

essays here, however, make a persuasive case that the "marginal productivity of capital," even when it is a well-defined concept, is not except in special cases an important determinant of the rate of profit on a long-run full-employment growth path. It would appear that the influence of technology on the rate of profit can only be sustained by banishing distribution of resources from the theory and reintroducing subjective psychological determinations, as general equilibrium theory does. These results of Pasinetti's building on other work of Cambridge economists constitute, in my view, a very great contribution to economic theory.

But perhaps we need to carry the methodological moral of this tale even further. If the context of assumptions turns out to be vital to their significance we ought to be extremely critical of every assumption in these highly abstract models. While Pasinetti is extremely acute in dissecting the marginalists' treatment of technology and saving, he is somewhat complacent about the assumption of full employment of labor. While this is a persuasive assumption for particular national economies with highly developed capitalist production, it is not so persuasive if we look at capitalism as a world system, since on a world scale capitalist production appears to operate with a large margin of unemployed and underemployed labor. A revision of Pasinetti's assumption of full employment of an exogenously given labor force would entail considerable further dramatic changes in the theory of distribution and growth. As Pasinetti writes: "The worst of Malthus' and Ricardo's fears, and the most frightening of Marx's predictions (that real wage rates would be nailed down to subsistence and all surplus above subsistence would be appropriated by the capitalists) . . . turn out to be incompatible with the maintenance of full employment" (p. 102). But whether this observation should lead us to reject Ricardo's fears and Marx's predictions or the assumption of full-employment growth paths with a labor force given outside the accumulation process remains an unsettled question.

Beyond Pasinetti's striking results in growth and distribution theory, I think the methodological hints and examples of this book are extremely important. Pasinetti's emphasis on emergent properties of economic systems, his support of one-way (which he somewhat apologetically and inaccurately calls causal) determinations, his aim of achieving sharp results from simple, strong, assumptions, and his sensitivity to the importance of the axiomatic context for particular ideas (what might be called the gestalt of economic models) are all exciting, and to my mind valuable, corrections to the growing body of economic theory that is formally elaborate but empty of substance.

DUNCAN K. FOLEY

Stanford University

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Lancashire in Decline: A Study in Entrepreneurship, Technology, and International Trade.

By LARS G. SANDBERG.

Columbus: Ohio State University Press, 1974. Pp. xii + 276. \$15.00.

A reader of this *Journal* does not need to be told that in the last decade or so there has arisen a new brand of economic history, with the emphasis on "economic," which has sharply revised the story of economic development in America. He may not be aware that recently the "new" economic history has moved on to other stories, in particular to the story of America's nineteenth-century predecessor as Top Nation—Britain. Lars Sandberg's book, a distinguished contribution to

this growing stream, is directed at the widely held premise in economic history that old industrial countries, Britain then as America now, experience entrepreneurial failure. Since the last quarter of the nineteenth century, when Englishmen awoke to find Germans and Americans overrunning their traditional markets abroad, alleged evidence for the premise has been mounting, and by now it is taken for granted that even in Britain's characteristic contribution to industrialism—cotton mills—British managers dallied in adopting new techniques and were therefore crushed in old markets.

Sandberg's argument is a very different one, and more persuasive. He argues in the first half of his book that the chief examples of entrepreneurial failure in British cotton textiles, notably the slow move from the mule to the ring spindle, are poorly chosen, for the techniques Englishmen adopted were in fact rational in view of the factor prices they faced. He argues in the second half that the natural spread of industrialization and of tariffs to protect it, rather than entrepreneurial failure at home, explains Britain's falling share of the world's market for cotton textiles.

In common with other pieces of new economic history, the reasoning used is often startlingly simple, although it takes a sophisticated economist to cast the issues into a simple form. Sandberg is able to show that the returns to the new technologies of the ring spindle and the automatic loom were lower in Britain than in America, for example, simply by recasting the evidence into the form of a cost-benefit analysis; or that larger investment in new technologies before World War I would have resulted in larger output but also lower profits after the War simply by noting that world prices would have fallen even further; or that Britain's position of comparative advantage in exporting cotton textiles was fragile and finally broken by Japan simply by emphasizing the low costs of transporting finished cloth. The weakness of the old economic history was not usually in collecting facts (although Sandberg develops some new statistical series, notably an index of the quality of British textile exports, on the whole he builds on the work of earlier scholars) but in refining the facts that matter and interpreting them in the light of simple economic theory. The theory in the book, indeed, is occasionally oversimple. It does not really follow from Britain's dominance of Indian imports of cotton textiles before World War I, for example, that Britain needed to bear the brunt of the 60-percent fall in those imports after the War (p. 182), for the market was a world one. Here and elsewhere in the book, especially in its second part, the arithmetic of market shares sometimes dominates the economics of market supply and demand curves. Yet the economics is seldom far below the surface of the argument, and the argument is always lucidly expressed.

If it is the applied economics in the new economic history which distinguishes it from most economic history, it is the history in it which distinguishes it from most applied economics. It is the historian in Sandberg who demands that he devote half his book to gathering evidence on the relevance of *homo economicus* to the British textile industry in the late nineteenth century rather than, as the economist in him would prefer (unless he were an agricultural economist disputing the idiot peasant model in India or Nigeria), simply assuming it relevant at the outset. It is the historian who demands he devote the other half to applying the model so validated to half a century's history rather than to last year's. And, finally, it is the historian who demands he write the book on Britain rather than America, for America's economic experience has been rich, pacific, and democratic—that is to say, from the perspective of Russia or India or Britain, strange in every way. The economic history of America is, certainly, of great interest, but more of parochial than of cosmopolitan interest. If economics other than the

American arc to be understood, their history must be studied, and studied with the tools of modern economics. Indeed, Sandberg's work, part of a growing body of work on Britain tending to the same conclusion, suggests that Britain's economic history in the last third of the nineteenth century is more applicable to America's in the last third of the twentieth: in America now, as in Britain then, the news of her industrial death has been greatly exaggerated.

