
REVIEWS OF BOOKS

- B. BELASSA (ed.), *European Economic Integration*. Contributions on Economic Analysis N. 89. Amsterdam: North-Holland, 1975, Pp. XXVIII, 416.

This book is the end product of research in European economic integration organized by Bela Balassa. As editor, Balassa offers an overview and three surveys: one each on empirical estimates of trade creation and diversion in the EEC, on monetary integration, and on "structural policies". The first survey is accompanied by papers by Truman and Resnick and Truman using two methodologies to estimate trade creation and diversion. The survey on "monetary integration", which really means policy coordination among countries, uses Resnick's linked EEC econometric macro-model, presented in the two preceding chapters, to assess the costs and benefits from and consistency of policy coordination. The third survey, largely descriptive, is followed by Thorbecke and Pagoulatos' assessment of the effects of integration on agriculture in the ECC, and by Kreinin's paper on European integration and the developing countries.

Most of the work in the literature on estimates of trade creation and trade diversion in the ECC involves a residual imputation methodology: projections of trade made under the assumption of "no change" in shares are compared with actual trade, and the difference is attributed to tariff changes. The most satisfactory example of this approach is the work of Truman in Chapter 2. A key feature of Truman's methodology is that he analyzes changes in domestic as well as those in partner and non-partner

shares in "trade". This tripartite share division is the only theoretically correct way in which to analyze trade creation and diversion.

Residual imputation would be unnecessary with a general equilibrium dynamic model — simulation with and without tariff changes "controls" for the factors that are changing in the background of the residual imputation approach.¹

In Chapter 3, Resnick and Truman (R/T) pursue a methodology closer in spirit to the general equilibrium one — they simulate a multicountry trade model with and without the tariff changes. Domestic activity and prices are treated as exogenous, however, so there is no feedback from the trade model to the domestic parts of the model developed by Resnick in Chapter 4. Thus, domestic shares can change only residually. Furthermore, Truman's use of shares of imports in the consumption of tradeable goods is foregone in the R/T model. It would be desirable to have at least all non-monetary feedbacks present and to use the theoretically correct shares.

Balassa's survey compares and contrasts work that does not satisfactorily integrate these desiderata, and it is likely that the differing methodologies go a long way toward explaining the variance in results. For example, a factor that is important in computing shares is trade in intermediate goods, since gross output is the natural flow level to use in the computation of domestic shares. Balassa describes the work of Prewo, who uses a gravity input-output model in an effort to take into account trade in intermediates in assessing trade creation and diversion. Prewo modifies the usual gravity approach by making trade flows inversely proportional to tariffs as well as well as directly proportional to bilateral supply and demand. However, Prewo adjusts for all other influences on trade in a residual imputation manner similar to that of his predecessors. More significantly, Prewo seems to ignore the elementary point made clearly by Truman: domestic shares are as important as partner and non-partner shares. With his 1-0 approach, Prewo would have been able to compute such shares quite easily.

In Berner (1976), many of the desirable features discussed above are incorporated in a multicountry, multisector model of the Six. Since the methodology differs from the studies surveyed by Balassa, comparison of the results is difficult, but the result for total trade may be summarized as follows (using Truman's terminology): Belgium exhibits double (internal and external) trade creation; France shows double trade *erosion*; Germany

¹ The approach verges on implicit theorizing as a result, although Truman attempts to control for many of the factors that affect trade shares, such as business cycles. Not surprisingly, his results strongly depend on whether or not these are taken into account. No clear presumption exists that cyclical adjustment of shares, for example, accurately accounts for the impact of business cycles on the shares. A large, simultaneous change in tariffs in many countries may in fact generate or contribute to cyclical movement in the economies in question.

has a less consistent pattern of the same type as France (in accord with R/T); Italy shows external trade creation and for some years internal trade creation; and the Netherlands exhibits a similar pattern — external trade creation and more often than not, internal trade diversion. The total share changes are small, but these conceal large reallocations of resources resulting from the tariff changes. The results are more similar to those of R/T than to those of Prewo, perhaps indicating the biases of the residual imputation approach.

The section on policy coordination and monetary integration is flawed by the use of Resnick's macro-model since it does not include financial markets — this is also noted by Sellekaerts (1977). It is difficult to see how one can draw conclusions regarding the appropriateness of an exchange rate regime using a model in which it is assumed *a priori* that exchange rates are fixed, that the residual capital account, not modelled, is completely passive, and that financial variables have no influence on domestic or foreign current accounts.² Relative to these considerations, Sellekaerts' reservations about Resnick's model (many of which I share) are unimportant.³ A more complete model would be more appropriate to answer the questions posed.⁴ Balassa's predictable conclusion is nonetheless important: conflicts in national objectives make policy coordination (and thus monetary integration) more difficult. His contention that floating exchange rates increase risk and *therefore* discourage trade and overseas investment is overly strong in the presence of well-developed forward exchange markets.

The last section of the book discusses a potpourri of policy issues: "structural" policies (Balassa), agricultural policies (Thorbecke and Pagoulatos) (T/P) and tariff preferences toward EEC LDC associates (Kreinin). The T/P paper is most interesting, since it attempts to estimate the effect of the Common Agricultural Policy on trade in agricultural products. It confirms strong prior assumptions about the trade diversionary effects of the CAP, also supported in Berner (1976).

In sum, this volume presents important, if flawed empirical work on various aspects of economic integration in the EEC in particular and on policy coordination in general. The descriptive material is less satisfying, although this evaluation obviously reflects my biases.

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² In fairness, most of the computations for the volume were done prior to 1971.

³ And some are irrelevant and incorrect, such as the assertion that the equality of tariff and price elasticities should be tested — it was tested by R/T, see p. 45 of their paper.

⁴ See, for example, BERNER, *et al.* (1976).

- R. BROWNING, *The Duke of Newcastle* (Yale University Press, New Haven and London, 1975), XIV, 388.
- R. A. KELCH, *Newcastle, A Duke Without Money: Thomas Pelham-Holles, 1693-1768* (University of California Press, Berkeley and Los Angeles, 1974) X, 222, \$ 12.00.

In the more than two hundred years since his death in 1768, there has been no biography of the duke of Newcastle. Since the art of patronage was at the heart of the eighteenth-century constitution, and since the duke was certainly the most notorious, if not the most formidable, practitioner of that art in his own day, it has been increasingly difficult to understand why so central a figure has had to wait so long for what is apparently only his historical due. Since the advent of Sir Lewis Namier's work, moreover, the understanding has become even more difficult because the reputation of the duke has been gradually rescued from both the acerbities of his very readable contemporaries and the travesties of the latter-day Whig historians. Sykes, Owen, Brooke, Beattie and Namier himself, among others, have all tempered the received portrait of Newcastle, and while the duke has not emerged either spotless or even admirable from their work, he is no longer either the corrupt or reckless representation of pre-Namierite mythology.

Why Newcastle has had to wait so long for his due, however, becomes readily apparent on reading these biographies of him. Obviously, his fifty years and more (1713-1786), of active political life, unredeemed by mind or will, has been up until now simply more than anyone has been able to sustain. In a word, the duke not only wears less than well; he is impossible to take in large doses. Since Professor Browning's study is both the more comprehensive and ambitious, it would be best perhaps to begin with it. Though Professor Browning demonstrates an eminent desire to be fair to his subject as well as an uncommon perseverance in pursuing him, his biography fails to come to terms with the crucial question of what motivated the duke politically, and even perhaps more important, what prevented him from realizing his political ambitions. Yet, the problem on the political, if not on the psychological level, at least, appears to be soluble in

BERNER, RICHARD B. (1976), *A General Equilibrium Model Of International Discrimination*, unpublished dissertation, University of Pennsylvania.

BERNER, RICHARD B., PETER CLARK, HOWARD HOWE, SUNG KWACK and GUY STEVENS (1976), *Modeling the International Influences on the U.S. Economy*, International Finance Discussion Paper No. 93, Board of Governors of the Federal Reserve System.

