

Economic History and the History of Economics

Mark Blaug

First published in the USA in 1986 by
NEW YORK UNIVERSITY PRESS
Washington Square
New York, NY 10003

© Mark Blaug, 1986

Library of Congress Cataloging-in-Publication Data

Blaug, Mark.

Economic history and the history of economics.

Bibliography: p.

Includes index.

1. Economic history. 2. Economics — History.

I. Title.

HC26.B58 1986 330.9 85-29840

ISBN 0-8147-1090-5

Typeset in 10/12 point Times by Alacrity Phototypesetters,
Banwell Castle, Weston-super-Mare
and printed and bound in Great Britain by
Biddles Ltd, Guildford and King's Lynn

All rights reserved

Acknowledgements

I wish to thank the following publishers for permission to reprint: Association of Polytechnic Teachers of Economics for "Economic Methodology in One Easy Lesson" in *British Review of Economic Issues*, May 1980; Duke University Press for "Was There a Marginal Revolution?" and "Kuhn versus Lakatos, or Paradigms versus Research Programmes in the History of Economics" in *History of Political Economy*, Fall 1972 and Spring 1976; *Economic History Review* for "The Productivity of Capital in the Lancashire Cotton Industry During the Nineteenth Century" in *Economic History Review*, April 1961; John Wiley and Sons for "The Classical Economists and the Factory Acts: A Re-examination" in *Quarterly Journal of Economics*, May 1959; *Journal of Economic History* for "The Myth of the Old Poor Law and the Making of the New" and "The Poor Law Report Re-Examined" in *Journal of Economic History*, June 1963 and June 1964; *Kyklos* for "Technical Change and Marxian Economics" in *Kyklos*, 13, No. 4, 1960; Macmillan Press for "Another Look at the Labour Reduction Problem in Marx" in *Classical and Marxian Political Economy*, eds. I. Bradley and M. Howard (1982); Oxford University Press for "The Economics of Education in English Classical Political Economy: A Re-examination in *Essays on Adam Smith*, eds. A.S. Skinner and T. Wilson (1975); *Revista di Politica Economica* for "Ricardo e il problema della politica economica" in *Revista di Politica Economica*, 73, serie 3, fasc. 5, mag. 1983; "Entrepreneurship Before and After Schumpeter" in *Marx, Schumpeter and Keynes: A Centenary Celebration of Dissent*, eds. Suzanne Helburn and David F. Bramhall, copyright © 1968 by M. E. Sharpe, Inc. and reprinted here by permission; *Southern Economic Journal* for "Welfare Indices in *The Wealth of Nations*" in *Southern Economic Journal*, October 1959; University of Chicago Press for "The Empirical Content of Ricardian Economics" in *Journal of Political Economy*, February 1956.

Introduction

Economic history — the story of what actually happened — is intimately related to the history of economic thought — the story of what economists thought had happened and was still happening. Sometimes economists got it right and sometimes they got it wrong. The English classical economists were reformers and addressed themselves to all the outstanding policy issues of the day: population growth, welfare relief for the poor, agricultural protection, factory legislation, the public debt, etcetera. The first six chapters of this book explore that theme in some of its dimensions.

The same theme is only too obvious in the writings of Karl Marx, in which the question of technical change emerged as one of the major problems of the day. In three further chapters, the theory of technical progress in both Marx and later economists is examined in some detail.

In the 1870s, economics became professionalised and entered the modern era of marginalist thinking. The “marginal revolution” is the subject of another chapter and the aftermath of marginalism is explored in a chapter on the history of entrepreneurship.

The volume closes with a couple of chapters on unresolved issues in the history of modern economics since 1870, viewed once again in the light of changes in the institutional structure of the economies that formed the background of theorising.

THE POOR LAWS

I am very fond of my two early articles on the Old Poor Law. For one thing the first was written under the most adverse personal circumstances: I was flat on my back with a compound fractured skull and forced to rely for research material on an endless round of graduate students carrying heavy volumes to and from the library; for another the two articles were iconoclastic, created quite a stir, were frequently cited (e.g. Marshall, 1968; Poynter, 1969; Checkland, 1974) occasionally attacked at article-length (Taylor, 1969; McCloskey, 1973) and led to many invitations to address learned gatherings — all heady stuff to a young Assistant Professor making his way through the Groves of Academia. On balance, they have worn well: their findings, at least in the main, although not in all details, have been confirmed by subsequent research (Huzel, 1969; 1980; Baugh,

1975; Wellington, 1984; Boyer, 1985, 1986) and none of the recent textbooks on British economic history during the Industrial Revolution stigmatise the Old Poor Law in the lurid rhetoric that was so common in the textbooks of yesterday.

