. 87 (Philadelphia, 1971), pp. 119, \$ 2.00.

In the bibliography of economics, the accolade of success comes for many reasons, intelligence above all, and often, too, the rare fortune of capturing the issues in hand. Other names, apparently no less deserving, remain for one reason or another on the side-lines of academic debate. Their contribution has won more restricted approval and missed the wider audiences concerned with the evolution of theory. So in Sweden, Pehr Christiernin. His *Lectures* float almost unnoticed on the mainstream of economic thought. Even, in that remarkable *Who's Who* of economics, Joseph Schumpeter's *History of Economic Analysis*, the procession of great personalities moves along without mentioning his name. Scholars can now appreciate Robert Eagly's combined edition of the *Lectures on the High Price of Foreign Exchange in Sweden* (1761), and *Some Comments on Current-day Economic Issues* (1769), and these studies are doubly welcome. They reveal a neglected contribution to economic thought in eighteenth-century Europe; but they also set in perspective the special case of Sweden.

Christiernin's analysis unfolds with remarkable clarity and perception. He seizes the issues; his exposition is succinct, as, for example, on the crude quantity theory of money: « the price of all goods is in proportion to the circulating money supply and its faster or slower velocity ». And he runs with ease through the gamut of problems: variable exchange rates; bullion points; credit, debts and wealth; the potential role of the state in controlling monetary circulation through the sale of government securities; the necessity

of cheap money, a foretaste of Keynes. Perhaps he did not win the popularity of his brilliant contemporary, Ferdinando Galiani; nor did he innovate, as John Law over half a century earlier, that the utility of money could increase its value. Yet, his contribution is still remarkable, indeed too remarkable to continue in the shade.

The reason for this oversight can no doubt be found in part in the language barrier: works in Swedish did not find a wide circulation among the intellectuals of Europe. But surely also some explanation must be attributed to the basic characteristics of the Swedish economy. These gave Christiernin some of his striking examples. When the *Lectures* appeared in 1761, the Seven Year's War was at its height. The Hats had been in power for over twenty years, encouraging economic development, mercantilist policies and inflation. After 1765, came the turn of the Caps to govern and open the way for laissez-faire policies and deflation, with a spell in office which lasted until the monarchist coup d'état of 1772. Christiernin, therefore, wrote in a wave of inflation, in the rising tension of confrontation. The struggle in Europe was reflected, inevitably, in debts and inflation. Even in this, Sweden played odd man out in the concert of nations. Monetary affairs alone were enough to emphasise these contrasts, for, in Sweden, plates of copper played a significant role as means of payment. In what other country were the worthy citizens obliged to carry their small-change in back-packs or on sledges? The inconveniences of this money were legion and led directly to the wider use of instruments of credit. In 1661, Sweden had been first to introduce the banknote in Europe; in the eighteenth century, this expedient again found favor. Little surprise, then, that in time of war the government sought the easy solution of extending credit and increasing the note issue. The results were predictable: the value of the *daler* fell on the markets of Europe, and continued after the war ended in 1763. These considerations were the background to Christiernin's studies and go far to explain the urgency of his proposed reforms. Yet, although they were remedies in the heat of the moment, his work clearly has the gloss of quality. Robert Eagly has performed an excellent service in bringing his *Lectures* to a wider circle of scholars, and redeeming Christiernin from a long neglect.

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R. S. DUNN, *Sugar and Slaves, The Rise of the Planter Class in the English West Indies 1624-1713*. Chapel Hill, The University of North Carolina Press, 1972, pp. XX, 359, \$11.95.

In this book Richard S. Dunn has written a clear and impressively documented account of the origins of the plantation dominated societies of the English-speaking Caribbean. The focus of attention is Barbados, Jamaica, and

four of the Leeward Island, St. Kitts (more formally referred to by Dunn as St. Christopher), Nevis, Antigua, and Montserrat. The book is divided into three sections: a long introductory chapter on the beginnings of settlement in the region, a series of chapters on the experiences of the individual islands and finally, a sequences of chapters on more general themes.

Dunn writes that his intention was to explain how the early English planters responded to three things: life in the tropics, the novelty of large-scale sugar production, and the use of slave labor. These are the questions of a social, not an economic, historian. Economic historians therefore may feel uneasy about his methodology and the range of issues which he has addressed. Dunn does not for example essay an analysis of the international market for sugar in the seventeenth century, although such an effort would have contributed substantially to his explanation of the economic emergence of sugar after 1640. He has succeeded brilliantly, however, in answering the questions he sets for himself. In so doing, he has presented much valuable material for economic historians of the region, and he has written an extremely worthwhile book.

It was in Barbados that the plantation economy developed most fully during this period. The decisive moment in this growth seems to have been in 1640 when the planters shifted from a tobacco and cotton economy to the growing and grinding of cane. Dunn does not fully explain the timing of this shift although he does cite the climatic and soil advantages of growing sugar in Barbados, the absence of competitive suppliers, and the relatively high price received for the commodity as contributory factors. Dunn expresses surprise that the shift to sugar was slow to come but notes that it developed with extreme rapidity between 1640 and 1643. It is a mark of Dunn's familiarity with his material and good sense in handling it, that even in the absence of an explicit model of market behavior, most if not all of the variables which such a model would call for are covered in his explanation of the movement to sugar. Whatever the reason for the shift, its consequence was a society organized to provide masses of undifferentiated workers to the estates, a concomitantly rigid social hierarchy, and an enormous and unbridgeable distance between the way of life of the ruled and the ruler.

The sugar boom, given the labor-intensive nature of the technology of the time, created a massive demand for workers. Until then, indentured white servants from England constituted the bulk of Barbados' labor force. What induced the planters to import African slaves to the island? Dunn's answer is their « economic needs » and « social assumptions » (p. 72). These profit-maximizing entrepreneurs were frustrated by the high cost and unreliability of the traditional labor force. At the same time the existence of this white indentured servant class reinforced the planters' « social assumption » concerning the necessity for the existence of such an unprivileged working class.