SELLEKAERTS, WILLY (1977), *Review of Balassa*, ed. « Journal of Economic Literature », XV, No. 3, (September), pp. 967-970.

terms of the evidence presented by Professor Browning. The key to understanding Newcastle's supreme failure as a prime minister as well as even his partial successes in subordinate office appears to be that the duke always refused to face up to the fact that in a representative system the marriage of power and responsibility was indissoluble. He was never able to say, for example, as did the Great Commoner, William Pitt, on the occasion of his celebrated resignation as effective prime minister in 1761, "I will be *responsible* for nothing that I do not *direct*".

At worst this inability of the duke to come to terms with political responsibility was cowardly and at best it was childish. Indeed, the adjective that occurs most frequently in describing his behaviour is childish. Professor Kelch, whose interest in the duke's career is more financial and personal than political, and whose dose, because it is so much smaller than Professor Browning's, is therefore easier to take, feels that any attempt to explain Newcastle's behaviour must be in part at least psychological. "It appears to me", Kelch argues persuasively,

that it was his child-like insecurity, as evidenced by his overwhelming need for recognition, appreciation, indeed love, which is the basic factor most useful in explaining his life-long patterns of behavior... His basic insecurity always defeated him and the next day he needed to be reassured all over again, for neither ducal rank in the peerage, nor vast wealth, nor high office gave him defenses against his fears.

Since virtually nothing is known, however, about the duke's childhood, any attempt to explain how or why he became a psychological cripple must remain at best tenuous, but that he remained such to the end of his days is made amply clear by Professor Kelch's description of how he dissipated an incredible fortune of some £ 400,000 in his own lifetime.

Where the two studies touch upon each other there appears to be a basic agreement that the duke's financial embarrassments were less the result of large expenditures on politics as such than his incredibly lavish and indulgent style of life. Still this depends on how one defines politics, and even in such a strict age as our own, the duke's accountants could surely have safely justified his several residences and his entertainments as necessary business and professional expenses. There are, however, some differences between the two accounts in terms of what they maintain was the annual income of the duke at his inheritance and what he was finally required to live on at the end of his life. Browning maintains that the duke initially had an annual income of about £ 32,000, of which £ 28,000 and his dukedom was inherited from his maternal uncle and £ 4,000 from his father. Kelch argues that the total annual income was only £ 27,000, of which £ 24,500 was received from his uncle and £ 2,500 from his father. When the doctors disagree, it appears, the patient must needs suffer. Still, the fact, according to Kelch, that the duke's father was worth about £ 12,000 per year appears

to make Browning's figure for what was inherited from the father the more reasonable. What the duke's actual income was, moreover, depends on distinguishing between the gross and the net incomes. Browning does not so distinguish and Kelch argues that his gross figure of £ 27,000 must be reduced by at least a quarter because of the expenses involved in administering the estate and the charges on it to arrive at the net income. Finally, whether the duke had to live on £ 9,000 (Kelch) or £ 6,000 (Browning) in his final years must apparently be adjudicated by further research and study. The supreme irony in all this, and especially for those who like that brand of humour, is that the duke, who was constitutionally prone to weasel whenever it was a question of political responsibility, never attempted to shift responsibility for his financial embarrassments to others, and in the end his financial, unlike his political creditors, did not have to pay a very stiff price for his lack of will.

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J. BURNETT, (ed.), *Annals of Labour: Autobiographies of British working-class people 1820-1920*, Bloomington & London, Indiana University Press, 1974. Pp. 364.

P. THOMPSON, *The Edwardians: The remaking of British society*, Bloomington & London, Indiana University Press, 1975. Pp. XI, 382.

It is fitting that these two books should be reviewed together: they have a great deal in common, and will attract much the same readership. But though the subject matter of the two is similar, the two authors' approaches to it could hardly be more different.

The Burnett book, which appeared in England under the title *Useful Toil*, consists of 27 autobiographies or extracts, divided evenly between skilled workers, domestic servants, and the "labouring classes", a diverse group including semi-skilled factory operatives, farm workers, coal miners, and a sailor. The birth dates of the subjects fall between about 1805 and 1910; the texts range from diaries or accounts set down fairly soon after the events they describe, to recollections of old men, recorded as late as the 1960s.

It is a valuable and interesting collection. More than half of the extracts are here published for the first time and most of the others will be unfamiliar even to specialists, having appeared in out-of-the-way periodicals or obscure books long out of print. Each selection is introduced by a paragraph or two sketching the subject's background and life, but beyond that there is the barest minimum of annotation or commentary. Even more

to be regretted is the absence of a subject index, which would have greatly enhanced the book's usefulness to other scholars.

That is not to say, however, that Burnett's only contribution is to make these 27 autobiographies available in print, even though he is listed as an editor, not an author. He prefaces the book with a brief but penetrating essay on "Autobiographies as history", and introduces each of the three main sections of the book with a substantial chapter discussing that category of the nineteenth-century working class, using statistics liberally as well as verbal description and commentary.

The narratives are too diverse for any brief general conclusion, but two themes run through many or perhaps most of them: the importance of drink and of evangelical Christianity. Often these are conjoined: after a wasted early life of dissipation and lack of purpose, the subject finds religion, leaves off drink, and forms sober and industrious habits which lead to a better life. Often, too, he first learns to read and write at the Sunday school, so that for many of these people religious awakening was the central fact not only of their spiritual and moral lives, but of their material and intellectual lives as well.

A number of Burnett's subjects volunteer that they liked their work, but few write about it in detail as being central or fulfilling in their lives. Most of them convey an "uncomplaining acceptance" (p. 14) of the conditions of life and work. Only one, George Sturt, a skilled wheelwright, writes lyrically of his feelings about the different types of wood and of the traditional skills required to shape and exploit them properly, even when the most back-breaking work was required, as in the saw-pit. Sturt's essay is an interesting and moving piece of microcosmic Burkeanism, but it is not typical. Burnett writes in his preface that his extracts speak for themselves: he would have « the historian, the sociologist, the psychologist and the general reader... draw their own conclusions" (p. 10). Paul Thompson's approach in *The Edwardians* is just the opposite. His book is a sociological and historical essay based in part on interviews with about 500 men and women born in England, Scotland and Wales between 1872 and 1906. He asserts that his sample is typical of the 1911 census, and does not hesitate, unlike Burnett, to draw the most sweeping general conclusions. The book in some ways resembles Peter Laslett's *The World We Have Lost*, in that there are a good many stimulating suggestions and some fascinating and odd bits of evidence, but also rather too many broad assertions which go beyond what the evidence will presently support. For example Thompson guesses that "a good quarter of Edwardian children" left school harbouring lifelong resentments against their teachers (p. 67). (Would not "a good many" be better than the specious precision implied by "a quarter"?) Again, Thompson tells us that marriage was a true partnership for "probably a quarter" of Edwardian working-class men and women (p. 78). More surprisingly, the

author asserts quite in the face of his own evidence that Edwardian parents though strict, rarely punished their children physically (p. 60). Repeatedly through the book his Edwardians testify to the opposite effect — e.g., her father, recalled a girl of seventeen (p. 66), "...hit me with his slipper, I turned round and got another one"; or again, in another family "... if we did anything really wrong we would get thrashed for it" (p. 129).

This ambitious book attempts not only to discuss details of social history, such as the supposed uniqueness of child-rearing in the Shetland Islands as compared with the rest of Britain (p. 63), but also to relate that level of things to the great political struggles of the period — Irish Home Rule, the women's movement, labour troubles, and the rest. Here Thompson's viewpoint is strongly Socialist — e.g., he dismisses Lloyd George's Old Age Pensions as "crumbs from the rich man's table", and not a real step toward "the nobler civilisation of which (George) Lansbury and his friends dreamt" (p. 251). His account of the politics of Asquith's time and of the effects of the Great War on British society is an odd mixture of the thoroughly familiar and the startlingly novel. In the main his story up to 1914 is not much different from George Dangerfield's *The Strange Death of Liberal England*. On the effect of the war, however, Thompson strongly disagrees with the leading authority, Arthur Marwick. The war, Thompson asserts (p. 280), had but a slight permanent impact upon British society!