But that is not to say that I would write these essays in the same way today. The first essay combined theoretical knock-down arguments with suggestive empirical evidence. It is only in the second essay that I got down to what I now consider to be the crucial question: the actual manner in which the Old Poor Law was administered. The older writers I was attacking—the Webbs, the Hammonds, Halévy, etcetera—made the mistake of confusing Acts of Statute with the law that is actually enforced, a cardinal error in this case where the statutes left an indefinite leeway in each parish.

It is rather similar to the notion in modern macroeconomics that the effects of unemployment benefits on the willingness to work can be measured by the length of time for which benefits are paid and the ratio of benefits to wages in previous employment, the so-called “replacement ratio”. But unemployment benefits are paid only to those who are involuntarily unemployed and seeking work. However in some countries (like Switzerland and Japan), the unemployed applying for benefits are required to produce firm evidence that they are actually seeking work, whereas elsewhere benefits are paid to the unemployed simply as a matter of right. It is this element of stringency or laxity in the administration of unemployment compensation, and not the replacement ratio or the duration of unemployment payments, that explains the impact of unemployment benefits on the volume of unemployment.

Of my two critics, it was McCloskey who scored most of the telling points. Taylor's main objection was that I took too narrow a view of the Old Poor Law—emphasising the rural Allowance System and neglecting the treatment of orphans, widows, the sick, the old and the disabled in urban areas—and that I arrived at far too many conclusions on the basis of too few data. But my preoccupation with allowances-in-aid-of wages was no more than a reflection of the main preoccupations of the authors of the *Poor Law Report of 1834*: contemporary opinion was indeed focused on the effects of the Old Poor Law in rural areas. He also convicted me of minimising the extent of indoor relief in 1802, catching a misprint in the original article whereby nearly 4,000 workhouses in 1802 appeared as a mere 400. In addition, he threw doubt on my fundamental division between Speenhamland and non-Speenhamland countries, based on a questionnaire answered by a small sample of Poor Law authorities for the 1824 Select Committee on Labourers' Wages, but neglected to mention the fact that I provided two other classifications of the countries and in any case supplied data on all the individual countries to allow the reader to

form his own classification. On balance, Taylor succeeded in chipping away at some of my detail (as did Cowherd, 1978) without affecting the principal points of substance.

McCloskey, on the other hand, largely ignored my empirical evidence and instead questioned my basic theoretical analysis. He began by doubting my argument that the poor rate, being levied on farmers in proportion to the value of land they occupied, was never fully shifted to landlords in the form of lower rents because, given that the standard lease ran for 7-14 years, sharp year-to-year fluctuations in the level of the rates hindered the full adjustment of rentals to the rate burden. I cited some evidence for the county of Warwickshire where no connection was found between land rentals and rateable values under the Old Poor Law but was unable to clinch the matter because so little is known about land rentals in the crucial years 1815 to 1834. I should have cited Thompson (1907) who reports gross and net ground rents (after deducting disguised profits included in the rent figures) for two large landed estates over the whole of the nineteenth century; he shows rents per acre rising rapidly from 1801 to 1821 and rising less rapidly in the 1820s and 1830s. This suggests that the rising burden of relief payments was not shifted forward from the occupier to the landlord. Be that as it may, the purpose of McCloskey's thesis that the incidence of poor relief fell wholly on landlords rather than farmers is to argue that the system of taxation used to collect the money for poor relief had no effect on the demand for agricultural labour; the analysis of the effects of the Old Poor Law therefore can focus exclusively on what happened to the supply curve of labour.

McCloskey then went on to draw a fundamental distinction between a wage subsidy and an income subsidy and accuses me, quite rightly I now think, of never making up my mind whether the Old Poor Law was in effect a sort of negative income tax guaranteed to provide a minimum income rather than a direct supplement to earned wages. A wage subsidy would have no effect on the amount of labour supplied at a given wage rate (would leave the supply curve of labour the same as before) but would produce a larger volume of employment simply by virtue of larger total wage payments made up of a wage paid by farmers and a wage paid by the parish. But an income subsidy paid to everyone, whether working or not, will reduce the supply of labour (shift the supply curve of labour to the left) by reducing the marginal gain from work and by giving workers an alternative source of income to working; it thus raises wages but only by lowering the volume of employment. On the face of it, the Speenhamland system under the Old Poor Law looks like a guaranteed minimum wage accompanied by children allowance payments. On the other hand, as the amounts paid out in subsidy were invariably reduced as a man's earnings rose and as unemployed paupers were also subsidised if they had large

families, the system amounted in fact to the guarantee of a minimum income, not a minimum wage.