Quickly learning from the Brazilians that a cheaper more reliable source of labor was available from Africa, they entered the slave trade. This represented rational economic decision-making, given a prevailing social welfare function which made no assumptions about the desirability of equality or indeed of the common humanity of the members of the local community. The tragedy for the West Indies, of course, was that this private sector rationality had devastatingly negative implications for the long-term development of the region, consequences which continue to mar the prospects of West Indian society.

If Barbados had become a fully articulated plantation economy by the end of the seventeenth century, the Leeward Island and Jamaica lagged substantially behind. The explanation seems to be that the Leewards, a prize hotly contested by the British and French, lacked the stability necessary for the expansion of production and the rooting of social institutions. Jamaica, on the other hand, was primarily a chief staging ground for buccaneering expeditions against the Spanish Main and, until relatively late in the period, was reputed to be the «*Dunghill of the Universe*». Plantation agriculture only gradually gained a foothold as conflict between the buccaneers and planters continued. Ultimately, with the Glorious Revolution in England, the planters gained the support they needed to triumph over the buccaneers. When the buccaneers moved elsewhere, the stage was set for Jamaica to take its place among New World plantation economies.

Following chapters on the growth of the individual islands, the remainder of the book is devoted to more general themes: sugar, slaves, mortality and fertility, and an all too brief discussion of the «*legacy*» of the period. On each of these topics Dunn is interesting and informative. In explaining the far greater incidence of slave revolts in Jamaica than in the other islands, Dunn argues that the chances for an uprising to be successful were far greater there than in the Leewards and Barbados. Not only was Jamaica itself larger, giving the insurgents the opportunity to escape, but its individual plantations also tended to be larger, thus creating greater concentrations of slaves there than elsewhere. In his discussion of life and death Dunn provides a fascinating account of the tenacity with which the settlers maintained traditionally English ways of life with respect to clothing and architecture. They paid a high price for their reluctance to adjust to the tropical climate in these matters in the form of exceptionally high rates of mortality and short life expectancies.

In sum, this is a major contribution to the literature. Dunn writes well, although he is not without his favorite and therefore overly used words. Sugar is forever «*fetching*» a price in the market while variables frequently are said to «*correlate*» with each other when no evidence of correlation analysis is provided. Let me also note that the book's usefulness for specialists would have been enhanced had a bibliography been included. Such a tool would have been invaluable especially given the author's wide-ranging research

as evidenced in his footnotes. These objections, however, are trivial when compared to Dunn's achievement. *Sugar and Slaves* is a first-class job of recounting the origins of the plantation economies of the English-speaking Caribbean.

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C. FORSTER and G. S. L. TUCKER, *Economic Opportunity and White American Fertility Ratios, 1800-1860*, New Haven, Yale University Press (Yale Series in Economic History), 1972, pp. IX-121, \$ 6.50.

In this short monograph, the authors take up the argument of Yasukichi Yasuba that the decline of birth rates of the white population of the United States before the Civil War was significantly associated with the progressive disappearance of unsettled farmland.¹ Yasuba had examined that relationship along with the more common view that the decline of birth rates was essentially a result of urbanization. Yasuba's argument, backed up by significant inverse rank correlations between birth rates and the density of population in relation to cultivable land, did not appear to have gained wide acceptance. Indeed, the author of a subsequent major review of population trends in the United States was content to pass it off as « ... not very convincing ».²

Forster and Tucker seek to redress this oversight and to elevate Yasuba's work into its proper place essentially by reworking the evidence to iron out some of the difficulties that may have diluted the impressiveness of the earlier work. First they reformulate the main explanatory variable. Where Yasuba has used the density of population in relation to cultivated acreage in 1949 (the most recent agricultural census year prior to his analysis), Forster and Tucker substitute the ratio of adult population to farmsteads in a year not long after the period of their analysis (they try 1850, 1860, and 1880). This substitute measure is claimed to be less impaired by the long span of time between the first half of the nineteenth century and the middle of the twentieth over which changes in technology may have substantially altered the population carrying capacity of any given land area. Second, Forster and Tucker quite properly suggest a one decade lag between their explanatory variable and the birth ratio — the number of children under ten (i.e., born over the course of the preceding decade) to the number of women 16-44. Then they under-

¹ YASUKICHI YASUBA, *Birth Rates of the White Population in the United States, 1800-1860*, Baltimore, The Johns Hopkins Press, 1962.

² J. POTTER, *The Growth of Population in America, 1700-1860*, in D.V. Glass and D.E.C. Eversley (ed.), « Population in History », London, Edward Arnold Ltd., 1965, p. 677.

take the laborious job of deleting from the analysis the urban populations (places of 2500 or more) so as to avoid the charge that population density, whether it be in relation to cultivable land or to number of farms, directly reflects urbanization. Finally, Forster and Tucker substitute multiple linear regression analysis for Yasuba's blunt instrument of rank correlation.

The result is a confirmation of the conclusion reached earlier by Yasuba. Birth ratios are in all decades inversely related to the adult/farm ratio in a statistically significant way while in none of the years up to 1860 was there any significant relationship to urbanization. The evidence stands more compelling than ever that the sizeable decline in the birth ratio that was accomplished in the United States in the period before 1860 was not associated with urbanization but had to do with the process of agricultural settlement and the disappearance of unutilized farmland. The point was certainly worth making and justly deserves the attention of all persons concerned with the course of demographic change but one cannot help wondering whether it really needed a 120 page monograph. Could it not really have been done as well in an article? If the topic is to be given book-length treatment, I think the reader could have expected an even more substantial structure built upon the foundation left by Yasuba.