One cannot neatly appraise a book such as this, except perhaps to say that it is at once unsatisfactory and valuable, and that it should be read.

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O. CRISP, *Studies in the Russian Economy Before 1914*, New York, Barnes and Noble, 1976, pp. XII, 278.

This is a collection of journal articles, symposia papers, and book reviews by Crisp which appeared from 1953 to 1973. They have been brought together for the convenience of students and scholars who have an interest in Russian economic history, but have not followed the wide range of Slavic and economic history journals encompassed by these works.

Chapter 1 provides an excellent overview of a number of issues in Russian industrialization from 1700 to 1914, including the sources of Russia's backwardness, the relative importance of induced and autonomous development, the effects of emancipation on industrialization, and the role of *kustar* industry.

Chapter 2, which bears the somewhat misleading title, "The Russian Economy Under Serfdom", is a composite review of books dealing with industrialization before the 1861 emancipation and, to a lesser extent, with

serfdom. While such commentary is needed in this field, the chapter is not well unified and ends rather abruptly.

The following piece on the state peasants under Nicholas I (Chapter 3) is one of the older articles (1959), but one which deserves to be re-read. Even serious students of Russian history are inclined to forget that the rural population was not a single undifferentiated mass and that some 40 percent were not serfs of private landowners, but state or Crown peasants. Dr. Crisp provides an excellent elucidation of the status of this group in comparison with the serfs of the nobility, a discussion of the reforms introduced by Kiselyov and convincing arguments that the retarding effects of serfdom on Russian industrialization have been exaggerated.

The remaining five chapters are linked to one another by a common time frame (the post-emancipation period, especially 1889-1914) and by two common and interwoven themes: 1) financial institutions and policy and 2) French investment in Russia. Chapter 4 traces the efforts of successive finance ministers (Reutern, Bunge, Vyhnegradsky, and Witte) to reform the Russian monetary system and introduce the gold standard. Chapter 5 provides an excellent overview essay on the role of the Russian banking system in industrialization after 1860, with particular attention to the characteristics and activities of the State Bank and joint stock banks.

Chapters 6 and 7 and the accompanying addendum to them form a sub-unit with a closely knit theme of French investment and influence in Russian industry during the period 1894-1914. This investment was heavily concentrated in basic industries (e.g., mining and metallurgy) and, after 1900, in Russian banking. This reviewer was particularly interested in Dr. Crisp's discussion of the French role in the creation and operation of two important Russian syndicates — *Prodameta* (metallurgical industry) and *Prodogol* (coal), although her attempts to assess their impacts on the economy at various times are not well documented. The profession could profit from more research on such syndicates.

The addendum to Chapters 6 and 7 expands the theme of foreign entrepreneurship through a review of John McKay's book on this subject (*Pioneers for Profit*). This is not entirely inappropriate, but somehow the review manages to end with Fred Carstensen rather than McKay. (Those who have met John and Fred may not be surprised at such an outcome).

In chapter 8 the author discusses the Russian public debt which was held by the French and attempts to straighten out some of the data on the size, composition, and yield of this debt. Especially interesting is Dr. Crisp's downgrading of the importance of Russian payments to the French press for favourable reporting on the Russian economy.

The book ends with a short concluding section, an excellent bibliography, and an index.

This reviewer has long been impressed with Dr. Crisp's work and

was pleased to have these essays (several of which he had missed) collected into a single volume. But at the same time, the volume seems to fall short of what it might have been with more editorial work. The Introduction highlights the aim of confronting traditional interpretations of Russian economic history with "disconfirming evidence" (p. 3). This is done in Chapter 3 when Crisp argues against the traditional view that serfdom seriously hampered industrialization, but elsewhere this theme is forgotten. As an example, with much of the world now on floating exchange rates, Crisp missed an opportunity to confront the fixed vs. flexible exchange rate question in her discussion of the Russian adoption of the gold standard.

Of course, the volume is a collection of articles written over two decades and it was not promised that they would "add up to an economic history". But at the same time, more unity could have been gained by selective rewriting, by inserting additional footnotes, and by the addition of unifying essays. In the last regard, Chapter 1 was excellent, but the "General Conclusion" was disappointing. Finally, this reviewer was fascinated by the idea of republication of book reviews, but thinks the concept is unlikely to catch on unless they are rewritten.

All things considered, this is a valuable volume. Readers can only hope that Dr. Crisp's next twenty years will be equally productive.

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F. HIRSCH, *Social Limits of Growth*. Cambridge, Mass: Harvard University Press, 1977, pp. IX, 208.

One might question the propriety of including a review of a work on socio-economics in a journal devoted to European economic history, but I feel that such an inclusion is justified (1) because the study under consideration derives its evidence from history, (2) because the book brings a message of utmost importance to Western society, and (3) because it comes from the pen of an extremely analytical and fertile mind. The theme of this homily may be stated very succinctly: — as men have increased their supply of worldly goods, they have become less content with the sheer accumulation of more and more material things and have sought personal satisfaction by attaining other than economic positions of status in society. By their very nature, such positions are in short supply and thus competition for them is extremely keen. In fact, striving for them may lead to strife for them, and strife leads to curbs on the struggle for them. Then society places restrictions on the realization of personal predilections and thus threatens the continuation of the *old* liberalism, even as a utopia.

Most of the volume is devoted to examples of the limits of satisfaction

to be derived from mere economic growth. The author's illustrations are taken from many facets of life — from education (prestige schools) and recreation (golf clubs) to mating (the daughter of a person of status) and lovemaking (a celebrity mistress). Cases of evidence to support the theme come to mind so readily that there is no reason why the point should be belaboured here.

Fortunately Mr. Hirsch includes in his study a chapter entitled "Inferences for Policy". Here is a programme of action for those who would do something about the problem he raises. "We may be near the limit of explicit social organization possible without a supporting social morality". As one who has been concerned with the issue before us, I can say that this work goes beyond anything with which I am acquainted.

If I might enter two caveats in my enthusiasm, albeit minor ones, I would say that the author does not put enough stress on the observation that so many who acquire wealth do not know how or are unwilling to use their purchasing power either for their own good or for that of society, as in the case of drug addicts, alcoholics, and, in general, all degenerates. Secondly, I would observe that the writing contains too many recourses to the *patois* of economics — that the message would have carried more force if it had been presented in simple English. The Twentieth Century Fund, the sponsor of the study, might be well advised to consider the publication of a more popular version of this testament. The populace needs the sermon.

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G. W. IRWIN, (ed.), *Africans Abroad: A Documentary History of the Black Diaspora in Asia, Latin America, and the Caribbean During the Age of Slavery*, New York: Columbia University Press, 1977, pp. XVI, 408.

Graham W. Irwin, director of the Institute of African Studies at Columbia University, has put together one of the more interesting of recent readers on involuntary emigrants from Africa. He has provided a mix consisting mainly of documents and writings from earlier times but with a few recent commentaries dealing with Africans in various parts of the world since ancient times. Two main areas are ignored: Europe because of the relative paucity of material and North America because of the abundance of readers already available. Although more than one-half the book deals with Africans in Latin America and the Caribbean, the most novel aspect is the attention paid to Africans in the Ancient World and in Asia. There are relatively brief discussions of Africans in Greece and Rome and in China, a longer section on Africans in India, and about 100 pages of selections on the Middle East and the Islamic world. Several of the source materials in these sections are translations from exotic contemporary sources, and, combin-

ed with the editor's introduction, will provide a good introduction to many scholars on slaves and slavery in these areas. The selections here include the two longest in the book, one from Bernard Lewis' *Race and Color in Islam* (which is easily accessible and seems unnecessary here), and the other an informative late nineteenth-century article on the great Zanj revolt in ninth century Irak, which lasted for fifteen years and apparently involved a slave fighting force of over 50,000.