If some parishes practised a wage subsidy and others an income subsidy, the two subsidies in combination might exactly offset each other, yielding an earned wage and a supply of labour identical to those yielded by a free labour market unencumbered by poor-relief payments. In any case, the two types of subsidy pull in opposite directions in respect of the incentives to work and the supply of labour and thus, McCloskey concludes, the task of assessing the economic effects of the Old Poor Law is that of estimating their separate influences. He agrees that it is difficult to tell from the evidence whether there was an employment test for eligibility both for relief in money and for relief in kind. Nevertheless, he produces two indirect and somewhat inconclusive arguments to establish the proposition that the Old Poor Law was predominantly an income subsidy, not a wage subsidy, and therefore concludes that the authors of the *Poor Law Report of 1834* must have been wrong in claiming that poor relief reduced both wages and the efforts of workers: shifting a supply curve back along a negatively inclined demand curve no doubt causes the equilibrium volume of employment to fall but only by raising the equilibrium wage rate.

The distinction between a wage-subsidy and an income subsidy is illuminating and I wish that I had seen it myself in 1961/2 as clearly as I now do. I am still not convinced, however, that the Old Poor Law was effectively a system of unconditional income subsidies, although it is perfectly true that this is what it had become by 1832 when the only widespread method of giving relief was to make grants to large families for extra children. The real difficulty with McCloskey's reasoning is that it is wholly static and concerned with shifting short-run demand and supply curves for agricultural labour, whereas my argument, and indeed that of the Poor Law Commissioners themselves, was a dynamic one in the sense of being concerned with the long-run effects of the Old Poor Law when the economy has become fully adjusted to the Allowance System. Thus, the Poor Law Commissioners were not inconsistent in arguing that the Allowance System depressed wages and that the same system lessened the supply of labour; their view was that it encouraged population growth and so led to lower wages within a decade or two. They may have been wrong about the magnitude of the demographic effect but at least they were not guilty of an analytical inconsistency. Besides, the Allowance System might have depressed both wages and employment even without a stimulus to the growth of population via a fall in both the demand and supply of effort as a result of reduced incentives to work, in turn feeding back on the level of output in the long run.

My own dynamic argument, for better or for worse, was wholly different from this sort of Marshallian long-run reasoning. I was deeply influenced

by Arthur Lewis's famous 1954 article on "Economic Development with Unlimited Supplies of Labour" with its analysis of the economics of "dualism", and by a spate of writings in development economics in the decade of the 1950s on "disguised unemployment" in Third World countries where wages are close to or below biological subsistence levels, so that an increase of wages raises the standard level of effort of workers by overcoming dietary deficiencies (e.g. Rao, Anshel and Eicher, 1964). Traditional marginal productivity theory cannot handle this case: once a worker's efficiency varies with wages paid, the supply of labour depends on the demand for labour, and demand-and-supply analysis breaks down. The novel feature of my analysis of the Old Poor Law, insofar as it was novel, was to treat Britain in pre-Victorian days as a Third World country, displaying all the familiar features of an underdeveloped economy with institutional devices to convert open into disguised unemployment at wages that were below the poverty line. I struggled to provide evidence of surplus rural manpower and sub-standard wages but that is not to say that I succeeded fully in showing that I had the right theoretical framework for analysing the impact of the Old Poor Law. Nevertheless, for McCloskey to have ignored my theoretical standpoint and to have proceeded as if it were a question of analysing a perfectly competitive rural labour market in full-blown stationary equilibrium was less than helpful.