The ratio of adult population to farms in a year shortly after the period under investigation is a doubtful improvement on Yasuba's measure of density of settlement. Presumably what is sought is some measure of the ease of availability of farmland. Economic theory might have suggested some reliance on land values or rentals. Better measures of potentially cultivable acreage may be available than what was used in 1949. Maximum acreage ever improved, whatever the date, or land classed as cultivable by soil surveys might be more satisfactory alternatives. However potentially cultivable acreage may be determined, densities or related measures calculated in acreage are to be preferred to measures involving number of farms. The latter are susceptible to variations in what gets reported as a farm and to reductions in farm size as one avenue of adjustment to population pressure. It is less problematic to work in terms of acres per person than farms per person, especially in a situation where farms averaged 100 acres or more.

A more serious concern, though, is that Forster and Tucker are content to keep the analysis at a very empirical level. Despite the conviction with which they express their conclusions about what the empirical evidence indicates, they are remarkably unwilling to strike out in the direction of causal analysis, to lay before us the sort of theoretical model that would explain why birth rates were falling in rural North America in the first half of the nineteenth century. The more general message is left completely unstated.

Forster and Tucker have nonetheless made a valuable contribution. They have made it increasingly impossible to toss off the evidence that an important early decline in birth ratios was largely unconnected with urbanization as

« ...not very convincing ». The more positive case, that the decline in birth rates was causally related to the disappearance of unsettled farmland, is not so convincingly made. A serious remaining problem is that, over the whole period 1800-1860, a large part of the decline in birth ratios occurred within the single decade 1840-1850. In that decade, the sparsely settled states on the frontier experienced declines that were just about as sharp as those in the long-settled states along the seaboard. Of course it is possible that the temporal pattern of change is largely artificial — that there was peculiarly deficient enumeration of children in the census of 1850. Unless that were shown to be the case, I would have preferred to see an analysis of the decline of White American fertility ratios that more persuasively tied together the cross-sectional variations with the temporal pattern of change.

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A. C. LEIGHTON, *Transport and Communication in Early Medieval Europe*, A. D. 500-1100, David & Charles, Newton Abbot, pp. 257, £3.75.

For a number of years Professor A. C. Leighton has devoted himself to the study of the history of transportation and communications. In 1967 he published a short but brilliant article on « The Mule as a Cultural Innovation »; in 1969 he wrote about « Secret Communication among the Greeks and Romans » and in 1970 he published in the *Encyclopedia Americana* a short article on « The Horse and Human History ».

In the present volume Professor Leighton condenses the results of his research on « Transport and Communication in Early Medieval Europe ». Although according to the title the period under scrutiny is that from A. D. 500 to A. D. 1100, much information can be found in the book about either previous or later times. The book is made up of four chapters: « The people, materials and concepts transported »; « Land transport »; « Water transport » and « Some social economic consideration ». The author concludes (p. 180) that

« The Romans never solved the problem of rapid, heavy transport on land. What success they achieved was due to administrative skill rather than technology. In the comparatively protected waters of the Mediterranean and on the rivers of Gaul, the Romans did achieve economical water transport... In the eleventh century by efficiently harnessing the horse, medieval men achieved more rapid and economical land transport than the Romans had known. In the twelfth century improvements began to appear in the old round ship, fitting it for a more effective role in sea transport ».

The book is indeed a good summary of all that can be gathered from the available sources and literature about the subject. The author knows well

the primary sources and he is familiar with the literature. References are abundant, but one questions the usefulness of numerous footnotes which refer the reader to works of general information.¹ One asks oneself what prompted the author to interject occasional hypotheses which are patently absurd in an otherwise solid text.² The important topics of communication of informations, ideas and diseases are treated in a cursory and superficial way (pp. 42-7). The general conclusions of the work do not represent a great novelty. These points however do not detract from the value of a learned book which essentially recommends itself as a good source of information on a very important subject.

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D. N. McCLOSKEY (ed.), *Essays on a Mature Economy: Britain after 1840*, Princeton University Press, Princeton, 1971, pp. XV-439, \$ 12.50.

This volume constitutes the proceedings of the « Mathematical Social Science Board Conference on the New Economic History of Britain, 1840-1930 » held at Harvard University in September 1970. It contains eleven research papers, prepared comments on these papers, a symposium on the future of the « new » economic history in Britain, summaries of the general discussions that were held, a note on some of these discussions by S. Saul and an introductory essay by the editor.

My role as a reviewer of this material is somewhat complicated by the fact that I was a participant, albeit a non-paper presenting participant, at this conference. On the plus side, this background has the advantage of letting me compare my reaction to the conference itself with my reaction to the book two and one half years later. In addition, I am clearly in an excellent position to judge the degree to which the content and spirit of the general discussions have been reproduced. On the other hand, my par-

¹ For instance when the author asserts that « a grown ox was little larger than a present day calf » (p. 65) or when he states that « pack-horses can carry approximately 220-330 lbs » (p. 104), the authority quoted is DAY's *History of Commerce* (New York 1922). For the information given on transportation on the Rhine River (p. 157), distance covered by oxen in one day (p. 159), cost of transportation of wool (p. 164) one is summarily referred to the Cambridge Economic History. Such references are perfectly useless.

² On page 30 one reads that « While it would be unwise to blame the downfall of the western empire on the increasing scarcity of rare beasts for the arena, this could have contributed to the discontent of the urban proletariat and perhaps have facilitated the acceptance of Christianity among them ». I am sure that one of these days, inspired by this passage, some champion of the so-called new-economic history will determine a linear correlation between « scarcity of beasts » and the rate of « progress of Christianity ».

ticipation may make my objectivity somewhat suspect. In all honesty, however, I do not feel any vested interest in the conference or the volume as such. Having put my cards on the table, I will now proceed to a discussion of the book.