DONALD N. McCLOSKEY

University of Chicago

Market Signaling: Informational Transfer in Hiring and Related Screening Processes.

By A. MICHAEL SPENCE.

Cambridge, Mass.: Harvard University Press, 1974. Pp. 221. \$10.00.

Today, the tuition per student per lecture at a big private university is around \$10. This is rather humbling; how many lectures do we give that are worth that much? Michael Spence in this lucid and interesting book gives us good cheer. We may be providing useful information, but about our students rather than to them.

His basic idea is that, since hiring and firing are expensive, employers (or anyone buying services from unestablished individuals) would like to know in advance how productive their employees will be. There are two types of information they can use to judge applicants: unalterable characteristics such as race and sex, and characteristics such as education which can be manipulated by the applicant at some cost to him. Spence calls the latter "market signals." Provided that the costs of employee signals are sufficiently negatively correlated with productivity, employers can use them to select more productive people.

This is a call to us as teachers. Suppose firms seeking intelligent drudges make the B.A. a prerequisite to well-paying jobs. If we can make our classes unbearable to the other potential job seekers, only the drudges will get the B.A. and the jobs. The situation will be in "informational equilibrium," because the employer's expectations about what the requirement meant would be fulfilled. Spence makes the interesting points that education may be unproductive in itself and remain useful as a signal; that multiple equilibria are possible (for example, firms might equally well require an M.A. and get essentially the same people, burdening them with the extra schooling); that equilibria persist like putty at whatever requirements currently prevail; and that since disconfirming evidence may never appear firms may maintain different requirements for identifiably different groups such as blacks and whites, even though their economic potential is identical.

Spence shows that if firms compete on signal requirements all firms will use the least-cost discriminating signal and so multiple equilibria will not exist. He implies that such competition is unlikely since the costs of unnecessary education are borne by individuals and not firms, and the firms would therefore have little incentive to compete by reducing requirements. This last assertion seems wrong; in exchange for labor services firms provide both wages and a job. The attributes of the job including its requisite signals are tied to the wages, and equality of marginal product with wages must be interpreted in this wider sense. Thus tuition may be subtracted when there is learning on the job, and in a market where useless M.A.'s are the standard a firm can offer a B.A.-and-lower-wages job and compete effectively for the best new people. Indeed, a better ability to discriminate is an asset that casting and employment agencies rely on for their profitability.

The theory has something to offer in understanding many other economic

phenomena, including guarantees, brand names, internal promotion, certifying agencies, and discrimination. In discussing the case of DeFunis (the student who sued the University of Washington Law School for admission on the grounds that they had accepted black students with lower grades and test scores than his), Spence points out that if the costs of educational signals are higher for blacks, blacks with the same productivity as whites may have less impressive academic records. In this case discrimination is unavoidable at some level—signals will be different if people are chosen by productivity, or productivity will be different if people are chosen by signals (as DeFunis argued they should be). Most people would probably prefer to use future productivity rather than test scores as the criterion of "merit." Unfortunately, there is little evidence of how future productivity varies by group with test scores, and it might be hard even to agree on a good measure of postschool productivity. Thus even if the assumption Spence would use to justify different test standards for different groups were true, it would be difficult to validate.

We could make a quite different argument than the one Spence employs against the use of uncompensated test scores. This argument does not rely on the differential signaling cost assumption. In such cases as professional school entry, union membership, or government jobs, the state (by cash or statute) may be subsidizing positions that have more qualified applicants than vacancies. This subsidy may make "division of the spoils" a more appropriate selection principle than merit, however defined. Every group with political power should get its fair share.

The author presents his many new insights by simple examples, neat arguments, and clear concise prose. Reading this book is a joy.

EMMETT KEELER

RAND Corporation

Late Nineteenth Century American Development: A General Equilibrium History. By

JEFFREY G. WILLIAMSON.

London and New York: Cambridge University Press, 1974. Pp. 350. \$19.50.

In this book Jeffrey Williamson develops a 72-equation, two-region, three-sector, three-factor neoclassical growth model, intended to represent the American economy between 1870 and 1910. Equipped with parameter values obtained through a melange of ad hoc procedures and rough guesses, the model generates 40 hypothetical years of history, telling an economic story which, in the author's view, remarkably resembles the real economic history of this period. Having received high marks from its creator on its ability to reproduce the actual record, the model moves on to tell a variety of counterfactual tales of the same period; histories which "would have been" had this or that factor been different in this or that way. Among the more surprising of the conclusions are these: (1) the major features of American growth are not associated with the closing of the frontier or the rise of financial intermediaries but may be explained most directly by the postwar acceleration in the rate of capital formation and all its ramifications; (2) the fall in interregional transport costs, however, had a major effect on resource allocation and growth; (3) industrialization was primarily a result of the exogenous effect of world market conditions; (4) farmers were fully justified in complaining of "capital starvation" in the early 1870s, but later in the period indicators of farm distress are more than offset by the capital gains on farmers' landholdings.