I agree that generous relief payments, whether geared to earned income or not, are perfectly capable of sapping incentives to work and eroding efficiency. But the actual effect of such a system depends both on its degree of generosity and its leniency of administration. The point about the Old Poor Law was that it was administered locally by 15,000 parishes that averaged less than a thousand people with half of them containing 368 people or less; such a system provides ample safeguards against the flagrant abuse of welfare payments based on personal knowledge of welfare recipients. We come back in the final analysis, therefore, to the vital question of how the Old Poor Law was actually administered. I believe that I threw some light on that question by the statistical analysis of the volume of answers to the Poor Law Commissioners' Rural and Town Queries. The fact remains however, that it can only be settled decisively by a detailed search through parish records. It is with regret that I note twenty-four years later that little has been accomplished in this area (but see Oxley, 1974; Neumann, 1982).

However, Boyer (1985, 1986) has recently taken a step in the right direction by drawing a random sample of 329 parishes from twenty-one southern counties, using the same returns of the Rural Queries of 1832 that I analysed at the county level of aggregation. In essence, his method consists of estimating three equations explaining cross-parish variations in per capita relief expenditures, agricultural labourers' annual wage

income, and the rate of unemployment. His results strongly confirmed my conjecture that poor relief expenditures in parishes varied significantly with specialisation in grain production, which in turn produced substantial seasonal unemployment, thus suggesting that a major function of outdoor relief under the Old Poor Law was to provide unemployment insurance for seasonally unemployed agricultural workers. Moreover, Boyer found little support for the fundamental hypotheses of the Poor Law Commissioners that outdoor relief caused an increase in voluntary unemployment, thus creating the very poor which the Poor Laws were supposed to relieve, no doubt because rural parishes were in fact selective in their granting of relief to able-bodied labourers. Boyer also found support for the contention of Digby (1978) — which had never occurred to me — that one reason why parish relief expenditures were positively correlated with the extent of seasonality in labour demand was that labour-hiring farmers dominated parish governments and exploited their political position by shifting part of their wage bill to other local taxpayers. On the other hand, my own favourite thesis — outdoor relief was used to supplement “substandard” wage income — was not supported by Boyer’s data. In short, my “revisionist” interpretation of the effects of the Old Poor Law is sustained but only in broad outline: I reached the right answers but, evidently, not by the right route.

THE LANCASHIRE COTTON INDUSTRY

The paper on the Lancashire cotton industry grew out of my interest in the problem of technical progress and a conviction that the literature on technical change had long overemphasised labour-saving and underemphasised capital-saving innovations. I summed up my general argument in a later theoretical paper (Blaug, 1963) but years before that I realised that the best way to demonstrate the importance of capital-saving innovations was to show that they predominated even in the early history of the cotton industry, the first industry to undergo the process of industrialisation that was later to spread throughout the whole of manufacturing. Somewhere around 1956 or 1957 I conceived a gigantic project to study and compare the growth of the cotton industry in the nineteenth century in Britain, France and Germany. The article on the Lancashire cotton industry was the only part of the project I completed: the data for France and Germany turned out to be so poor as to defeat me.

I wove my story of the significance of capital-saving innovations in the growth of the cotton industry around an estimate of the trend in the capital coefficient or capital-output ratio. This was one of the “great ratios” of the economics of the day and was usually applied at the aggregate level for the economy as a whole. There are serious objections to the concept of an

aggregate capital-output ratio, chiefly on the grounds that it has no behavioural meaning, but the concept is much less objectionable when applied at the level of an industry as I indeed applied it. Moreover, I supplemented my estimates of the capital coefficient for five benchmark years between 1834 and 1886 by estimates of the labour coefficient and by a review of the evidence on wage rates and profits in cotton. In the light of recent debates about the difficulties and indeed impossibilities of measuring capital in any meaningful way, it is amusing to note that estimating the output of the Lancashire cotton industry in the nineteenth century gave me much more trouble than estimating the value of the capital stock.

By the time I wrote this article some time in 1960, the New Economic History (known also as cliometrics) had just started its upward climb in the United States, reaching a crescendo in the early 1960s, although it was not until the late 1960s that it came to be applied to British economic history (see McCloskey, 1971). I had never heard of the New Economic History when I wrote this article and the two later ones on the Old Poor Law: like Monsieur Jourdain in Molière's *Le Bourgeois Gentilhomme* who never realised that he spoke prose, it was years later that I realised I had been unwittingly practising the New Economic History all along!

This paper on the cotton industry has fared better in some ways than my two papers on the Poor Laws, largely escaping criticism. It has been cited frequently (e.g. Deane and Cole, 1967; Crouzet, 1972; Chapman, 1972) and complemented by capital estimates for the earlier period 1770-1834 based on fire insurance valuations (Chapman, 1971). On the other hand, the idea of tracing precisely how the factor-saving pattern of technical progress altered in the course of industrial change in the eighteenth and nineteenth centuries does not seem to have been taken up in the study of other British industries.