The heart of the book is the collection of prepared research papers. These are divided into four groups: I. Britain and the Atlantic Economy (S. Engerman, « The American Tariff, British Exports and American Iron Production, 1840-1913 » and B. Thomas, « Demographic Determinants of British and American Building Cycles, 1870-1913 »), II. The Functioning of the Capital Market (M. Edelstein, « Rigidity and Bias in the British Capital Market, 1870-1913 » and D. Moggridge, « British Controls on Long Term Capital Movements, 1924-1931 »), III. Economic Efficiency and the Choice of Technique (P. David, « The Landscape and the Machine: Technical Inter-relatedness, Land Tenure and the Mechanization of the Corn Harvest in Victorian Britain », C. Harley, « The Shift from Sailing Ships to Steamships, 1850-1890: A Study in Technological Change and Its Diffusion », P. Lindert and K. Trace, « Yardsticks for Victorian Entrepreneurs », and D. McCloskey, « International Differences in Productivity? Coal and Steel in America and Britain Before World War I ») and IV. Problems of Measuring Productivity: The Capital Goods and Service Sectors (R. Floud, « Changes in the Productivity of Labour in the British Machine Tool Industry, 1856-1900 », W. Vamplew, « Nihilistic Impressions of British Railway History » and G. Hawke, « Railway Passenger Traffic in 1865 »).

Since these papers claim to be « new » economic history, the most obvious approach is to inquire exactly what is « new » about them. One popular definition of « new » economic history, or « cliometrics » requires the extensive use of econometrics. I have never liked this definition, partly because I do not do much econometrics myself and I certainly like to consider myself « new ». I am therefore happy to note that only the papers by Engerman and Edelstein can be said to pass the econometrics test. Thus, either econometrics is not essential to « new » economic history or else this book contains precious little « new » economic history. If, however, the definition is extended to include all work that explicitly uses economic theory to frame testable hypotheses that are then tested using quantitative information, then the seven papers on choice of technique and the measurement of productivity would also seem to qualify. At least they explicitly utilize economic theory and quantitative measures. This leaves only the papers by Moggridge and Brinley Thomas. I find it difficult to view Moggridge's paper as « new ». This judgment makes the point that economic history need not be « new » to be good. As to Thomas' paper, it might also be called « new ». Since, however, it is essentially a continuation, in both subject and methodology, of work that Thomas has been doing for a good long time, such a classification would mean that economic history need not be new in order to be « new ».

In addition to methodology, some interest naturally attaches to the conclusions of these papers. This interest should be increased by the fact that collectively they constitute a nontrivial fraction of all the currently existing « new » economic history papers dealing with Britain, particularly late nineteenth century Britain. On an individual basis, the conclusions reached are naturally a direct outcome of the particular questions asked. Thus, for example, David tells us that the reaper was introduced into Britain in an economically rational way and McCloskey is able to assert that there did not exist a large difference in total productivity in the British and American steel industries in the pre-World War I period. These individual conclusions are no doubt of interest to specialists in the particular areas concerned. Moreover, it would not be surprising if their publication resulted in considerable controversy and, at least in some cases, in further work on the particular problem studied.

On a different level, and one which should be of greater interest to economic historians in general, it is possible to ask whether there was any clear trend or unifying theme to the results of the various papers. The answer to this question is a definite yes. The general theme is, as McCloskey puts it in the introduction, « that British performance was better than has been commonly believed » (p. 5). In particular, the well-known and widely accepted proposition that the British economy suffered greatly after circa 1870 as a result of « entrepreneurial » or « managerial » failure was severely buffeted. Only the papers by David, Edelstein and McCloskey presented a direct conclusion of nonfailure for their particular field of study, but almost all the papers supported the contention that British managers (and bankers and farmers) generally behaved in an economically rational way. The only paper to explicitly argue otherwise was that by Lindert and Trace. Although it is certainly not clear from the title, the main section of this paper is a study of the adoption of the Solvay process in Britain. The authors originally concluded that the managers, who only get entirely out of the Leblanc business during World War I, should rationally have junked their Leblanc equipment around 1890. A revision to take account of the authors' failure to consider fixed costs moved the junking date up to 1897. In addition, I personally am very doubtful of the low interest rate (which creates a bias towards a conclusions of irrationally) used in the calculations.

The great interest generated by this question of economic rationality is made clear by the fact that it was the chief topic of the general discussions held at the conference. Indeed it is also the chief historical topic of McCloskey's introduction and the sole subject of the note that Saul added after the end of the conference. While all participants may not have agreed with all the criticisms made of the failure hypotheses, its defenders were clearly on the defensive throughout the conference. Furthermore, to my knowledge they have not yet launched a published counter-attack. Thus, if

not dead, the entrepreneurial failure hypotheses has been severely wounded and is in serious need of new empirical evidence if it is to recover.

In my view, this general conclusion makes the substantive historical output of the conference important. On an individual basis, however, I must regretfully conclude, on the basis of my subjective standards, that while all the papers are good, no single paper is by itself a really important contribution. That is, none of the papers present either a major innovation in methodology or a solidly established, important new conclusion. This, however, should not come as a great surprise. Authors usually know when they have written a really important paper and they are unlikely to present such a valuable piece of property to any conference. Indeed, a conference, such as this one, in which the participants maintained such uniformly high standards which is totally free from clunkers is a fortunate exception to the rule.

The last section of the book is a very interesting symposium on the role of the « new » economic history in Britain (J. Hughes, M. Hartwell, B. Supple and R. Matthews). The key question discussed is the generally cool reception received by the techniques of the « new » economic history in Britain. I am enough of a cynic, and economist, to credit the argument that British economic history departments, dominated by professors trained as historians, have generally viewed the « new » economic history as a threat to their human capital.

In conclusion, I would like to say a few kind words about the editor « who was also the highly efficient organizer and host of the conference ». McCloskey's introduction is excellent and his editing of the general discussions is also very good. He has accurately summarized what was said and he has succeeded in capturing at least some of the flavor of the discussions. Inevitably, however, no summary, or complete protocol for that matter, could capture the intellectual excitement and stimulation that permeated the meeting when they actually occurred.

