Piero Sraffa's Political Economy

A centenary estimate

Edited by
Terenzio Cozzi and Roberto Marchionatti

Routledge Studies in the History of Economics

Piero Sraffa's Political Economy

A centenary estimate

Edited by
Terenzio Cozzi and
Roberto Marchionatti

2001

London and New York

Piero Sraffa's Political Economy

Piero Sraffa's work has had a significant impact on the study of economics in the twentieth century. This book presents a centenary assessment of his work by leading academics in the history of economic thought.

Contributions from a wide range of Sraffa scholars are brought together to evaluate Sraffa's overall contribution to economics as well as selected aspects of Sraffa's biography. His *Production of Commodities by Means of Commodities*–still a source of extensive international academic debate–and Sraffa's edition of Ricardo's *Work and Correspondence* are examined.

Piero Sraffa's Political Economy is divided into four parts. Part I looks at Sraffa's biography during the first three decades of the century. Part II is dedicated to Sraffa's work between the middle of the 1920s and the early 1930s–the criticism of Marshallian orthodoxy, the contributions to the Cambridge debates on imperfect competition and Keynes' monetary work. Part III considers the relationship between the articles of the 1920s and the subsequent work leading to the publication of *Production of Commodities by Means of Commodities*. Part IV looks in detail at Keynes' *General Theory;* Sraffa's relationship with mathematicians; the controversy and critical influence on Hayek; Sraffa's 1960 model and open economy.

Terenzio Cozzi is Professor of Economics at the University of Torino, Italy. He is also Professor of Money and Finance, Coripe Master of Economics, Torino. He has previously authored *Teoria dello sviluppo economico,* and *Elementi di economia politica: Un testo europeo* (with S.Zamagni), and has published essays and articles in the fields of macroeconomics, growth and cycles, and Keynesian economics.

Roberto Marchionatti is Professor of Economics at the University of Torino, Italy. His previous publications include *Karl Marx Critical Responses* and *"From Our Correspondent", Luigi Einaudi's Articles in The Economist, 1908–1946,* and he has published extensively in the fields of economic theory, industrial economics and history of economic thought.

Contents

Contributors

Salvatore Baldone, Politecnico of Milano, Italy
Riccardo Bellofiore, University of Bergamo, Italy
Marina Bianchi, University of Cassino, Italy
Duccio Cavalieri, University of Firenze, Italy
Terenzio Cozzi, University of Torino, Italy
Marco Dardi, University of Firenze, Italy
Nicolò De Vecchi, University of Pavia, Italy
Giancarlo de Vivo, University of Napoli, Italy
Angelo D'Orsi, University of Torino, Italy
Heinz D.Kurz, University of Graz, Austria
Giorgio Lunghini, University of Pavia, Italy
Roberto Marchionatti, University of Torino, Italy
Maria Cristina Marcuzzo, University of Roma, Italy
Nerio Naldi, University of Roma, Italy
Carlo Panico, University of Napoli, Italy
Luigi L.Pasinetti, Universitá Cattolica del Sacro Cuore, Milan, Italy
Pier Luigi Porta, University of Milano, Italy
Tiziano Raffaelli, University of Cagliari, Italy
Fabio Ranchetti, University of Pisa, Italy
Alessandro Roncaglia, University of Roma, Italy
Annalisa Rosselli, University of Roma, Italy
Andrea Salanti, University of Bergamo, Italy
Neri Salvadori, University of Pisa, Italy
Rodolfo Signorino, University of Pisa, Italy
Ian Steedman, Manchester Metropolitan University, UK
Sergio Steve, University of Roma, Italy
Carlo Zappia, University of Siena, Italy