RICARDIAN ECONOMICS

The chapter on the empirical content of Ricardian economics is a distillation of my doctoral dissertation, later published as a book with the title *Ricardian Economics: An Historical Study* (Yale University Press, 1958), and it was my first published article. It attracted little or no attention until it was attacked in a sophisticated essay by Neil de Marchi (1970). De Marchi's paper was a strange example of a critique which was designed to refute its object, while actually confirming it with additional detail — or so it seemed to be at any rate. I had argued that Ricardo's followers failed to test the accuracy of Ricardo's predictions about the declining rate of profit, the rising rental share of national income, and the constancy of even fall of real wages, even though they were in possession of statistical data that would have been perfectly adequate to undertake that task; this led some

of them, and particularly John Stuart Mill, to devise a series of *ad hoc* excuses for the Ricardian system.

De Marchi argued that Ricardo committed himself under pressure to a definite length of time required to reach a long-run equilibrium — twenty-five years — and he shows that Mill gradually extended this period in successive editions of his *Principles of Political Economy* to forty and then fifty years in order to account for the failure of Ricardo's predictions to materialise. Both Ricardo and Mill were convinced of a close correspondence between their analysis and reality, partly because they thought that their axioms were grounded in fact and partly because they thought that they had actually identified the crucial economic forces. Although they did not consciously seek predictive accuracy in their statements about long-run tendencies, it is clear that both of them would have been and were surprised by the failure of these tendencies to be borne out by experience. I attribute something like duplicity to Mill on behalf of Ricardo and it is here that de Marchi parts company with me: "it was Ricardo's and Mill's greater concern with 'understanding' (explanation *ex post*) than with predictive accuracy that made them unwilling to abandon their propositions in the face of contrary facts. What has been said above goes part of the way towards showing that this preference does not represent contrived duplicity; it was rather a candid acknowledgement of the difficulty of knowing all causes in advance, and of conducting conclusive historical tests" (de Marchi, 1970). In the final analysis then only a hair's breadth separates my interpretation from that of de Marchi (see also Blaug, 1980b, pp. 73-7).

De Marchi and I agreed more often than we disagreed. Since then, Samuel Hollander has "improved" on de Marchi's critique by going so far as to argue that Ricardo never committed himself to any clear-cut prediction about any economic variable, so that the very title of my piece, "The Empirical Content of Ricardian Economics", implies a category-mistake. Hollander's thesis is argued, along with many other similarly surprising theses about the Ricardian system in a massive book, *The Economics of David Ricardo* (1979), which amounts to nothing more or less than a direct frontal assault on the entire secondary literature about Ricardo, purporting to show that everyone before Hollander got it wrong.

The final pages of the next chapter, "Ricardo and the Problem of Public Policy", attempt to explain how Hollander arrived at his bizarre conclusions. The essential message of this chapter, however, is to throw cold water on Ricardo as a policy adviser. His theories were directly aimed at the solution of the outstanding policy problem of his time, namely, the protection of agriculture, and his writings bristle with policy recommendations: to repeal the Corn Laws, to abolish the Poor Laws, to raise a levy on capital wealth, to pay off the public debt, to nationalise the note

issue function of the Bank of England, and many others. All these proposals, he realised, threatened contemporary "interests" and raised profound administrative problems. Yet he never spelled out a practical programme for implementing these unpopular policies. In short, he exemplified all the weaknesses that have characterised the writings of economists on problems of economic policy right up to the present day.

THE LABOUR THEORY OF VALUE IN ADAM SMITH AND RICARDO

The chapter on "Welfare Indices in *The Wealth of Nations*" takes up a little puzzle that has caused more confusion about classical economics than any other: it is Ricardo's rejection of Adam Smith's "measure of value" as the amount of labour "commanded" in exchange, replacing it with the amount of labour "embodied" in commodities, a rejection that was endorsed by Marx and that led to the common view that Smith had confused the "measure" with the "cause" of value. Both Ricardo and Marx, however, flagrantly misunderstood Smith's intention, which was to construct an index of improvements in economic welfare. What Ricardo and Marx did was to change the question about value rather than the answer: they wanted to know the cause of variations in exchange value and that was a question which simply did not rouse Smith's interest.