LARS G. SANDBERG
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B. PULLAN, *Rich and Poor in Renaissance Venice. The Social Institutions of a Catholic State, to 1620*. Cambridge, Mass., Harvard University Press, 1971, pp. X-689, \$ 13.50.

In this impressive study of institutional (as distinct from private) philanthropy in Renaissance Venice, Profesor Pullan has brought to life a little known, yet fascinating aspect of the history of an Italian city-state, and notably the way in which it tried to cope with the problem of its poor and disabled. On the basis of a prodigious amount of research in the Venetian archives he has succeeded in throwing much new light on a city-state which

is generally better known for its patrician oligarchy, its enduring constitution, and its commercial achievements than for its concern for the nameless and largely voiceless multitude at the bottom of the social pyramid.

The book consists of three parts. The first deals with the older, traditional agencies of poor relief, those religious fraternities, known as *Scuole*, whose rather exclusive lay membership included both well-to-do citizens and needy artisans, with the former supplying economic aid to the latter in return for prayers. The second part, aptly entitled « The New Philanthropy », is devoted to the new agencies which proliferated in Venice and her dominions from the late fifteenth century onward — all those hospitals, almshouses, orphanages, and shelter homes for repentant sinners built by the religious fervor of the early Catholic reform movement and of the Counter-Reformation. That same fervor was at the root of much of the heightened concern over the problem of loans to the poor. As told in the third, and possibly the best part of the book, that concern revolved around the desirability of replacing the Jewish bankers and pawn brokers with charitable, low-interest loan institutions known as the *Monti di Pietà*. The success of the *Monti* was partial on the Venetian mainland where the new institutions mushroomed between 1450 and 1550, and proved to be more a complement to than an outright substitute for Jewish moneylending. In Venice itself, on the other hand, all plans to set up a publicly controlled *Monte di Pietà* were blocked by the government. Ultimately the Jewish community was forced to ensure that low-interest loans would be available to the Venetian poor year after year, the Jewish banks themselves serving, in fact, as charitable credit agencies. In return for this burdensome responsibility, however, the Venetian government allowed the Jews to remain in the city and to engage not only in the traditional second-hand trade, but also in the more lucrative Levant trade; it was the profits from that trade that helped finance low-interest loans for the Christian poor.

In undertaking this massive investigation of poor relief Professor Pullan has aimed at more than just uncovering a much neglected facet of Venetian society; he has also aimed at testing two broader and widely held assumptions. One is to the effect that the astonishing stability and longevity of the Venetian commonwealth owed much to the ways the common people were treated and cared for as well as to the fact that the numerous charitable institutions, with their elaborate administrative machinery and their hierarchy of elected officials, provided the non-noble citizen class with an outlet for social ambitions. This created a sense of involvement in public affairs, and was a substitute for that genuine political power which the patricians alone were entitled to wield. The second assumption is that in the age of the Reformation, philanthropy took sharply different forms depending on the religious loyalties of individual countries: while Catholic countries, so it has been argued, persisted in the medieval practice of indiscriminate almsgiving and in the view that alms are

to be valued primarily as meritorious acts on the part of the giver, in Protestant countries charity began to be rationalized and judged for what it does to the body of the recipient rather than to the soul of the benefactor. On the strength of his extensive findings Professor Pullan cautiously confirms the first assumption and firmly rejects the second. In particular, he convincingly shows that Venetian philanthropy and social legislation in the sixteenth century tended to become more sophisticated and diversified as they attempted to meet specific and clearly perceived social needs; that they tried to eliminate begging, vagrancy, and idleness; and that they made serious, although not necessarily successful, efforts at reintegrating the able-bodied poor into the mainstream of economic life.

As an inquiry into the motivations, aspirations, and policies typical of an early modern society, *Rich and Poor in Renaissance Venice* stands as a model that will hopefully be emulated by other students of Italian social history. But it will be read with interest and profit by the intellectual and the economic historian as well. The former, for instance, will find in the chapters dealing with « the new philanthropy » a rich harvest of fresh insights into the Catholic reform movement, while the interest of the latter will surely be aroused by the wealth of new information on the nature and causes of poverty in a pre-industrial society, on population pressure and the problem of the food supply, on the repercussions of famines and plagues on the fragile mechanisms of an economy precariously poised between a glorious past and an uncertain future.

DOMENICO SELLA

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H. SEELING, *Die Eisenhütten in Heerdt und Mülheim am Rhein*, Cologne, Rheinisch-Westfälischen Wirtschaftsarchiv zu Köln, 1972, pp. 168.

This volume on the ironworks of the districts of Heerdt and Mülheim on the Rhine is a final addition to the series *Schriften zur rheinisch-westfälischen Wirtschaftsgeschichte* (edited for many years by Professor H. Kellernbenz and at present by Professor F. W. Henning) which, in the space of little more than a decade, has gathered together a rich and most valuable series of studies centred on the Rhine-Westphalia region which, in a way paralleled by few other regions in Germany, has consistently made an essential contribution to her economic development.

In many ways the economic and industrial history of this area of Germany can be identified with the history of the iron industry, a sector to which many economic historians in Germany have devoted their attention both now and in the past. It will suffice to mention, for example, the monumental history of iron by Beck (cf. L. BECK, *Die Geschichte des Eisens*, Braunschweig, 1903, 5 vols.) or the no less remarkable work by Johannsen, (cf. O. JOHANNSEN, *Geschichte des Eisens*, Düsseldorf, 1953, 3rd. ed.). Seeling himself already

has to his credit numerous studies devoted to the development of this industry in the Ruhr valley. However, too often the interest of scholars has been attracted by the 'giants' active in this sector in the period when Germany was attempting to overcome English supremacy in iron and steel production, rather than by the group of more modest, but still considerable, industrial concerns which were in existence both before and at the same time as these 'giants'. In fact the various studies of Krupp by Menne (cf. B. MENNE, *Krupp. Deutschlands Kanonenkönige*, Zurich, 1937) von Klass, (cf. G. VON KLASS, *Krupps, The Story of an Industrial Empire*, 1954) and others, are well known, as well as Redlich's study of the leaders of German industrialization (cf. F. REDLICH, *The Leaders of the German Steam-engine Industry during the first Hundred Years*, in «Journal of Economic History», 1944, IV).

