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integration and its consequences. Since the theoretical problems of economic integration are closely interwoven with most aspects of the theory of international trade, we get in fact a summary discussion and evaluation of this theory starting from specialization and comparative advantages through factor price equalization, optimum tariffs, *etc.* to trade and investment diversion. All this is executed in the clear expository manner that one has come to expect in Machlup's writings. By foresaking the more difficult and involved proofs, Machlup succeeds "in explaining complex economic arguments without the use of a single mathematical equation or of any graphical demonstration. In these days, exclusively verbal expositions of economic theory are not easy to come by" (p. x). This makes the book very readable for a wide circle of economists and practitioners, though a basic knowledge of economic and international trade theory will be necessary for a full understanding.

Parts one and two, which together make up one-half of the book, match almost exactly—step-by-step—the printed version of Machlup's address at the Budapest meeting, only that in this volume all arguments are more extensively developed. The third part is new. It is called "The Contributors" and divulges the materials on which the first two parts are built. In five chapters, Machlup lists with all the necessary biographical and bibliographical details the persons and organizations who, as historians, political economists, promoters, men of affairs, committees, and—last not least—as economic theorists, have contributed decisive ideas to the integration problem. Since Machlup seems to have covered all the more important names (at least as far as English, French, and German publications are concerned) and since he provides very useful summaries—sometimes extending to a page and more—of the essential ideas of his protagonists, this part of the book will prove a very useful source of reference for everybody working in this field.

What are the weaknesses in Machlup's approach (if any), which one could point out? I personally am not absolutely happy with the complete separation of the discussion of the ideas on integration on the one hand and the enumeration of the representatives of these ideas on the other. As I mentioned, this unusual approach can be explained by the genesis of the book; but it makes it a bit difficult to see the interrelations between persons, interests, and ideas in the development of thought on economic integration.

On an even more subjective plane, I would mention my impression that Machlup's great virtue of paying attention to the clear and exact use of words sometimes verges on pedantry. (Is it really "unfortunate" (p. 32) that the word "global" in connection with integration sometimes means "worldwide" and sometimes "comprising all activities"? Can we not rely on the context for clarity?) And then, of course, for the person who does not quite share Machlup's belief in the smooth and beneficial working of market mechanisms, there is occasionally room for irritation with regard to certain assumptions and evaluations.

But these are minor criticisms, in particular since Machlup always aims at a fair presentation of different views. The main conclusion stands unimpaired: That this is both a very readable survey and an excellent source of reference.

KURT W. ROTHSCHILD
Institut für Volkswirtschaftslehre
University of Linz
Austria

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040 ECONOMIC HISTORY

The economy of England 1450–1750. By D. C. COLEMAN. London; New York; Melbourne and Tokyo: Oxford University Press, 1977. Pp. viii, 223. $8.00, cloth; $3.95, paper.
JEL 76–0355

If anyone can cram three centuries of English economic history into 200 or so pages, Donald Coleman can. The Professor of Economic History at the University of Cambridge is master of the early modern economy of England and here demonstrates his mastery in lucid prose and balanced judgment. The result is the best survey of a poor and unspecialized country in 1450 transformed into a rich and specialized one by 1750. The intended consumers are British college students, well-prepared by the study of economic history in high school. Other readers, especially non-British non-historians, will have some difficulty absorbing the geographical and political detail that enlivens the book. They should make the effort. The book is real history, a corrective to the fairytale history about early economic growth (or its lack) that economists carry about in their heads.

Many of the themes are Colemanesque. The most characteristic assertion is that the late seventeenth century witnessed a surge (truly, a serge) in industrial growth. Coleman does not tamper with the official date of the Industrial Revolution: he is not attempting to affix his name to another one, like John Nef's of the sixteenth century or Eleanora* M. Carus-Wilson's of the thirteenth century. For this, God bless him. He merely asserts that there was a distinct light before the dawn, not unrelieved darkness. That the light was shielded by tariffs, as he argues, will comfort all who believe in growth through protection. That the tariffs and other features of "mercantilism" were caused less by ideas than by interests, as he also argues, will comfort all who believe that the gradual encroachment of ideas is vastly exaggerated compared with the power of vested interests. That the alleged fuel shortage that drove Britain to coal was local and spotty and doubtful, as he argues again, will comfort all who believe that all generalizations are always false. This is the tendency of the book and of the recent research it uses: distrusting the neat generalization, overturning myths, preferring the fox to the hedgehog, knowing many little things rather than one big thing.

The one big thing that survives is the importance of foreign trade. A third or so of the book is devoted to it. It is false, of course, that a third of English national income before the Industrial Revolution was involved in foreign trade; or that a third of the modernizing sectors were; or even that a third of the exogenous influences on the economy were. The real reason for the overemphasis on foreign trade in the book and in the literature is that statistics on foreign trade are uniquely plentiful—although the temptation to disproportion on this score becomes in the late seventeenth century uniquely weak, so fragmented by political change are the English statistics. A parallel overemphasis on the history of wool textiles can be viewed as either a cause or a consequence of the overemphasis on foreign trade. Until colonial products became important (after the mid-seventeenth century) England's foreign trade consisted of wool textiles.