This was a paper that emerged directly out of teaching the history of economic thought at Yale University: while attempting to explain Ricardo's criticism of Adam Smith to a class of students I suddenly realised that it made no sense and retired in confusion. I spent twenty-four hours rereading the relevant chapters in *The Wealth of Nations* and wrote the paper as it now stands in a few hours. Admittedly the argument is compressed and the paper, brief as it is, does not make easy reading, being a kind of verbal mathematics in prose. Nevertheless, twenty-five years later, I still believe every word of it. It was with great delight that I later read Gordon (1959), who confirmed my interpretation of Smith to the letter.

POLICY ISSUES IN CLASSICAL ECONOMICS

My doctoral dissertation on Ricardian economics took up a number of policy questions that troubled the classical economists. Some of these questions, however, raised issues that had little to do with the rise and fall of Ricardian economics and so were left out of the dissertation. One was the famous question of regulating the hours of work in factories which became a central problem of economic policy in the 1830s and 1840s. My paper on "The Classical Economists and the Factory Acts" was written in

reaction to the common view of economic historians that the classical economists were unalterably opposed to the factory regulation and the less common but nevertheless prevalent view of historians of economic thought that the classical economists favoured the regulation of child labour, only disapproving of legislation for adult men and women. My reappraisal of the debate was designed to show that their views were more complex and divided than is suggested by either of the two extreme interpretations and to demonstrate that the classical economists largely ignored considerations of administrative feasibility for their concrete recommendations, thus elaborating the theme set out in the chapter "Ricardo and the Problem of Public Policy".

To this day I remain troubled by the question touched upon in its closing pages: if the steady reduction in factory hours in the nineteenth century turned out eventually to raise output per man-hour and even output per man-day, thus justifying itself as economically efficient, why is it that it required legislation to achieve this result? I suggest in the paper that it is because employers are myopic in the sense of maximising short-run profits; in addition, the pressures of competition prevent each employer from conducting experiments with shorter hours to see whether it might raise output per man. But I am not sure even now that this is the right answer to the question of whether the regulation of hours constitutes a genuine example of "market failure". Johnson (1969) and West (1983) provide further evidence on this issue without, however, coming to grips with the basic question.

In the 1960s and early 1970s, I abandoned the history of economic thought and for over a decade I worked largely in the economics of education, that is the application of economics to current questions about education and training. I always hoped that some day I would be able to bring these two interests of mine together; when invited in 1974 to contribute a paper to a collection of essays on Adam Smith, I saw my chance and the result was the paper on "The Economics of Education in Classical Political Economy". In some ways, it makes much the same point as my essay on the Factory Acts: the classical economists invariably adjusted their ideas on education (as they did their ideas on the regulation of hours) in the wake of legislative changes; instead of having an influence on policy, policy had an influence on them. But that is only one theme of the paper; another is to deny the widely held view that the classical economists originated the theory of human capital and still another is to deny the still more widely held view that they approved of the now standard belief in the principle of free but compulsory schooling. This is a long chapter, indeed the longest in the book, but the complexity of the subject warrants it and, besides, much space is taken up with a review of the bewildering interpretations of previous commentators.

MARXIAN ECONOMICS

The next two chapters, on Marx, take up aspects of Marxian economics, a subject that has always fascinated me. I have written about Marx at greater length elsewhere (Blaug, 1980b, 1985, Ch. 7) but my early essay on "Technical Change and Marxian Economics", arguing that Marx's vision of capitalism was fatally marred by the unwarranted conviction that technical progress is inherently biased in the labour-saving direction, has coloured all my subsequent thinking about the Marxian system. Needless to say, the discussion among Marxist economists of this range of questions has moved on since 1960 when my essay was written; the interested reader should consult Van Parijs (1980), which provides a useful survey of the debate over the last forty years.