Although such names as Jacobi, Haniel, Huyssen and especially Krupp are better known for this period than those of the obscure traders of Neuss, who in 1857 founded the ironworks of the same name in this little town near Heerdt, or their fellows in Mülheim on the Rhine, who also established a foundry in their city some years later, it seems to be the author's intention to show that the contribution they made to the industrial development of Germany in the second half of the 19th century was no less important than that of the former.

Seeling's work is based, not only on valuable material from public and private archives in Düsseldorf and Neuss, much of which was unedited, but also on a remarkable mass of pamphlets and articles (on mining and industrial problems) of the time, many written by the men who participated in the events described in the book, in particular by F. Büttgenbach. It is divided into two parts. The first deals with the iron foundries in the district of Heerdt, the second with the foundries at Mülheim. The period under consideration in both is that from the late 1850s to the early 1870s. These were the years that witnessed a great expansion on the production of cast iron and steel in Germany due to the more modern production methods that were adopted, especially in the Ruhr. Between 1852 and 1861 75 companies were formed, with a total capital of more than 80 million talers, for the production of cast iron and other iron products. In the Ruhr alone between 1851 and 1857 27 coke blast furnaces, about half the total number (60) in the whole of Germany, were installed.

The Neuss ironworks were founded in 1857 through the initiative of a group of local businessmen, one of whom, Reiner Broix, gave his name to the enterprise. Its initial capital was 200,000 talers, soon increased by a further 40,000 talers in 1859. The presence of a family skilled in mining techniques and mining — the Büttgenbachs — at Neuss-Heerdt in this period meant that the output of the local blast furnace was constantly on the increase, due to skilful and timely management. Moreover this was the period in which the small coal furnaces were moved to districts where the deposits of iron

ore were situated and gave way to the great coke furnaces. The production of iron increased by leaps and bounds during the 1850s and 1860s in Germany, particularly in the Rhine-Westphalia region. This increase in production was illustrated by the greater quantities of ferrous ores absorbed by the Neuss foundry and by the greater output of pig iron there, and by the greater value of this production. The peak of this growing productivity was reached in 1873 (162,000 tons to the value of 600,000 talers compared with 2,600 tons to the value of 18,000 talers in 1860. It was in this period that a second coke furnace was put into operation in the Neuss foundry, but this did not save the concerns from the crisis of the later years of the 1870s.

The foundry at Mülheim was established in 1863 by Elfes and Thywissen, with a modern coke furnace built by Carl Abel on the lines of the one exhibited by the Russian engineer Vladimir Karlovitsch at the Universal Exhibition in London in 1862. But its productive career did not differ from that of its 'sister' at Neuss-Heerd: there was a boom in production around the end of the 1860s and crises in the late '70s.

Although the crisis of the years after 1873, which was, moreover, a crisis of world-wide proportions, did not bring about the disappearance of the foundries of Neuss-Heerd or Mülheim — the former at least was not closed down until 1927 — it is certain that from it emerged the great enterprises which through cartels of various kinds controlled the supplies of coal and iron ores, manufactured iron and steel and ran their own engineering works, so damaging finally the chances the smaller firms had to expand. In the Ruhr the number of concerns with several blast furnaces increased after this period, — the Phoenix, for example, had 12 located at Berge — Borgbeck, Ruhrort and Kupferdreh.

The part played by the foundries of Neuss-Heerd, Mülheim (and others of the same size, one might add) cannot be underestimated so far as their contribution to the economic development of Germany in the phase preceding the formation of the industrial 'cartels' is concerned, and hence to the German industrial revolution. In this sense it was no less than that made by concerns which were to attain quite different stature after the crisis in the 1870s. Above all the technical capacities attained by these concerns not lost but constituted a valuable contribution — in the sphere of the German steel industry as a whole — to the fact that Germany had become the second most important producer of cast iron in the world by the eve of the First World War.

Seelin's study thus represents a most valuable contribution to our knowledge of the achievements of medium and small size enterprises in Germany's rapid industrialization in the second half of the 19th century, even if the difficulties facing these concerns are not always related by the author to the wider, more complex structure of the German economy of the time.

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A.G.L. SHAW (ed.), *Great Britain, and the Colonies, 1815-1865*, London, Methuen, 1970, pp. IX-216, 80 p.

The study of the nature of imperialism has undergone a change of direction which contrasts with those affecting other controversial topics in that no one could claim that it was a strictly linear change. In terms of the theoretical tradition of the Marxist movement — which adopted the analysis of imperialism almost as a theoretical corner-stone, given the far reaching importance of the topic — the position adopted by Lenin, as expressed in particular in 'Imperialism the Highest Stage of Capitalism', does not follow directly from Marx's own premises. In fact Lenin seems to have proceeded in a way totally different to Marx by taking a series of major categories — such as Finance Capital — as starting points, rather than examine in detail how they came into being.

It was in the 1920s that the great divide between economists and politicians with regard to the study of imperialism became evident. Looking at the literature on imperialism written after 1920 one has the impression of a distinction between a purely 'academic analysis' — provided by historians and economists — and a 'political' analysis, where the latter tended to give greater emphasis to relations of power and subordination of the state, so coming rather within the canons of political science rather than those of economics. One can then speak of two different analytical approaches which are introduced into the debate in relation to the respective strengths of the theoretical and political tradition in the various countries under consideration. In Southern Europe (France and Italy) where the debate on imperialism is essentially political, analysis conforms more to the latter. Alternatively where the tradition of economic theory — and hence Anglo-Saxon influence — is stronger it is more usual to examine the phenomenon in terms of traditional political economics by verifying individual mechanisms and general theories.