Preface

Piero Sraffa is considered one of the great economists of the twentieth century. Such a reputation is based on what amounts to a small number of writings, though all of lasting impact and influence. His 1926 anti-Marshallian article, 'The laws of returns under competitive conditions', had a dramatic effect on the economic theory of that time–particularly in the United Kingdom–and contributed in an essential way to the abandonment of the Marshall-Pigou theory of value and the subsequent emergence of the imperfect competition theory in the 1930s. Sraffa's edition of Ricardo's *Works and Correspondence*–a work which started at the beginning of the 1930s and took nearly thirty years to complete–has been considered a masterpiece in the history of economics; in recognition, Sraffa was awarded the Söderstrom Gold Medal for Economic Sciences by the Swedish Academy of Sciences, an honour which he shared with Keynes and a small number of other scholars. His 1960 book, *Production of Commodities by Means of Commodities,* is seen as a classic in the theory of capital and has been the source of extensive academic debate.

Although the importance of Sraffa's writings is fully recognised, general consensus on their meaning and relevance has not been achieved. With regard to the 1926 article, the majority of commentators emphasise that Sraffa constructed a sturdy intellectual foundation for the English branch of the theory of imperfect competition; though recognising this historical role, others prefer to see it as a contribution to a new mainstream, distinguished by the tacit agreement that is better to have a poor, useful theory than a rich, useless one. Other authors emphasise that Sraffa was essentially a critic of partial method and an advocate of a simultaneous equation approach, the outcome of which would be his 1960 book. As to Sraffa's 1951 introduction to Ricardo's *Principles,* reviewers initially saw no break with Ricardo's own interpretative tradition, indeed they were struck by Sraffa's 'unfailing neutrality'. Later, however, it was viewed as strongly influenced by Sraffa's own particular path of research, to the point, according to some critics, of misinterpreting Ricardo. Above all, disagreement has prevailed with regard to the 1960 book. Some of the first reviewers considered it a great theoretical

advance, while others judged it to be just another Leontiev-type model and original only from a subjective point of view: these differences of opinion were ascribed to the extreme difficulty of the work and to what seemed to many commentators its mathematical incompleteness. Later some economists tried to show Sraffa's distinction with respect to neoclassical economics, maintaining that he represented the rehabilitation of the classical objective theory of value and distribution against marginalist subjectivism and demonstrated that the Ricardo-Marx approach was logically consistent. Different and opposing judgements are still present, however, as can be seen for example in some of Samuelson's recent contributions.

At the end of the 1970s, for a decade or more, interest in Sraffa's work waned quite considerably, but around the beginning of the 1990s it had started to return. Undoubtedly, the availability since December 1994 of Sraffa's unpublished papers, kept in the Wren Library at Trinity College, Cambridge, has contributed to this renewal of interest, representing both a great stimulus and a fundamental source of information in assessing Sraffa's work.[1] The present book is largely a result of studies based, directly or indirectly, on the new evidence available. We asked a wide range of Sraffa scholars to present and evaluate Sraffa's contributions to economic theory and the history of economic thought on the occasion of a conference commemorating the centenary of Sraffa's birth, held in Turin at the Fondazione Einaudi from 15–17 October 1998: after revision and rewriting, these papers are collected here. Although a complete assessment of Sraffa's work is certainly not possible at present, we like to think that this book represents a relevant contribution in that direction.

The chapters cover different phases and subjects of Sraffa's writings. Part I contains a group of chapters which consider the biographical details of Sraffa during the first three decades of the century. The articles in Part II are concerned with the famous 1925 and 1926 articles on the criticism of Marshall and Marshallian orthodoxy, and Sraffa's contribution to the Cambridge debate on Kahn's and Joan Robinson's theories of imperfect competition and Keynes' *Treatise of Money*. The third group of papers considers the relationship between the articles of the 1920s and subsequent work leading up to *Production of Commodities by Means of Commodities*. Finally, Part IV deals with some specific topics (open economy, money and Keynes' *General Theory,* Sraffa's relationship with the mathematicians, the controversy with and the critical influence on Hayek). The majority of the chapters are accompanied by the comments presented at the Torino conference, some of which represent short chapters in themselves.

Note

1 For a history of the papers since Sraffa's death and the methodology used in their cataloguing see Garegnani (1998a) and Smith (1998).