"Another Look at the Labour Reduction Problem in Marx" is yet another synthesis of my interests in the history of economic thought and the economics of education. The "labour reduction problem" is an old question in Marxian economics, being the problem of whether we can in fact invoke a quantitative measure of labour-time to account for the value of commodities without resorting to differences in wages to reduce different types of labour to a single one. I was struck by the fact that the manner in which this problem was usually handled by Marxist economists bore a striking resemblance to the views of advocates of human capital theory, according to which differences in wages can largely be explained by differences in the length of schooling and on-the-job training. But "largely" is still not the same thing as "entirely" and to the extent that earnings differentials are at least in some degree due to differences in inherited or acquired talents (it matters not which), the Marxian labour-reduction problem cannot be solved. Some commentators regard this unsolved problem as the Achilles Heel of Marxian economics but I doubt that it is, or rather, it is itself a symptom of a much more serious flaw in the Marxian system, which is its inconsistent view of how labour markets work under capitalism: in Marx, occupational mobility is said to establish a uniform rate of wages for every type of labour, a proposition that is false and which Marx, having read Adam Smith, knew to be false.

THE MARGINAL REVOLUTION AND AFTER

The chapter on the marginal revolution considers the historical puzzle of the overthrow of classical economics in the 1870s and the emergence of a new kind of economics usually labelled misleadingly as "neoclassical" economics (the right term is clearly "postclassical" economics). This was a peculiar "revolution" because it took so long to make itself felt and it is not

clear even today why it took place in the 1870s when it might as well have happened in the 1850s or even 1840s.

There follows a chapter on the history of the concept of entrepreneurship, or rather the strange disappearance of the entrepreneur, that central figure in the operation of a capitalist system, from the corpus of received economic doctrine. It raises a central question about the entire history of orthodox economics in the last hundred years, namely, that all the substantive findings of modern economics rest on the use of static equilibrium analysis and yet static equilibrium analysis seems to preclude fruitful discussion of such vital problems as the process of competition, the process of capital formation and the role of entrepreneurship. Economics began as *An Inquiry into the Causes of the Wealth of Nations* and yet 200 years later we have virtually abandoned that inquiry as unproductive and have taught ourselves to be content with smaller questions. Worse than that, static equilibrium analysis has furnished us with standards of rigour that cannot be met by the analysis of the dynamic problems of entrepreneurship and the competitive process, so that discussion of these questions is met with scorn almost as soon as it is started.

I do not claim to see my way out of this dilemma. But it poses a nice problem that I want to go on thinking about — so long as I can still think.

METHODOLOGY

The essay on “Kuhn versus Lakatos” has frequently been reprinted, not because it was so good — some of it is rather clumsy — but because it was the first exposition for economists of some recent developments in the philosophy of science. I extended and improved my presentation subsequently (1980b, Chs. 1, 2) but I never returned to the Lakatosian explanation of the Keynesian revolution, which I sketched in this paper. In a recent thought-provoking essay, Hands (1985) has thrown doubt on the proposition that the Keynesian revolution can be explained by Lakatos’ methodology of scientific research programmes. According to Lakatos, a scientific research programme wins professional approval when it is theoretically “progressive” in the sense of predicting “novel facts” that were either unknown or at least were not themselves employed in the construction of the programme. The Keynesian revolution is one of the greatest success stories in the history of economic thought. Yet what were the novel facts predicted by Keynesian economics? Not mass unemployment, since *The General Theory* was written precisely to explain unemployment. Not the consumption function or the inverse relationship between the demand for money and the role of interest, since these concepts were used explicitly to construct the Keynesian system. Thus, Hands concludes the Keynesian programme was not “progressive” in the

strict sense of Lakatos and hence the overwhelming professional acceptance of Keynesian economics in the 1930s was "irrational".

This is a striking argument that must make us think again. However, Hands fails to mention one of the central features of Keynesian economics that *may* go a long way to explain its amazingly rapid acceptance by the economics profession. It is that the Keynesian system is formulated in terms of a model whose key variables and relationships are specified in such a way as to be capable of quantitative measurement. The stimulus which *The General Theory* gave to national income accounting, incorporating Keynes' *ex post* identity of saving and investment, and to the construction of testable models of economic behaviour, is an integral feature of the Keynesian success story. I do not claim that this is a decisive answer to Hands' argument but it does suggest that he may have omitted vital elements in the story. Nevertheless, his deeper point, that professional success and failure in a subject like economics simply cannot be fitted into Lakatos' framework, cannot simply be shrugged off. As they say in America: the jury is still out on that question.

The last chapter in this collection is a lecture delivered just before the publication of my book on *The Methodology of Economics*. It attempted to present the material of the book in what I hoped would be a palatable and accessible form to undergraduates in economics, innocent of the philosophy of science. Judging by the confusing discussion that followed the lecture, it was only partly successful in accomplishing that aim.