The book's 80 selections are of quite varying lengths, from a few sentences to 22 pages, with most tending to be on the short side. In content they range from "success stories" of black slaves in the Islamic World, to descriptions of the plantation regiment in Barbados, Jamaica, Cuba, and Brazil, and to recent writings on the nature and impact of racial attitudes in different societies. There is material on maroons, slave revolts, and free blacks in Latin American and the Caribbean, in addition to a longer section on the operation of the slave economy there. The nature of the introductions varies: in the first part providing annotation for each selection, while in the second there are rather brief descriptions of broad topics. In general those in the first part are more useful for those not familiar with the subject, while those interested in Latin America will find things a bit sketchy and will probably follow the editor's suggestion and refer to sources in the useful bibliography. It is seldom clear for whom books of this type are intended, since they fit into few standard courses, and the excerpts are frequently too brief to meet the needs of the interested scholar. Yet because of the coverage of black slavery in areas with which few will be familiar, it does draw attention to quite neglected issues of importance, both in their own right and as a basis for comparisons with more recent periods and those areas which have been better studied. Thus *Africans Abroad* makes a most interesting documentary history for those studying the black experience under slavery.

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T. H. LLOYD, *The English Wool Trade in the Middle Ages*. Cambridge: University Press, 1977, pp. XI, 351.

This volume provides us with the first full history of the political economy of the foreign wool trade of medieval England. Most of the topics dealt with by T.H. Lloyd may sound familiar enough. But never before have these been presented, as they are here, in full chronological scope "the growth of the Flemish connection", "the end of the Flemish ascendancy", "the Italian hegemony", "the English triumphant", "Edward III - woolmonger extraordinary", "quest for a staple policy", "the evolution of the Calais staple" and "the decline of the wool trade".

This is traditional British historiography at its best. No paragraph is far removed from a reference to some public record. Indeed, comments upon the degree of information available in these various records give this volume an intrinsic value as a critical index to the potential of such sources. Speculative analysis is kept to a minimum throughout these studies. Not that T.H. Lloyd eschews traditional points of controversy. Rather, in every section these are dealt with directly. If, as in the question of the presence of the Hanse in London in the early thirteenth century, Lloyd finds no solid evidence, he disposes with the issue peremptorily as speculative (p. 23). On other points, such as Unwin's characterization of the 1341 contracts as "monopoly profits" for the king, Lloyd brings to bear upon the point a full analysis of the elements involved in these indentures. The neat concept of monopoly profit gradually dissolves as the various elements of advanced pricing, delivery costs, employment of merchant expertise and merchant recovery of royal debts are explicated (p. 168).

Given this critique of Unwin, economic historians may be somewhat confused by the fact that Dr. Lloyd continues to employ the term "monopoly" for the policies of Edward III (pp. 186, 190). However, this is not shoddy or uncritical use of language. One may point to the concluding pages of this book as evidence for the author's capacity for excellent economic analysis. At the same time, the main contribution of this study in the opinion of this reviewer, is the degree to which Lloyd can posit political explanations for various problems of long-standing economic interpretation. In this fashion, prolonged conditions of war force the decline in importance of Flemish merchants, the private company (so Unwin and Sayles) of 1343 remind us of Powicke's "community of the realm" as they become representatives of the merchant community as a whole (pp. 195 ff.), the staple was at Bruges until 1348 for purely political reasons (p. 202), political relations between England and Flanders are more important than anti-monopoly resentment (205 ff.) and so with the establishment of the Calais staple (210 ff.), parliamentary considerations about overseas trade from the 1370's (225 ff.) and the interpretation of mint competition (246 ff.). On many of these issues Lloyd performs an immensely useful service to scholars by his reconstruction of the actual administrative steps taken by the government of the time. Although some of his suggestions are admittedly tentative and must stand the test of alternative theses, on the whole his description of administration remains clear and convincing.

Economic historians may well be perplexed by the fact that wool trade means simply foreign trade for the methodology followed by Lloyd. The tradition may be traced through Oxford Histories of the thirteenth and fourteenth centuries (Powicke, McKisack) where domestic trade receives but a scant paragraph or two in chapters on trade. As a result, Lloyd can proceed with this story of trade without necessary reference to domestic supply and

demand. The valuable work on Cistercian production, for example, still leaves unanswered the place of the older Black Monk establishments in the trade. Edmund King's recent study of Petersborough indicates the high degree of specialization that might be found in these older organizations. To proceed further in this direction, it would be interesting to superimpose Lloyd's map of licensed English wool exporters (p. 52) on a map of all major markets and fairs in England. Or again, recent revisionist hypotheses by Paul Harvey and Edward Miller compel us to ask whether we know enough about the technology of domestic cloth production and inflation to feel confident that foreign trade can be assessed on its own terms. Granted the usual assumptions of population increase over the twelfth and thirteenth centuries foreign trade may indeed have been disassociated from domestic cloth and wool trade dynamics. Or, perhaps we do not know enough about regional trade to assess the significance of foreign trade, if any, in the pre-industrial economic structure of thirteenth-century England. After all, it was the region and not the specialized wholesaler (such as the Cistercian monastery) that developed cloth production to the take-off level of international competition.

These points are not raised to criticize T.H. Lloyd for the book he has not written. Rather, it is hoped that Lloyd will complement this splendid volume by a parallel study of domestic trade. Perhaps, he has already begun to move in this direction with the final chapter entitled "Marketing the Wool".

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J. P. MCKAY, *Tramways and Trolleys, The Rise of Urban Mass Transport in Europe*, Princeton, New Jersey, Princeton University Press, 1976.

This book is a study of the origins, development and effects of the electric street railways built in European cities between 1890 and 1970. The author has concentrated his attention upon questions which he considers to be of importance with respect to implementation of technological innovation. Most of the discussion is based upon information pertaining to France, Britain and Germany: during the 1890s, and probably also during the subsequent decade, roughly three-quarters of the electric tramway track mileage built in Europe was built in those three industrially developed countries. It is hardly surprising that the geographic scope of the book is not as comprehensive as its title might suggest, given the magnitude of the subject, the novelty of such an undertaking, and the incompleteness of the available information.

The book commences with an historical outline of pre-electric public transportation and an account of the early technical development of the electric streetcar up to 1888, the year of Sprague's successful work at

Richmond, Virginia. The author then turns to the adoption and diffusion of the new technology in Europe, in which he discerns a significant pattern:

a. Public and official concern over the visual impact of overhead wires in city streets delayed the introduction of electric tramways in European cities by periods ranging between five and ten years;

b. that concern led to the development of several alternatives to the overhead system of power pick-up;

c. although no economically feasible alternative was found, the European electric tramway networks were designed with far more attention to aesthetic quality than their American counterparts.

In the middle section of the book the author considers institutional mechanisms of adoption of the electric streetcars in France (taken as representative of Continental countries), and in Britain. In the case of France, he argues that strong competition between the large tramway investment companies placed the municipalities in a strong bargaining position, which enabled them to negotiate franchise agreements embodying most of their aesthetic and social objectives. In Britain, on the other hand, national legislation enacted during the 1870s for the regulation of horse tramways proved so unfavourable to private investment in the new technology that electrification did not begin on a large scale until the very end of the century — some five years later than in France — and then mainly under direct municipal management and ownership. Yet that development, once begun, was hardly less swift than it had been in France, and the tramways as built in the two countries seem to have been similar in most respects.