Introduction

Roberto Marchionatti[1]

This introduction presents a survey of the chapters published below. It is intended to serve as a reading guide and to provide an updated reconstruction of the theoretical path followed by Piero Sraffa.

The Italian years

Education and early writings

Piero Sraffa was born in Turin, Italy, on 5 August 1898, the only child of Angelo Sraffa and Irma Tivoli. He attended primary school in Pavia and secondary school in Milan and Turin. In 1916 he entered the Faculty of Law at the University of Turin.[2] As D'Orsi's chapter shows, that faculty constituted a prestigious part of the humanistic academic culture in Turin, a city that was at that time one of the prominent centres of European positivism, a position which it had held since the last decade of the nineteenth century. Among the economists of the faculty and of the associated *Laboratorio di Economia Politica,* a postgraduate centre founded by Salvatore Cognetti de' Martiis in 1893, were outstanding Italian economists like Achille Loria, Luigi Einaudi, Pasquale Jannaccone, Giuseppe Prato and Attilio Cabiati. On an academic and scientific level Sraffa knew many of these men, but from the evidence available he was quite close only to Cabiati, during and immediately after the writing of his thesis, and Einaudi.

Sraffa graduated in law from the University of Turin on November 1920 with a thesis entitled *L'inflazione monetaria in Italia durante e dopo la guerra;* Luigi Einaudi was his supervisor *(relatore)*. Sraffa's thesis was an original dissertation on the monetary policy measures that had been taken in Italy during the war and post-war period. It deals with some of the themes most discussed in the monetary literature and the international economic conferences of that time: the causes and consequences of inflation, the stabilisation of internal prices and exchange rates within an unstable international financial system, and the arguments for restoring the gold standard and the re-evaluation of the currency to pre-war gold parity. Sraffa's

thesis was an applied work behind which we may recognise a theoretical position which bears similarities to that proposed three years later by Keynes in *A Tract on Monetary Reform;* it is also a work which, as Panico notes in his chapter, presents some valuable peculiarities, such as the suggestion that inflation and deflation have permanent effects on the social conflicts which regulate the real wage rate.

After graduating and a short period of training in a bank, in April 1921 the young Sraffa moved to England and became a research student at the London School of Economics, where he remained until June 1922. In his chapter, Naldi maintains that this period was very important for Sraffa's formation as an economist. Undoubtedly an important date was August 1921, when Sraffa went to Cambridge to meet John Maynard Keynes: one result of their meeting was that Keynes asked Sraffa to write an article on the crisis of the Italian banking system for the weekly supplement of the *Manchester Guardian Commercial,* which dealt with the monetary and financial problems of post-war reconstruction in Europe.[3] Sraffa's article proved too long for the *Manchester Guardian Commercial* and was published in the June 1922 issue of the *Economic Journal;* a shorter article took its place in the 7 December issue of the newspaper. As Panico shows, in these articles Sraffa acknowledged that the conflicts within the capitalist class, together with the autonomous interests of political and administrative bodies, affected economic policy, and that government intervention had permanent effects on income distribution, an idea present in Sraffa published and un-published writings of the years 1923–7.

In November 1923 Sraffa was appointed as a lecturer in Political Economy and Public Finance at the University of Perugia and in 1925 obtained a full professorship in Political Economy at the University of Cagliari. He taught there only briefly and, after leaving for Cambridge, held the post *in absentia* until the end of his life.

The criticism of Marshall and the outline of a theory of imperfect markets

At Perugia Sraffa undertook a careful reading of Marshall's *Principles.*[4] If Sraffa's interest in monetary affairs never ceased, it nevertheless lost some of its importance within his activities, for as early as spring 1923, according to Naldi, he began a research project in which the critique of Marshallian theory emerged as a crucial element.[5] 'Sulle relazioni tra costo e quantità prodotta', published in *Annali di Economia* in 1925, was the first important result of this project and enabled Sraffa to be appointed professor at the University of Cagliari. As is well known, Francis I.Edgeworth, editor with Keynes of the *Economic Journal,* had such a high opinion of this article that he decided to invite Sraffa to submit a version to the *Journal:* the result was the article 'The laws of return under competitive conditions', published in the December 1926 issue.