The present anthology makes it possible to judge the validity of this approach. It should be noted however that the connections with the tradition of Classical political economy are not made explicit. They emerge however in the course of an attempt, which is again implicit rather than stated, to reexamine that tradition in terms of its content.

The book does not concentrate on the constitutional relations between metropolitan country and colonies, but rather on the problems of English capitalist development in relation to falling rates of profit, problem of employment and declining agricultural revenues. It also examines the function of the state in relation to such problems.

After mentioning the book's specific subject many of the questions posed follow naturally. This is certainly the case for the 'Imperialism of Free Trade' the meaning of which is made clear by Gallagher and Robinson in terms of the general development of English capitalism and its particular needs

for growth. In this sense it is clear that the terms 'formal empire' and 'informal empire' are meaningful only in terms of a short period when considered as instruments for the realization of certain ends.

The inclusions of MACDONAGH's article, « The Anti-Imperialism of Free Trade » is to be welcomed. Although it subjects a number of the classifications used by Gallagher and Robinson to detailed examination, it does not substantially modify their fundamental arguments as J. MOORE recognized in his article 'Imperialism and Free Trade in India'. This means that the reader is in a position to draw on a series of critical analyses for factors on which to base a more general judgement.

But, for the reasons given at the beginning, the central part of the anthology is taken up by the articles of KITTRELL « The Development of the Theory of Colonization in English Economics and the Case for Colonization »; of GHOSH, « The Colonization Controversy: R. J. Wilmot-Morton and the Classical Economists »; and of TUCKER, « The Application and Significance of Theories of the Effect of Economic Progress on the Rate of Profit, 1800-1850 ». These are works which all tackle the problem of the relationship between the theory of colonization and classical English political economy.

The first difficulty that one comes across concerns the possibility of establishing a meaningful relationship between these two. In fact, while it is certainly possible to follow through the thread of classical political economy, it would seem to be much more difficult to isolate within this a sufficiently uniform and independent nucleus of texts which would permit one to establish a specific « theory of colonization ». Additional proof of this is given by the Italian economist M. Fanno, among others, in two works of 1905 and 1906 (« *Il regime delle terre nelle colonie moderne* »; « *L'espansione commerciale e coloniale degli Stati moderni* ») — which cannot at once be reduced to a history of colonization or even to supplementary appendices of individual historical and economic attitudes.

One exception, however would seem to be E. G. Wakefield, the only person, perhaps, to whom it is possible to ascribe a real attempt to establish a theory of colonization, although this was expressed in political and ideological terms rather than those of rigorous economic analysis. In relation to this the attempts of J. S. Mill to make clear that 'systematic colonization' was not contrary to the teaching of the classics but was in fact a corollary, are extremely interesting.

In any case it would be difficult to agree with Pappé, who, in his article « Wakefield and Marx », holds that Marx considered Wakefield to be « the most notable political economist of the thirties ». Marx in fact made perfectly clear both the limitations and the interest of Wakefield's argument, suggesting that he would not have found anything new with regard to the colonies had he not been anticipated by other English economists and by Mirabeau *père*. Nonetheless Marx was quick to acknowledge that « ...it is the great merit

of E. G. Wakefield... to have discovered in the colonies the truth about the conditions of capitalist production in the mother-country ».

There still remains the problem of establishing a meaningful relationship between the whole body of classical economic theory and the hypothesis — theoretical rather than political — of colonization. « Generally speaking » — states E. R. Kittrell at the beginning of his article which would seem to give the widest attention to the problem in question — « the modern view is that the classical economists were incapable of recognizing the value of colonies to the mother-country. The reason simply being that the classical theory placed great emphasis on the rate of investment being equal to the rate of savings. Full employment of the nation's resources was thereby maintained. Thus there was apparently no economic need for colonization » (op. cit., p. 46).

Kittrell holds, then, that for the classical period at least, the hypothesis of colonization can be sufficiently reconciled to the premises of economic analysis, given that the real problem was « the political feasibility of the most important goal of the classical economic policy, namely, free trade. The real doctrinal impediment to be faced by those advocating emigration and/or colonization was the population principle of wages ». (op. cit., p. 47).

Whether and to what extent the author succeeds in proving this line of argument would open a discussion that, due to the seriousness and detail with which he discusses the problem, would go beyond the limits of the present review.

In fact it is this series of problems which emerge as the most interesting part of the anthology. In addition to Kittrell's article there are a number of others which treat similar problems, which are both relatively recent and which also avoid a mere repetition of well worn formulae. It is this, in fact, which distinguishes the present collection from other anthologies (such a 'The Theory of Capitalist Imperialism' edited by D. K. Fieldhouse for example), and means that it is able to cover in sufficiently broad terms a major gap in this field of study.

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UNIONE REGIONALE DELLE PROVINCE TOSCANE, *La Toscana nel regime fascista (1922-1939)*, Florence, Olschki Editore, 1971, 2 vols., pp. VV, 787.

The publication of the proceedings of the conference held in Florence in 1969 on Tuscany during the Fascist regime is an important contribution to the history of contemporary Italy. This is due not only to the fact that the papers collected in these two volumes, edited by Olschki, bring into the open (as Giorgio Spini emphasized in his opening paper at the conference on 'Tuscany and fascism', p. 3) the vast and largely uninvestigated problem of fascism viewed from a regional perspective. These papers reveal how

much remains to be reconstructed, beyond purely ideological considerations and essential studies of the structure of political forces, of the organic trauma of Italian society in the twenty years between the world wars. In this sense we feel that the contribution of the economic historians, to whom we shall refer here, are highly relevant, especially because they generally tend to show an awareness of the 'state of research' both in terms of what has been done and what there remains to do.