A final chapter is devoted to a discussion of some of the social changes wrought by the electric tramways. "In the first place", as the author stresses, "public transport simply became a much more important factor in the lives of European urban populations". *Per capita* utilisation of pre-electric mixed-traffic modes — the horse omnibus and horse-drawn streetcar — had been stagnant for two decades; the lower fares made possible by electric traction opened up a vast new market. *Per capita* transit use in European cities doubled or tripled immediately following the construction of electric tramway networks; moreover, many of the new riders were working-class people who previously could not afford to travel regularly. Hence the term "mass transit revolution".

Municipal governments were well aware that momentous changes were afoot, and attempted to use the electric tramways as agents of social improvement. These efforts were embodied especially in special "workers" fares, and in the building of initially unprofitable outer suburban tramway lines. The suburban lines were intended to enable workers to live in salubrious housing at some distance from their places of employment, and so to alleviate the squalid and overcrowded condition of inner city slum residential areas.

Their impact on the low-income housing market was less dramatic than had been anticipated, but they did play a significant part in promoting residential decentralization.

In attempting a trans-European treatment of this critical stage of tramway development, the author has gone far towards correcting the local and technical bias in the tramway literature. The "tramway revolution" which he has elucidated is a significant event not only in transit history, but in the history of technological innovation, of economic development, and of cities; and the book surely will be of great value to students of these subjects. The arguments are presented clearly, systematically and forcefully; to support them the author mustered information from a wide range of sources, including previously unpublished archival material on the French and Belgian tramways. The few photographs in the book are well chosen to illustrate the discussion of tramway technology and installation, although the quality of reproduction is rather disappointing.

Our main criticism of this book is its neglect of the ideological aspects of public policy: the author deals with political controversy as if it had no life or logic of its own. This is evident especially in his treatment of the British tramway "municipalization" movement, whose significance he seems to underrate. Municipalization first established public transport as a *sine qua non* of urban life and so helped to lay the ideological groundwork for the normative multimodal approach to transportation planning which has been successfully implemented in several European countries during the past 20 years.

MARK HORN and VUKAN R. VUCHIC
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TH. MCKEOWN, *The Modern Rise of Population*, New York and San Francisco, Academic Press, 1976, p. 168.

The author's purpose, stated in his opening paragraph, is to explain the remarkable increase of world population which began in the late seventeenth or early eighteenth century and has continued to the present day. It must be said that his achievement falls short of that objective, but what the work does to elucidate causes of the modern decline of mortality in England and Wales is enough to qualify it as an important contribution to the literature of demographic history.

The most valuable feature is an admirable analysis of the English statistics of causes of death since their beginning in 1838. Results of previous studies by the author and his associates on segments of this statistical record are brought together and supplemented to provide a comprehensive view of

cause-of-death components of the mortality decline over a period of a century and quarter. The analysis makes it plain that only a relatively small share of the decline during this period as a whole can be put to the credit of medical measures, defined as immunization and therapy for diseases. It was not until the 1930's, McKeown argues, that such measures began to take on major importance, and even since that time he doubts that they have been principally responsible for the continuing reduction of mortality from infectious diseases. More importance is assigned to improvements from the second half of the nineteenth century onward in water supplies, sewage disposal, and food hygiene, and since the beginning of the present century in domestic, working, and general environmental conditions, whereby exposure to infections was progressively reduced. But above all, in McKeown's view, it was strengthening resistance to disease, thanks mainly to better nutrition, that enabled the English people to nearly double their expectation of life from the 1840s to the 1970s.

In the eighteenth century and earlier decades of the nineteenth, prior to the beginning of the national record of vital statistics, the trend of English mortality is less clearly defined and causal interpretations of apparent declines are more speculative. McKeown holds improvement of food supply and nutrition to be the only tenable explanation for a substantial reduction of mortality during this period. He discounts possible effects of other factors to which some writers have attributed importance, such as smallpox inoculation, improving medical practices, and natural changes in relations between infective organisms and the human hosts. While his arguments for denying that the latter factors could have been very influential may not be fully convincing, they are much better substantiated than the postulate that the state of nutrition of England's population did, in fact, improve during the eighteenth or early nineteenth century. No evidence of this is cited.

The trends of mortality in less developed regions of the world are scarcely considered in this book, although it is in those regions that most of the modern rise of population has taken place, in circumstances very different from those of England. There is no discussion of the precipitate decline of death rates in many developing countries during recent decades, achieved in spite of malnutrition, poor housing, and adversity in other material conditions of life. The findings of Stolnitz, Preston, Newman, Gray, Fredericksen, Mandle, and others with regard to the contributions of disease-control technology, medical services, and economic advances to declining mortality in developing countries are not considered.

A lucid summary of theories of the evolution of disease in human populations since the days of the stone-age hunters and gatherers is another valuable feature. Less satisfying are the author's depiction, in uncompromising Malthusian terms, of mortality as the stern regulator of population and food shortage as the underlying cause of generally high mortality in past ages, and

his rejection of the possibility that premodern societies could have controlled fertility effectively.

On the whole, the work is better grounded in medical history than in demographic, economic, and cultural history. It suffers from insufficient attention to anthropological literature on the demography and economy of primitive peoples and to the findings of research in European historical demography, as well as the studies of recent mortality trends in developing countries. Despite its shortcomings, it bears repeating that this is a valuable and welcome addition to the demographic library.

JOHN D. DURAND
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K. OHKAWA - H. ROSOVSKY, *Japanese Economic Growth*, Stanford, Cal. Stanford University Press, 1976.

The meteoric growth of the Japanese economy since World War II has long excited students of economic history and development, not least because of the possibility that a study of Japanese growth might yield clues to a process by which the growth of many of the poorer developing countries might be accelerated. Particular interest is added by the wealth of recorded data that has allowed scholars to trace the growth of Japan in detail. The pioneering work of Ohkawa and Rosovsky in the development of the historical statistics that have done much to illuminate the early stages of industrialization in Japan, is well known.¹

A short summary description of the state of Japanese development at the beginning of the century opens this book, and the narrative then proceeds to an analysis of the long-run profile of economic growth from about 1887 through 1969. The pre WW II data reveals peaks in the rate of growth in the years 1887, 1917, and 1937, from which the existence of "long-swings" lasting about 20 years is inferred. The book then expands a view of the causes of Japanese economic growth explored in an earlier paper,² which assigned an important role to an apparent acceleration of both the long-swing and secular trends in the remarkable growth of the Japanese economy since World War II. The explanation of this phenomenon is based on "four concepts and four preliminary assumption".

¹ KAZUSHI OHKAWA et al., *Estimates of Long-Term Economic Statistics of Japan Since 1868*, 10 vols (Tokyo: The Oriental Publishing Co.). HENRY ROSOVSKY, *Capital Formation in Japan, 1868-1940* (New York, 1961).

² KAZUSHI OHKAWA and HENRY ROSOVSKY, *Postwar Japanese Growth in Historical Perspective; A Second Look*, in «Economic Growth, The Japanese Experience Since the Meiji Era», ed. by Lawrence Klein and Kazushi Ohkawa (Homewood, Ill.: Richard D. Irwin, Inc., for The Economic Growth Center, Yale University, 1968).

Briefly, the concepts may be summarized as: (a) the existence of the long-swing phenomenon, (b) the association of the long swings with spurts in private non-agricultural investment, (c) the existence and strengthening of a differential structure in the labour market, which is interpreted as an increasing gap in productivity and wages "between the modern and traditional sectors of the Japanese economy". This wage/productivity gap, insofar as it served to stabilize expected production costs, is viewed as a major stimulatory influence on investment. Since managers do not generally view wage trends in a macro framework, allowing for income effects on demand, a long lag in wage response to growth in Y and profits might be expected to have this result.

The "preliminary assumptions" of the model are (i) the existence of a gap between the available technology and that actually employed in Japan from 1900 at least to the late 1960s, (ii) that the main driving force of observed growth has been autonomous investment based on the available (borrowed) technology, (iii) that the observed pattern of growth reflects a modern-traditional dichotomous economic structure in Japan, and (iv) that the postwar performance of the Japanese economy "is linked to the development and strengthening of forces already in operation before the war".