This article assured Sraffa an important position in the long international debate on the Marshallian theory of value and competition, known as 'the controversy on costs', which involved some of the most important economists of the time. Marchionatti's chapter reconstructs the role of Sraffa's article within the general framework of this controversy which contributed substantially to the foundation of contemporary theories of value and competition. The major issues were the solution of the Cournot dilemma on increasing returns and competition proposed by Marshall, and the concepts of external economies and the representative firm. If undoubtedly, as Raffaelli emphasises, there was an epistemological difference between Marshall and the economists who discussed and criticised his theory in the 1920s, that criticism was permitted, Marchionatti demonstrates, by the fragile analytical translation of Marshall's methodology. To begin with, as Sraffa wrote in his 1926 article, qualifications, restrictions and exceptions to Marshall's theory were 'scattered about in footnotes and articles and carefully segregated from one another'; then, progressively, direct criticism openly emerged.[6] Sraffa highlighted the analytical limits and the limited relevance of Marshall's theory of value. Under conditions of stable partial equilibrium, he noted, the shape of the supply curve could not be decreasing unless, as Pigou showed, the hypothesis of external economies is introduced in a very precise way–external to the firm but internal to the industry–in order to guarantee the *ceteris paribus* condition. Moreover, under such conditions of partial equilibrium the shape of the supply curve can be increasing only if the totality of the factor of production is used to produce the commodity under examination. Sraffa touched on the weak point of Marshallism–the problem of the coexistence of logical consistency and practical relevance of both Marshall's and Pigou's theories of value–with extraordinary acuteness. Though he agreed with Knight and Schumpeter on the requirement of rigour, as Marchionatti notes, he could not share their conclusive judgement on the practical irrelevance of the theory of value in general. For Sraffa, the lack of the necessary theoretical rigour did not mean sentencing the theory of value to practical irrelevance but simply demonstrated the irrelevance of Marshall's theory. In their paper on Sraffa's methodology, Salanti and Signorino indicate that for Sraffa the acceptance or rejection of a theory depended on its internal logical consistency and its ability to conform to the requirements of realism of the (implicit and explicit) premises; consequently Marshall's theory was to be discarded because it was devoid of relevant empirical content.

In the second part of his 1926 article, Sraffa followed an original path of research in order to examine the connection between cost and quantity produced and to overcome the Marshallian theoretical difficulties: this was the Cournotian route, as offered by the theory of monopoly. Sraffa suggested that increasing returns could be accommodated by abandoning the competitive hypothesis and recognising that an industry's output is limited by

the difficulty of selling larger quantities of a good without lowering its price. In this imperfectly competitive framework, each firm has its own market in which the relevant demand curve is not infinitely elastic.

The outline of a theory of imperfect markets in the 1926 article greatly impressed Keynes, Pigou and the younger Cambridge economists, and led to Sraffa being offered a lectureship at Cambridge at the end of January 1927.

The Cambridge years: the late 1920s and early 1930s

Sraffa was appointed lecturer in May and took up the position the following October. He spent the rest of his life (fifty-six years) at Cambridge, initially as a lecturer then, after only three years, as Marshall Librarian in charge of graduate studies, and from 1935 to 1963 Assistant Director of Research; he became a Fellow of Trinity College in 1939, and in 1963 he was made a Reader. It was in this long and, from an intellectual point of view, extremely rich period at Cambridge that Sraffa developed an original programme of research in economic theory.[7] This section considers the years 1927–32, whilst the next deals with the successive period up to the publication of *Production of Commodities by Means of Commodities* (hereafter *PCMC*).