This one disproportion of the substance is unimportant beside the attractive proportions of the method. The first paragraph declares that the book "tries to look through two different pairs of eyes: those of the historian and those of the economist or social scientist," and the book later carries the intention through. A long if ill-documented tradition, for example, puts the awakening of the profit motive in the sixteenth century. Coleman will have none of that: even "medieval history does not suggest that peasants consistently failed to behave like economic men" (p. 32).

Yet looking through two sets of eyes, as Coleman remarks in a characteristic witticism, entails "some involuntary squinting." Two important errors in economic reasoning underlie much of the book and the literature it summarizes. Both errors arise from a misunderstanding of the budget constraint facing men and nations. The first is the argument that growth in population (independent of the supply of money) caused the inflation of the sixteenth century by raising the price of food, a large item in the nation's budget. This *non sequitur*—which Coleman in fact takes up with commendable caution—mixes relative and absolute prices, confusing a rising budget line with a twisting one. The second is the argument that income was determined by demand. It is implicit in the oft-repeated question, where did the demand to buy a rising supply of food or textiles come from? The reply, of course, is that it comes from the income earned by those very suppliers. If income is spent, as it is in the long run, the budget constraint solves the problem of effective demand. Both errors, then, reflect a misunderstanding of

* Editor's note: Although generally this author is known as E. M. Carus-Wilson, which the reviewer has pointed out, this is a bibliographic journal and, irrespective of common appellation, we choose (in response to consumer requests) to use at least the full first name for citations.

Keynes's message—if "misunderstanding" is quite the word considering Keynes's own tendency to understand it in precisely this way. Keynes notwithstanding, then, Keynesian theory is a theory of business cycles, not growth. To apply the theory of aggregate demand to prices and output in the very long run is to commit an error more typical of economists than historians: an error of timing.

Still, Coleman cannot be blamed for economic nonsense that as often flows from the pens of professional economists as from those of historians and economic historians. And he cannot be blamed for failures of logic in entire literatures of early modern history. His task here is that of the writer of textbooks, summarizing and weighing present knowledge of what Charles Wilson called in his own book "England's Apprenticeship." He has succeeded in it very well indeed.

DONALD N. MCCLOSKEY
University of Chicago

Studies in Roman property: By the Cambridge University research seminar in ancient history. Edited by M. I. FINLEY. Cambridge Classical Studies. Cambridge; New York and Melbourne: Cambridge University Press, 1976. Pp. 212. $9.95. *JEL 76–0932*

Nobody doubts that land had a dominant place in the Greco-Roman economy. Finley himself has elaborated that point in his recent *The Ancient Economy* (1973). The present study deals with a couple of important aspects of this thesis: how rich were rich landowners in comparison with other strata of the population; how scattered and how concentrated was landed-property? What kind of labor was used? What was the relation between investment in urban and in rural property?

The term "investment" has in itself dangerous modern overtones. Finley holds that "investment in land . . . was never in antiquity a matter of systematic, calculated policy, of what Weber called economic rationality" (p. 117). One of his arguments pertains to the absence of a "recognizable real-property market" and of "a profession of estate agent or realtor" (p. 118). As a result "the normal purchase of land . . . was windfall purchase" (p. 119; *cf.* also in the present study p. 4). Finley does not deny that there was constant movement of landed property, but he claims that the general mentality was acquisitive and not productive. The acquisitive approach—so I presume—is supposed to reflect itself in the haphazard way in which new land was acquired: there is no infrastructure, which enables people systematically and on the basis of economic criteria to extend their estates.

An attractive aspect of the book under review, edited by Finley himself, is that several collaborators mildly criticize Finley's views about the non-existence of a land market. Admittedly R. P. Duncan-Jones writes that the "principal means of transfer of wealth appear to have been inheritance and . . . marriage" (p. 12; see also fn. 30); however, others demur. John Crook concludes at the end of his "Classical Roman Law and the Sale of Land" that "the Roman possessing class thought of land . . . as a commodity like other commodities with a realizable cash value" and that "the disposal of inheritances" pointed to "a brisk movement of land (or dispersal of land, or 'market in land') in Roman Society" (p. 83). As to the sale of land, some weight is to be attached to the fact that the Roman agronomists "spend pages advising on purchase of estates, and must have had some audience in mind" [3, Fredericksen, 1975, p. 168]. It is not correct to argue as Duncan-Jones does that "agronomists could scarcely be expected to give advice about what property to inherit" (p. 170, fn. 30). The agronomists reflect the normality of land purchase, but the crucial point missed by both Fredericksen and Duncan-Jones is that the emphasis that they put in their advice *on the economic factors* most probably reflects the utter lack of interest in these aspects on the part of the average landowner (*cf.* R. Martin [5, 1974, p. 267]: "ce sur quoi ils croient devoir insister . . . a toutes chances d'être ce que les exploitants agricoles ne faisaient pas"). Thus there was much selling and buying also, but not exclusively, within the framework of split-up inheritances, but Finley's point about the non-Weberian mentality still holds.

This is not at variance with Elizabeth Rawson's view that the Ciceronian aristocracy was "feverishly engaged in property deals" (p. 85). First, as Rawson points out and Finley himself