Few economists will quarrel with the first "concept" and its relevance to this study. Shinohara, in a monograph appearing between the dates of the original Ohkawa-Rosovsky essay and the book under review, has described the postwar growth phenomenon as a manifestation of the upward phase of the Kuznets cycle,³ and has also included a version of assumption (i), noting that high recovery rates, and thus high rates of growth, were associated with countries that had developed gaps through war damage and which were thus subject to "rehabilitation factors". The rapidity of the postwar recovery was, according to Shinohara, at least partially explained by the preexistence of labour force skills appropriate to the reacquired technology. These considerations serve to strengthen the impact of the gap introduced by ongoing technological progress. All of this is consistent with the Gerschenkronian model and with the model advanced by Ohkawa and Rosovsky.

The elapsed time since WWII is now sufficient, according to Kuznets' criterion,⁴ for a meaningful analysis of secular growth and cyclical fluctuations during that period, though it is unfortunate that this book was published just too soon to pick up the economic consequences for Japan of the changes in oil prices and international financial arrangements that opened the 1970s and which were associated with the first major setback in Japan's remarkable postwar growth pattern.

³ MIYOHEI SHINOHARA, *Structural Changes in Japan's Economic Development*, Economic Research Series No. 11, The Institute of Economic Research, Hitotsubashi University (Tokyo: Kinokuniya Bookstore Co., Ltd., 1970), ch. 1.

⁴ SIMON KUZNETS, *Modern Economic Growth* (New Haven: Yale University Press, 1966), p. 27.

The essential thesis of this book, recapitulated in chapter 8, is that the economic forces underlying XXth century secular growth and long swings in Japan have intensified in the postwar period, thus leading to the observed performance. The authors make a distinction between long swings of the "spurt and recovery" type and Kuznets long-period cycles which may be meaningful or may simply reflect the idiosyncratic nature of long cycles in Japan resulting from the special pattern of economic forces outlined in the book. Ohkawa and Rosovsky may perhaps be interpreted as having fleshed out the structure of observed long-period cycles, reminding us of their probable nature as a combination of exogenously triggered upswings coupled with endogenously shaped recovery profiles, and proposing a model for the endogenous response. If this is the case there is, of course, no single frequency associated with any such cycles, observed periods existing primarily as historical artifacts.

The exogenous factor which initiates the long swing is found to be the recognition of a structural "gap" which leads to increased private monagricultural investment. The ratio I/Y was found to have been rising substantially through this century, concomitantly, as might be expected in the absence of a corresponding growth of foreign investment, with the growth of S/Y . The capital-output ratio K/Y was found to be inversely related to I/Y during the spurts, the result of an implementation of economies of scale, the introduction of new capital saving technologies or adjustments to changes in the output mix. Coupled with these phenomena was a long term upward trend in the capital/labour ratio. The authors comment on the importance of such factors in the determination of the residual.

Perhaps as a result of a misreading on my part I found the discussion of the relation between investment spurts and the rate of return on capital rather confusing. The authors use a rate of return based on actual profits and book value of capital, an "average" concept, but investment plans are based on anticipated returns to additions to the capital stock and are, if expectations may be assumed to be unbiased, more closely related to changes in the incremental output-capital ratio dY/dK after adjustment for expected changes in factor costs.

The authors carefully avoid the trap of seeming to assert that trend acceleration is more than an artifact, or that it is likely to be useful in predicting the future pattern of Japanese economic growth:

«What about the future? No one can predict with certainty but we do have reason to believe that at least some of the regularities of 1901-66 will change in the 1970's and 1980's... but one possible change may be mentioned now. The ratio I/Y and the domestic savings ratio have risen secularly and have reached levels of over 30 per cent... it is difficult to believe that these ratios have not reached something very near an upper limit. Whatever happens, our present concern is the explanation of the historical past and not the prediction of future events».

In this context the model's representation as the "ratchet" equilibrium growth path of Figure 8.2 requires careful interpretation.

This book attempts to provide a coherent economic analysis of the principal aspects of postwar economic growth in Japan and is clearly an important contribution to the literature, though the major hypotheses that it presents both deserve and require further investigation. Hopefully such a study will soon be forthcoming.

JACK LUCKEN
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R. T. RAPP, *Industry and economic decline in seventeenth century Venice*, Harvard University Press, 1976, pp. IX-195.

Undaunted by the passage of many distinguished historians over the well-known terrain of Venetian economic decline, Professor Rapp offers a new and deliberately controversial interpretation. His concern is with two linked but separate issues — the structure of Venetian industry and the decline of Venice — although he prefers to superimpose the one upon the other. Concentrating on production rather than on commerce or consumption, and on the industrial labour force rather than on output, Dr. Rapp argues that Venetian inability to withstand international competition was due to the rigidity not of guild structures but of State regulations; and that, although in one sense the loss of overseas markets constituted a decline, it also hid a significant (and successful) adaptation away from export industries to regional production and services.

The basis source exploited by Dr. Rapp is a series of five censuses of guild membership drawn up by the Venetian administration between 1595 and 1960 for purposes of galley service in times of war. From a highly critical and often ingenious discussion of the limitations and potential of this source, Dr Rapp concludes that the censuses offer the basis for an effective structural analysis of the changes in the composition of employment, since the guilds represented two thirds of the workforce. He illustrates the major shift away from the traditional export industries (above all wool, but also soap, leather, glass, etc.) towards employment in a few selected luxury industries still geared partially towards foreign markets (primarily silk, followed by mirrors, printing and spice-trading), but above all towards retailing, construction and food processing trades. He demonstrates the interesting phenomenon of the movement of individuals away from exclusive participation in one guild towards contemporaneous membership of anything up to five *arti*; and the dramatic ageing of guild members, especially in the export-based textile industries. He concludes on the basis of this evidence that the workforce within the guilds displayed considerable flexibility in responding to the

international challenge and that within the economy as a whole aggregate income and the level of employment remained stable.

Then linchpin of Dr Rapp's argument is his identification of registered lists of guildsmen with the level of employment. But, even if one accepts his carefully reasoned estimate of the weight and size of the guild membership relative to the overall population of the city (14 to 18%), this does not prove (as Professor Pullan has noted elsewhere) that to belong to a guild guaranteed constant employment. Indeed there are many indications to the contrary. The evidence provided by Dr Rapp, as well as by Sella and Pullan, of emigration of skilled workers, of the development of textile and other industries outside the capital in the *terraferma*, of the levels of charity administered by the highly sophisticated Venetian poor relief structures, could all be interpreted as indicators of an inadequate guarantee of employment and a growing problem of pauperism.

The crisis of the woollen industry, the backbone of traditional Italian exports, was not peculiar to Venice, but can be seen in other major Italian cities, such as Florence. Equally, the ability of the silk industry to resist the crisis, alongside some smaller highly skilled artisan industries, was not a Venetian phenomenon, but common to the major Italian cities. It would seem to reflect, according to Professor Aymard, a structural change from production for an export market to the provision of luxury goods for a far smaller, wealthy clientele. Such a change, while certainly indicative of a notable adaptive capacity and resilience on the part of the artisans, must have had deep and lasting effects on the composition and continuity of employment. Unfortunately the guild rolls do not reveal this. But two small pointers can be noted. Was female employment so minor an element as Dr Rapp believes, or was it not possibly increasing during the century? Cannot the sharp rise in food processing trades, such as pork butchers, dried fish sellers or grain sellers, be regarded as symptomatic of a growing division within the workforce between the producers of luxury goods and the providers of cheap foodstuffs for a population ever less sure of its level of income?