The chapters presented below confirm that the period 1927–32 was crucial in the development of Sraffa's thought and was characterised by the study of the theory of value in a historical perspective, a deepening of the critique of the dominant economic theory, the abandonment of the theoretical path of imperfect competition, and the beginning of a reconstruction of economic theory based on classical categories. All these 'streams of thought'–to use Pasinetti's words–are present in the set of *Lecture notes on advanced theory of value* kept among the *Sraffa Papers:* these notes, which deal with the theories of production and distribution and the forms of competition, were the result of the extensive work in which Sraffa was engaged, most likely since the summer of 1927, in preparation for the lectures given between 1928 and 1931. In the same period, the late 1920s, Sraffa was involved in a debate on various aspects of imperfect competition with his closest Cambridge interlocutors–Gerald Shove, Richard Kahn and Joan Robinson–which probably led him to abandon the Cournotian path.

The abandonment of the path of imperfect competition

When Sraffa undertook his exploration of the field of imperfect competition he probably believed that a generalised theory of monopoly might constitute a sound foundation for a theory of value in which increasing returns coexisted within a supply-and-demand framework (see Mongiovi 1996); yet quite soon he lost all interest in the subject, and from 1930 detached himself from the discussions going on in Cambridge and elsewhere. Marcuzzo, Dardi and Cavalieri investigate the reasons behind this abandonment. Initially,

Sraffa saw market interaction as something that produces results which are subject to a logical necessity, derived essentially from the structure of technology and only in a very small part from subjective motivations. Through discussion with his fellow economists in Cambridge, according to both Marcuzzo and Dardi, he realised that this was not so, and that in the field of market phenomena the relative weight of the subjective element is in fact overwhelming.

The first point of conflict with Sraffa's original position was the criticism by Kahn who, in his fellowship dissertation on *The Economics of the Short Period* written between October 1928 and December 1929, maintained that Sraffa's 1926 article contained 'a serious error'–although, as Dardi notes, it would be fairer to talk of a non sequitur–in its implicit statement that, under conditions of uniformity among firms, provided that the market is slightly imperfect, the magnitude of the imperfection is irrelevant to the equilibrium price. Contrary to what Sraffa had claimed, Kahn showed that if the sources of supply make their decisions independently of each other, then equilibrium will be a function of the assumptions that each firm makes about its rivals' conduct. It is quite possible that this proof–clearly demonstrated by Kahn–that dealing with imperfect markets renders the 'mental' determinants of equilibrium unavoidable, was one of the main reasons for Sraffa's estrangement from the entire problem.

The second point of conflict was connected to the question of including marketing expenses in the cost of producing a commodity, as Shove and Kahn had done. Sraffa did not agree because in this way the Marshallian notion of the individual demand curve as a definite independent unity had to be adopted, whilst according to Sraffa the demand curve was not independent of the supply curve since marketing expenses were designed to affect the demand curve. There is of course nothing against supply being determined by demand, but Sraffa probably rejected the Marshallian approach because, in short, by means of marketing expenses utility enters into costs. However, Sraffa did not manage to convince his Cambridge colleagues of this. Marcuzzo interprets these various discussions as a sign of the incomprehension surrounding Sraffa in his early years at Cambridge; Dardi, on the other hand, believes that discussion helped Sraffa obtain a sense of the distance between his own interests and those of most of the other economists of the period, who were much closer than Sraffa to mainstream research. From then on Sraffa followed a different path, increasingly further from the theoretical neoclassical mainstream.

The formation of Sraffa's programme of research. The study of the classics and the first steps towards PCMC

Garegnani (1998a) locates in Sraffa's research, in the winter of 1927–28, 'an initial (and decisive) turning point…that led to an examination of the classical

economists with consequent abandonment of the Marshallian interpretation of them that had been behind the articles of 1925–26' (Garegnani 1998a:152). During this period Sraffa introduced the notion of 'physical real cost', derived from Petty and the Physiocrats, which would allow him to abandon his Marshall-type interpretation of classical economic theory. De Vivo brings forward this important development in Sraffa's thought, suggesting that it would more likely have taken place early in 1927 when he began (re)reading the classical economists, in particular Marx. What is certain is that in the late 1920s Sraffa studied the classical theory of value from Petty to Marx in order to comprehend the transformation of the notion of cost of production from the classical school to the marginal school. His apparent discovery was that there were two notions of costs–one concerned with 'necessaries' and the other concerned with 'motives'–which gave rise to two theories of distribution and two conceptions of wages and profits–one as 'surplus' of the product over necessaries and the other as 'shares' in the product.