In his wide-ranging paper on « Material, themes and suggestions for a history of industry in the Tuscan region under fascism (1923-1939) », Mori emphasized the difficulties which anyone intending to reconstruct the development of industry at a regional level under fascism would meet, so initiating a debate which has not been tackled critically until now. The problems are due to the huge quantity of printed material which has not yet been critically sifted, as well as the fact that often provincial archival sources, and those which are related to the history of the major industrial concerns, cannot be used. Moving on from examining printed documents only, he stated that he was aiming at 'an integrated and also detailed reconstruction of the development, plant structure and growth (or contraction or stagnation) of the region's industry, related both to the economic, social, political and cultural factors of the region and to the history of the nation as a whole, in all its complexity' (p. 271), and his paper provided an excellent basis for such an attempt.

After examining the individual sectors comprising Tuscan industry at the end of the First World War, showing that there had been no major innovations since the pre-war period excepting only the 'entry of landed wealth of noble origin into the management, and certainly also the financing, of industrial enterprises' (p. 167), Mori analysed the evolution of industry in Tuscany between 1927 and 1937-40 (dates which coincide with the publication of industrial censuses). He came to the conclusion that during these years « industry in the Tuscan region », in terms of the number of concerns and the numbers employed, and their capacity, « made an increasingly feeble impression on the over all productive backwardness of the nation » (p. 181).

The cause of what we might consider a real reversal of previous tendencies can be found in a decline in investment, which the author explains in the attraction for capital exercised by the North, after the holding and finance companies had appeared. It was also a consequence of the crisis of 1929 for investments and exports of the mining products of the region (this could be studied further by relating the Tuscan situation to the more general crisis for national exports between 1930 and 1939).

Various new factors do emerge in the author's description. For example he describes the development of the chemical industry, in relation to the continual increase of state intervention in support of national finance capital, and so also the strengthening of Montecatini which by 1937 had come to control a considerable share of the region's natural resources (p. 208).

Mori's paper ended with two paragraphs dedicated to the geographical fragmentation of the industrial concerns and to the condition of the working class in Tuscany under fascism. Despite being over-long at times, the paper provides a solid model for studying the regional structure of Italian industry — a model which we hope will quickly be expanded and applied.

Giorgio Predieri's paper on « Tuscany and the corporative organization of the State » proved less interesting. After reviewing in a largely descriptive way, without elucidating the main problems, the stages in the formation of the corporative order, the author emphasized the distinction between ' fascist syndicalism which was indispensable for the Fascist state, and corporativity, which was never effected ' (p. 42). But he failed to establish any of the causes of this failure and restricted himself to describing a typical example of the inadequacy of the corporative system at a local level, taking the institutions of the industrial Apuanian zone.

Emilio Sereni's paper analysed the work of Arrigo Serpieri and various aspects of Tuscan agriculture under fascism (« Tuscan agriculture and the *mezzadria* during the fascist regime and the work of Arrigo Serpieri »). While recognizing the consistency and reformist intention of Serpieri, who was professor of agrarian economics at Florence and then Under-Secretary at the Ministry of National Economics, Sereni argues that both his attempts to consolidate the interests of the large land-owners by means of the ' Battle for Wheat ', and his attempts to rationalize methods of production through a programme of wholesale land reclamation in which, alongside the state's financial participation, the landowners would contribute to works which concerned them, failed by and large. The lack of any commitment to reform on the part of the latter group was unquestionably the major factor in the failure of the reclamation programme, as Serpieri pointed out and did not fail to condemn unhesitatingly. But Sereni argues that the underlying causes of this must also be sought in the contradictions within the Italian economy during (and after) the fascist period, and especially in agriculture. Non-existent or insufficient capitalist development necessarily led to the survival of feudal or semi-feudal forms of income ' which only the non-application of the obligations projected in Serpieri's reform programme could have guaranteed ' (p. 328). The *mezzadria* in Tuscany is approached in the same way. Sereni explains the fascist policy toward the share-croppers in terms of the contradictions within the regime's agrarian policy — while they attempted to launch Serpieri's reforms, they also blocked the better-off share-croppers' attempts to gain ownership of land, through the economic policy which (as in the case of financial revaluation) reduced agricultural prices and so worsened the share-croppers' living standards. This was confirmed even more precisely in the two papers by Franco Catalano (« the Tuscan Apennines between 1920 and 1930 ») and Reginaldo Cianferoni (« The income of the sharecroppers of the province of Florence during the fascist regime »). The information col-

lected by the latter shows, in particular, that the income of the sharecroppers in the province of Florence constantly decreased between 1924 and 1939 with the result that half of the tenant families were forced 'to cancel outstanding credits and become indebted to the concessionaires' (p. 503).

Finally the paper by Bando Bortolotti (« Building and fascist housing policy with special reference to Livorno ») is devoted to the question of building and real estate under fascism. Although limited to the example of one city, this provides an interesting picture of the complex relations between house owners and tenants under fascism, relating them to the regime's housing policy. Bortolotti's research (which was based largely on the periodical publications of the various groups involved, shows that once the initial phase in which house owners were rewarded for their support of fascism by means of the abolition of the freeze in rents (1923) was over, the regime attempted to juggle a series of contradictory demands — the need to keep prices stable, to revive the building industry, to organize the increasingly numerous groups of immigrants into the towns and then, after 1928, to contain the exodus from the countryside so as not to compromise the objectives of the agrarian policy. There were a number of changes in the fascist housing policy, the return to the freeze in rents, various programmes for slum clearance and for the construction of working class suburbs. These programmes did not always succeed, and when they did it was because they conformed with the new interests that building speculation and industrial changes (in the case of Livorno) had created for the traditional owners of urban property.

Of the papers which concerned non-economic subjects those by Ernesto Ragionieri on « The fascist party — notes for research », and by Roberto Cantagalli on « The first split in the Florentine *fascio* » (October 1920-February 1921) deserve special mention.

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