Dr Rapp seems to me have pushed his evidence too far because of his determination to challenge the orthodoxy about the decline of Venice. His analysis of structural changes in employment seems far from unique to Venice. But this should not discourage readers, because he has provided a wealth of valuable new information and insights about the workings of the guild system, which must shed doubt on the conventional wisdom about seventeenth century guilds as wholly negative, outdated modes of production, in Venice and probably elsewhere in Italy.

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W. W. ROSTOW, *How It All Began: Origins of the Modern Economy*, New York: McGraw-Hill Book Co., 1975, pp. XII, 264.

Professor Rostow has attempted to analyse the processes that led to the emergence of modern growth for more than twenty years. The sustained growth of income per capita and of population that has characterized the Western economies since the Industrial Revolution is in sharp contrast with the economic history of earlier times. In other economic epochs there has been modest growth at best and long cyclical processes of expansion and decline. The changes that have occurred in the relationship between man and his material surroundings and have given rise to modern growth are still only partially understood. The economist's models of growth are of little help in understanding this change; they analyse relatively static economic processes or can provide insights into an economy where sustained growth has become the norm, but they have little to say about the transition to sustained growth. Economists formally account for the more rapid growth by more rapid technological progress; but since the economist defines the rate of technological progress as the difference between observed growth and the growth predicted with static technology, this is unsatisfactory. The explanation becomes tautology. Perhaps, then, history can help.

In this volume Professor Rostow examines in detail the period of European history he has identified as the "pre-conditions for take-off" by drawing on a variety of historical and international comparisons. Early modern Western Europe is contrasted with the classical civilizations of India and China and Britain with the rest of Europe. In the consideration Rostow examines the role of the state, of trade, and of science, invention and innovation. Since other economies have experienced government activity similar to European mercantilism and trade growth similar to the Commercial Revolution without experiencing a "take-off", he concludes that European science must have been central to the European "take-off".

This emphasis on science and technology, of course, represents a change of view since the *Stage of Economic Growth* where capital formation took pride of place. This new emphasis on science and technology surely reflects, in no small measure, the historical research of economists into the history of capital formation and into the nature and implications of technological change. It is disappointing and surprising that this work is not more explicitly acknowledged, for Professor Rostow has otherwise been generous in his acknowledgement of the work of others. To be sure, there are large gaps in economists' knowledge of technological change and Rostow appropriately criticizes the shortcomings of the existing understanding. Surely, however, we think differently and more fruitfully about technological change now than we did a generation ago primarily because of the careful work of a considerable number of economists.

Despite the fact that this is a reflective book of a mature scholar, it is disappointing. There is no exciting new suggestion about how to see the world, or new data and evidence nor an attempt at scientific hypothesis testing. Professor Rostow has in the past made contributions in all areas with his *Stages of Economic Growth*, his contributions to *The Growth and Fluctuations of the British Economy 1790-1850*, and his *Essays on the British Economy in the Nineteenth Century*, respectively. In this book, however, there is little for the specialist who has read the works of Ashton and Tawney.

Professor Rostow has decided that science and technology are the key to modern growth, so one hopes that the discussion of these issues would contain a contribution. Rostow's view of technological changes in the Industrial Revolution is, to my mind, rather old fashioned. He adopts the Schumpeterian view of innovation as a heroic act by a few entrepreneurs and sees the Industrial Revolution as essentially led by cotton, iron and steam. This may be a correct view but there is considerable recent literature, that Rostow does not consider, that suggests that the Industrial Revolution was a more broadly based phenomenon and technological change was widespread. This view gains support from a realization that the growth of the principal industries cannot account for British growth and that the likely linkages from these industries to the rest of the economy were relatively weak. Furthermore, detailed studies of agriculture and various industries suggest that a widespread process of gradual improvement characterized the British economy as a whole in the eighteenth century. My inclination is to believe this latter view, but surely what we need are careful attempts to formulate specific hypotheses and bring the evidence to bear that will enable us to choose among competing views. In short, apply the methods of the "Scientific Revolution". Professor Rostow may feel that he "understand(s) well enough how it all began" but I am unable to share that sense until I have seen specific hypotheses subject to trial within the adversary procedure of scholarly debate. Unfortunately, this book does not make much of a contribution to that process.

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F. B. TIPTON, *Regional Variations in the Economic Development of Germany During the Nineteenth Century*, Wesleyan University Press, Middletown, 1976, p. xiv + 270.

When dealing with XIXth century Prussia or the German Empire political historiography has always been aware of the political consequences of regional

differences in economic development, e.g. the dichotomy between the agricultural eastern and industrial western provinces of Prussia. In spite of the general awareness of this process of regional specialization only a few studies by economic historians have been devoted to this question (e.g. articles by Borchardt, Hesse, Orsagh cited by Tipton). Hence German historiography has long been waiting for an elaborate and thorough study of regional aspects of modern economic growth.

Undoubtedly Tipton's book is a major step in this direction. He uses an impressive amount of literature, especially the highly specialized descriptions confined to a particular region's economic development, which German economic historiography has so richly developed since the rise of the German Historical School of Economics. His quantitative analysis, based on occupational census data, starts with Bavaria, Baden, and Saxony in 1847 and 1849 respectively, and Prussia in 1861. For the entire Empire five censuses are evaluated between 1871 and 1907. These data are arranged very lucidly in a statistical appendix.

When discussing Germany's modern economic growth Tipton does not confine himself to the short span which is covered by his statistical evidence. He rather describes developments since the close of the Seven Year's war thus dating the "beginning of sustained economic expansion in Germany" (p. 143) from 1763. The following 150 years were characterized by increasing regional specialization, although in 1861 only two regions seem to diverge significantly from the national average structure, i.e. the industrial Kingdom of Saxony and the agricultural eastern provinces of Prussia. One wonders, however, why in 1861 no other region, e.g. the Ruhr, revealed a pattern distinguishing it from the national average. To a great extent this irritating issue may be a direct result of Tipton's way of defining regions and of measuring regional specialization.

The smallest unit for which occupational data are available of course influences any definition of regions for which these data are used. In Prussia these data were published for the rather large administrative districts (*Regierungsbezirke*) and in most cases Tipton even combines several districts to form a "region". When measuring specialization Tipton draws upon the widely accepted three-sector model of the economy — agriculture, industry, and services — and distributes occupation among them. In order to obtain a region's degree of specialization the absolute differences between actual and — based on the national average — hypothetical total sectoral employment of the three sectors are added together and expressed as a percentage of total regional employment.

This measurement of regional specialization, however, tends to understate sectoral as well as locational shifts over time due to both the considerable size of the regions and the rather crude three-sector model.

Although the process of industrialization and a shift away from agricultural employment are likely to be considered two sides of the same coin, one should remember that this process historically did not start in a purely agrarian economy. Therefore industrialization also means structural change within non-agricultural employment, i.e. a shift from pre-and proto-industrial secondary activities to modern ones within this sector. If this modernization takes place within the boundaries of a region — a process that is more likely to occur the larger and the more heterogeneous a region is — it is not measured by Tipton's index. This downward bias or conceptual weakness, however, does not cause any serious problems in the positive identification of a region's increasing specialization. The emergence of the Ruhr e.g. is easily seen through occupational censuses for the administrative districts of Düsseldorf and Arnsberg between 1861 and 1882. On the other hand Tipton seems to get caught in the trap of "structural ecological fallacy" when he fails to identify any distinctly specialized regions in the Rhineland or Westphalia before the 1840s or 1861 respectively. But his index of specialization on a provincial or administrative district basis turns out too crude to allow identification of intraregional sectoral and locational shifts. Especially the two administrative districts of Düsseldorf and Arnsberg, which Tipton somewhat misleadingly calls the "Ruhr", could serve as a paradigm for a region with already highly specialized subregions before and at the beginning of modern economic growth. Besides the urban centres with their textile industries and other secondary activities there also existed distinct regions, e.g. the *Grafschaft Mark* where already in 1800 more than 40 percent of the total employment was engaged in industry (*Gewerbe*), mostly in metals. After 1800 this region first declined but later on it caught up mainly through the linkage effects of the Ruhr's emergence in the vicinity. This coincidence of decline and rise within a region cannot but result in merely minor changes in its aggregate measurement.