Pasinetti identifies three streams of thought in this crucial period, which constituted a huge research programme. The first was Sraffa's increasing belief that the dominant (marginalist) economics had caused an 'aberrant' change in the content of economic theory, with respect to what it was previously, and that 'Marshall's attempt to bridge over the cleavage and establish a continuity in the tradition is futile and misguided'. The second was the necessity to develop a critique of marginal economic theory. The third research objective was a logical consequence of the previous two: the necessity to return to the political economy of the Physiocrats, Smith, Ricardo and Marx. This was a positive programme of research, of which the immediate outcome were the equations in the 'draft of the opening propositions' of *PCMC,* which Sraffa showed to Keynes and Pigou in autumn 1927; the final outcome was the 1960 book.

De Vivo emphasises how this work was directly and explicitly linked to Marx, in particular with the schemes of reproduction of volume II of *Capital;* moreover, de Vivo notes that Sraffa envisaged as his ultimate goal a restatement of Marx's theory set free from the terminology of Hegelian metaphysics. Cavalieri maintains that Sraffa's new analytical approach also drew on the theoretical background of the Russian-German school of mathematical economics whose main representatives were V.K.Dmitriev and L.von Bortkievicz. Sraffa's unpublished papers do contain a notebook on Bortkievicz, but this was written in the 1940s, by which time the 'central propositions' of *PCMC* had already been completed (Sraffa himself informs us in the preface (p. v) that 'the central propositions had taken shape in the late 1920s'). Indeed, on the basis of the available evidence it is not possible to suggest that Sraffa used Bortkievicz or Dmitriev as a starting point for his research or found the idea for his book in their works.[8] Kurz and Salvadori throw light on the emergence of Sraffa's new theoretical construction, examining the relationship between Sraffa and the mathematicians around him

when he first began the foundations of the 1960 book. It is known that this relationship was an important concern throughout the whole period of the writing of *PCMC,* crucial to its eventual completion–a fact which Sraffa indirectedly acknowledged in the preface where, aside from Keynes, he thanks only three mathematicians, Frank Ramsey, A.S. Besicovitch and Alister Watson–but what has been somewhat ignored is its basis. New evidence shows that, from the mathematical point of view, it was Frank Ramsey who aided the Italian economist in the initial period of work. In the early formulation of what came to be called the 'conditions of production' or the 'production system' in terms of systems of simultaneous equations, Sraffa did not distinguish between the quantity and the price or value of a commodity. He appears to have corrected this error from conversations with Ramsey (and probably also with his help) between 1928 and 1929. On the basis of this correction Ramsey then reformulated the theorem, first by putting the system of homogeneous linear equations into its canonical form, then by setting the determinant of the coefficients to zero in order to obtain a non-trivial solution.

At this point, in the early 1930s, Sraffa had to interrupt more or less completely this promising path of research for about a decade to plunge himself 'like a maniac' into the editing of the Royal Economic Society edition of Ricardo's work and correspondence.

Sraffa and Keynes' Treatise on Money

At Cambridge, Sraffa's interests in monetary studies were further stimulated by the fruitful relations with Keynes who was at the time engaged in the publication of *A Treatise on Money*. Keynes' *Treatise* was revised between 1929 and 1930 following the stimulus and criticisms, which continued after publication, of Hawtrey, Hayek and Robertson on the one hand, and on the other the members of the *Circus*–the *Treatise* informal discussion group that met between late 1930 and spring 1931 and which included Sraffa, Richard Kahn, James Meade and Joan and Austin Robinson, and also some of the most brilliant economics students of the younger generation. On the role of Sraffa in the *Circus* discussions, on which very little was known, new archival evidence has been found by Marcuzzo. This gives further support to the idea that Sraffa was influential in the debates with both the younger and older generations of Cambridge economists. However, according to Marcuzzo's interpretation, although Sraffa's contributions proved to be important, the impact of his criticism of Marshallian theory and his attempts to gain acceptance for an alternative approach proved ineffectual: in fact by 1932 the direction of Cambridge economics, as regards its most important developments, was much more towards the adaptation of the Marshallian apparatus rather than its abandonment.