REFERENCES

- Baugh, D.A. (1975), "The Cost of Poor Relief in South-East England, 1790-1834", *Economic History Review*, 2nd Series, 28(1), February.
- Blaug, M. (1963), "A Survey of the Theory of Process Innovations", *Economica*, N.S. 30 (February) repr. in *The Economics of Technological Change*, ed. N. Rosenberg (London, 1971).
- (1980a), *A Methodological Appraisal of Marxian Economics* (Amsterdam)
- (1980b), *The Methodology of Economics* (Cambridge)
- (1985), *Economic Theory in Retrospect*, 4th edn. (Cambridge)
- Boyer, G. R. (1985), "Economic Model of the English Poor Law circa 1780-1834" *Explorations in Economic History*, 22(1), Spring.
- (1986) "The Old Poor Law and the Agricultural Labor Market in Southern England: An Empirical Analysis", *Journal of Economic History*, forthcoming.
- Chapman, S.D. (1971), "Fixed Capital Formation in the British Cotton Manufacturing Industry", in *Aspects of Capital Investment in Great Britain, 1750-1850* eds. J.P.P. Higgins and S. Pollard (London).
- (1972), *The Cotton Industry in the Industrial Revolution*, *Studies in Economic History*, ed. M. W. Flinn (London).

- Checkland, S.G. and E.O.A. (1974), "Introduction" to *The Poor Law Report of 1834* (London).
- Cowherd, R.G. (1978), *Political Economists and the English Poor Law* (Athens Ohio).
- Crouzet, F. (ed.) (1972), *Capital Formation in the Industrial Revolution* (London).
- Deane, P. and Cole, W.A. (1967), *British Economic Growth, 1688-1959*, 2nd edn. (Cambridge).
- Digby, A. (1978), *Pauper Palaces* (London).
- Gordon, D. F. (1959), "What Was the Labour Theory of Value" *American Economic Review*, 69, No. 2 (May).
- Hands, D.W. (1985), "Second Thoughts on Lakatos", *History of Political Economy*, 17, No. 1 (Spring).
- Huzel, J.P. (1969), "Malthus, The Poor Law, and Population in Early Nineteenth Century England", *Economic History Review*, 22, No. 3 (December).
- (1980), "The Demographic Impact of the Old Poor Law: More Reflections on Malthus", *Economic History Review*, 33, No. 3 (August).
- Johnson, O.E. (1969), "The 'Last Hour' of Senior and Marx," *History of Political Economy*, 1, No. 2 (Fall).
- Marchi, N. de (1970), "The Empirical Content and Longevity of Ricardian Economics", *Economica*, N.S. 37 (August).
- Marshall, J.D. (1968). *The Old Poor Law 1795-1834*, *Studies in Economic History*, ed. M.W. Flinn (London).
- McCloskey, D.N. (1971), *Essays on a Mature Economy: Britain After 1840* (Princeton, N.J.).
- (1973), "New Perspectives on the Old Poor Law", *Explorations in Economic History*, 10, No. 4 (Summer).
- Neumann, M. (1982), *The Speenhamland County. Poverty and the Poor Laws in Berkshire 1782-1834* (New York).
- Oxley, G.W. (1974), *Poor Relief in England and Wales, 1601-1834* (Newton Abbott).
- Poynter, J.R. (1969), *Society and Pauperism: English Ideas on Poor Relief, 1795-1834* (Carlton, Victoria).
- Rao, C. H. C., Anshel, K. R. and Eicher, C. K. (1964), "Disguised Unemployment in Agriculture: A Survey", in *Agriculture in Economic Development* (New York).
- Taylor, J. S. (1969), "The Methodology of the Old Poor Law", *Journal of Economic History*, 29, No. 2 (June).
- Thompson, R.D. (1907), "An Inquiry into the Rent of Agricultural Land in England and Wales During the Nineteenth Century," *Journal of the Royal Statistical Society*, 70 (December).
- Van Parijs, P. (1980), "The Falling-Rate-of-Profit Theory of Crisis: A Rational Reconstruction By Way of Obituary", *Review of Radical Political Economics*, 12, No. 1 (Spring).
- Wellington, D.C. (1984), "The English Poor Law: A Negative Income Tax", *Atlantic Economic Journal*, 12 (4), December.
- West, E.G. (1983), "Marx's Hypothesis on the Length of the Working Day", *Journal of Political Economy*, 91, No. 2 April.