To sum up, I have no objections in principle to Tipton's methods of measuring regional variations; on the contrary I think that he develops and applies a useful tool for grasping regional differentiation during modern economic growth. When using aggregate measurements, however, conclusions regarding developments within smaller units should be drawn with utmost caution.

Finally I would like to recommend Tipton's book as a standard textbook to every student of XIXth century German history; it is sure to serve as a starting point for detailed research on regional aspects of German industrialization.

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E. WANDEL, *Hans Schäfer. Steuermann in wirtschaftlichen und politischen Krisen* (Deutsche Verlags-Anstalt, GmbH, Stuttgart, 1974), 378 pp.

Although the governments of the Weimar Republic were notoriously short-lived, the Republic did experience considerable continuity of personnel in the higher levels of its administration. Unhappily, some of the bureaucrats involved were holdovers from the old regime and had at best only a marginal commitment to the regime. In other instances, as for example in the case of the subject of this interesting biography, Hans Schäfer, they were talented, hard-working and loyal servants of the new political order. Whatever their political inclinations, however, they frequently played a decisive role in both the formulation and the execution of policy. The high bureaucracy of the Weimar Republic is of great significance to the historian, therefore, on two counts. First, it was directly responsible for much of its development, particularly in the realm of economic policy where governmental measures were instituted and implemented largely by decree rather than through the legislative process. Second, the upper echelons of the bureaucracy had constant access to the stream of ministers and politicians who ostensibly ruled the Republic and were thus in a position, insofar as they chose to record their observations, to provide the historian with valuable and intimate information on the political life of the Weimar Republic.

For these reasons, Hans Schäfer is a particularly worthy subject of a biography... A Jewish lawyer from Breslau, Schäfer was called into the Economics Ministry in 1919, where he rose to the position of Ministerial Director and Section Head before he transferred to the Finance Ministry in 1929 as its State Secretary. He remained in that powerful position until May 1932, when differences with Chancellor Brüning finally drove him to resign from public service and assume a brief general directorship of the famous Ullstein publishing concern. After the Nazis took over, Schäfer went into exile in Sweden, where he played a major role in the liquidation of the Ivar Krueger empire, was active in the rescuing of Jews from Nazi Germany and in refugee work and, after 1945, was often called upon to give advice to the Bonn regime until his death in 1967.

While in the Economics Ministry, Schäfer was responsible for the framing of some of the most important economic provisions of the Weimar Constitution (Article 165), and he served as the ministry's expert on the reparations question during the working out of the Dawes and Young Plans. His involvement in the major internal discussions and international negotiations concerning the reparations problem continued after his transfer to the Finance Ministry, where he also performed important tasks connected with the emergency decrees and budgetary policies of the depression period. During most of his career, Schäfer kept an incredibly detailed diary that not only recorded his views and official activities but also described his

encounters with an extraordinary number of influential colleagues, acquaintances and friends encompassing a wide political spectrum. Although he worked largely behind the scenes, and did so by preference, Schäffer was an important man who knew and cultivated important people, and his diaries and correspondence are one of the most important sources for the history of the Weimar Republic.

This does not mean that Schäffer's biography is easy to write, and any historian who has used the Schäffer papers will find Wandel's study somewhat "flat" because of the author's decision to sacrifice the full range and richness of his sources to general background for his concentration on the reparations question. This is not a very imaginative biography, and it has something of the laudatory tone of a commissioned work, but it is a solid study that should receive the attention of professionals and non-professionals interested in the period. The author is quite convincing in his argument that the deflationary policies of the Brüning government, to which Schäffer was even more committed than the Chancellor, did not arise from any doctrinaire rejection of countercyclical alternatives but rather from the priority given to the ending of reparations through the demonstration of Germany's total inability to carry out the Young Plan provisions. The most fascinating chapter of the book deals with the 1931 bank crisis and provides important evidence on the failure of the bankers to inform the Brüning regime about the true nature of their plight. The study also demonstrates the degree to which Brüning kept his policies and aims secret even from his most important officials, so that Schäffer, for example, was not informed ahead of time about the Austro-German Union scheme. Whatever the consensus among the higher officialdom concerning Brüning's general policy, there was considerable difference of opinion within the government about the modalities of its implementation, and much work remains to be done before the various actions and goals of the Brüning regime in economic and foreign policy become truly intelligible. This biography provides a useful introduction and important clues to these and other important problems of twentieth-century European economic history.

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CH. WILSON, *The Transformation of Europe, 1558-1684*, Berkeley and Los Angeles: University of California Pres, 1976, p. xi, 301.

This is a history of Europe from the death of emperor Charles V to the Peace of Westphalia, two events which the author of this book rightly views as symbolic of the "transformation of Europe" from medieval to modern. For the former marked the end of the dream of a universal

monarchy as well as the failure to restore unity in the Church universal, while the latter signaled the eclipse of the Empire and the Papacy as a major political forces. The immensely intricate story of those ninety years is surveyed in these pages with an eye to the educated layman rather than to the professional historian: footnotes have, accordingly, been kept at a minimum; esoteric questions and unnecessary minutiae have been deliberately avoided; and the style has a lucidity and elegance such as few history books have nowadays. After three introductory chapters devoted respectively to the physical setting, the economy, and the social structure of early modern Europe, Wilson guides his readers through the maze of the French wars of religion, the revolt of the Netherlands, the politics of the Spanish monarchy, the English Civil War and, lastly, the European conflagration known as the Thirty Years' War. In addition, one chapter is devoted to intellectual and art history. Throughout, the complex interconnections between individual national histories as well as between economic, social, political, and religious forces are carefully mapped out.

While primarily intended for a broad audience, Wilson's new book will be read with both enjoyment and profit by professional historians too. Although most of the ground covered in it will be familiar enough to them, scholars will not fail to appreciate the author's mastery of a complex subject, the soundness of his analysis, and the admirable balance he has achieved between detail and generalization. (This last quality is especially visible in chapter 4, "Kings and Nobles", which stands as a genuine *tour de force*). Specialists, of course, may occasionally find fault with a point of detail or question the choice of emphasis here and there: this is inevitable in a book of such scope. Not everyone, for instance, will agree with Wilson's perhaps unduly negative assessment of Philip II as a statesman (p. 57 and 129), while others may regret that intellectual history did not receive a somewhat fuller treatment. On the other hand, all should welcome Wilson's critical comments on major historical issues and controversial interpretations. Here the keynote is one of caution and indeed humility as the author underlines the complexity of events and the many gaps in our knowledge of them. Discussing the price revolution, for example, Wilson reminds us (p. 29-30) of how much is still obscure about that vexed question and how futile it is to single out one cause (be it American bullion or population pressure) to account for it; the historian's task, as he sees it, is rather to try and understand the interaction of diverse forces and pressures on an economy that was subject to severe constraints. On the question of the social involution experienced by eastern Europe in the sixteenth century and epitomized by the introduction and the establishment of serfdom, Wilson warns us (p. 20) against the danger of explaining the whole process as the inevitable result of a "colonialist" collusion between western grain merchants and local landowners; the same process of involution, he pointedly remarks, occurred

at the time in areas of central and eastern Europe which were not affected by a booming grain trade. He is equally critical of recent efforts aimed at explaining seventeenth-century revolts and revolutions purely in terms of socio-economic forces or of the class struggle (p. 211).

The evidence, he convincingly argues, is too complex to admit of any such simple, univocal explanation and "the gap that divides economic and social motive from political act remains as wide as ever" (p. 219). These and similar comments are certainly not going to still debate, nor are they meant to. But they must be carefully pondered at a time when the twin temptations of oversimplification and reductionism seem to be stronger than ever.

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