Notoriously, the *Treatise* was unfavourably reviewed by F.A.von Hayek, giving rise to a debate between Hayek and Keynes. In this debate Keynes was

supported by Sraffa's famous review article of Hayek's *Price and Production,* entitled 'Dr Hayek on money and capital', in the March 1932 issue of the *Economic Journal* in which Sraffa attacked the foundations of Hayek's theory. Sraffa argued that the Austrian economist had failed to identify the essential properties of money, in particular the role it plays as a store of value. Sraffa's principal point of criticism was Hayek's idea that there was a natural rate of interest which corresponds with the equality between saving and investment, and that trade cycles occur because banks held their money rates below the natural level so creating distortions in an economy's capital structure. Sraffa put forward an alternative theory and demonstrated that there exists a multiplicity of natural rates, all of which diverge from one another when the economy is not in equilibrium. The review included many interesting and fruitful ideas which, however, remained undeveloped–Sraffa confined himself to pointing out some logical defects of the classical theory of interest–and this has raised questions about the nature of Sraffa's criticism. Panico believes that Sraffa essentially followed Keynes' approach to monetary questions (for example, he adopted the version of quantity theory presented in the *Treatise*) and that the review lacks a general critique of neoclassical theory. Yet it should be noted that, as Ranchetti points out, Sraffa makes no recourse to marginalist concepts in his argument. In any case, as Panico makes clear, the review of Hayek's book, along with the other discussions surrounding *A Treatise on Money,* led Sraffa to reconsider the links between monetary theory and the theory of prices and distribution.[9]

The Cambridge years: from the 1930s to the end of the 1950s

The study of Ricardo and the edition of Work and Correspondence

In February 1930 Sraffa became the editor of the Royal Economic Society edition of *The Work and Correspondence of David Ricardo*. He already had a good knowledge of Ricardo's work and of the difference between classical and marginalist theory. However, as Pasinetti and de Vivo show, it was only during these years that he really acquired a comprehensive first-hand knowledge of the classical economists: the editorship, moreover, offered Sraffa the opportunity to clarify for himself the incongruities in classical economic thought and to consider the relevance of Ricardo's works to his own programme of research.

Rosselli traces the long history of Sraffa's edition of Ricardo. It began when Keynes persuaded the Royal Economic Society to entrust Sraffa with the task of editing the publication and succeeded in obtaining help and collaboration for him. After 1933 the work was seen to be slowing down due to diminishing returns in the search for manuscripts and the rigorously high standards Sraffa had set himself; Keynes obtained an extension from the

Royal Economic Society, at first until December 1939 then to December 1941. Later, in July 1943, came the discovery of the Mill-Ricardo Papers and the post-war collaboration between Sraffa and Maurice Dobb, which continued up to spring 1951 and the publication of the *Principles* and its three successive volumes (a further seven volumes were published in the years that followed, the index was completed in 1973).

For a long time, Sraffa's introduction to the *Principles* has been considered both a fundamental contribution and also an essentially 'neutral' one, not breaking with Ricardo's interpretative tradition. In recent years, however, a new school of thought (see Hollander 1979 and Peach 1993), which has striven to interpret Ricardo's work as part of the marginalist tradition, has criticised Sraffa's interpretation of Ricardo, in particular Sraffa's celebrated explanation of a passage in a letter where Ricardo illustrates to Trower the contrast between his own and Malthus's positions on the decline in the rate of profits with the accumulation of capital–Ricardo asserting his conviction that 'it is the profits of the farmer which regulate the profits of all other trades.' Sraffa argued that, in his attempt to determine how variations in the conditions of the production of agricultural goods, which constitute the majority of wage goods, affected the rate of profits, Ricardo reasoned as if the agricultural sector showed homogeneity between input and output, so that both the product and the capital needed for production could be compared in physical terms and profit determined without recourse to any theory of value. Thanks to the principle of uniformity in the rate of profits, the variations in prices in other sectors would adjust to the rate of profit emerging in the agricultural sector, which would thus take on the role of 'regulator' or guide. Criticism of this interpretation has concentrated on what is judged to be insufficient textual evidence to support Sraffa's reconstruction. Against this 'new view' interpretation, Rosselli convincingly (as Porta notes in his comment) supports Sraffa's interpretation on the basis that it is the only reconstruction we have which is compatible with Ricardo's recorded observations on the relative variations in wages and prices, compatible, that is, with the idea that Ricardo had some rationale for his thesis.

Sraffa and Keynes' General Theory

The preparation of the *Work and Correspondence* became Sraffa's prevailing activity in the 1930. Nevertheless, he continued to follow Keynes' work and was a witness to the changes occurring within it in those years. From new evidence contained in the *Sraffa Papers,* Panico and Ranchetti examine Sraffa's participation in the discussions on *The General Theory* and in particular his comments on Keynes' attempt to present an alternative theory of the rate of interest. From his notes on chapter 17 of the *General Theory* and his annotations in the text it is evident that Sraffa was critical of the analysis of liquidity preference and of the concept of own rate of return introduced by Keynes. With regard to the liquidity preference theory, on which Keynes

based the determination of the interest rate, Sraffa maintained that a unique functional relationship between the rate of interest and the quantity of money held did not exist, and thus the descending curve which for Keynes represented liquidity preference was equally non-existent. Yet, as Sraffa admitted, the fact still remained that 'abundant cash and low interest go together': to explain this empirical phenomenon, Sraffa argued that the causal order should be reversed, i.e. the low interest causing the abundant money. As Ranchetti points out, in this way Sraffa focused on the behaviour of the banks. Concerning the notion of own rate of interest, Sraffa believed that Keynes confused the idea with that of the marginal efficiency of capital. Certainly these amount to strong objections, which seem to confirm the historical Cambridge rumour that Sraffa judged Keynes' *General Theory* a 'confused' book. On the other hand, as Panico and Ranchetti both emphasise, Sraffa commented positively on those passages by Keynes relating to the formulation of a conventionalist theory, which would appear to indicate some agreement between Sraffa and Keynes on a monetary and conventional determination of the rate of interest.

The development of Sraffa's programme of research

In 1941, when the bulk of the work on Ricardo had gone to print, Sraffa returned to his research programme, and so began a new phase in which from his notes it now appears he was led to the correct formulation, in terms of equations, of some of his 'classical' propositions. As has been noted earlier, Sraffa had already tried to formulate his theory in terms of 'equations' in the late 1920s but had barely been able to satisfactorily overcome 'equations without a surplus'. Between 1941 and 1944, however, as Pasinetti notes, he really made a breakthrough. With advice from Besicovitch, essentially between 1942 and 1944 (see Kurz-Salvadori's chapter), Sraffa succeeded in correctly formulating equations with both a surplus and labour explicitly introduced, and discovered the notions of a maximum rate of profit independent of prices (the main issue discussed with Besicovitch), basic and non-basic commodities, and 'standard system'. At this point, July 1943, the Mill papers were unexpectedly discovered and, from 1945–55, Sraffa's research was interrupted yet again.

Once the first volumes of Ricardo's *Works and Correspondence* were published Sraffa finally resumed his theoretical work, and from 1955 to 1959 he succeeded in transforming a part of his notes into a book, finished to all intents and purposes in 1958 yet published, amongst endless hesitations as Pasinetti reminds us, only at the end of May 1960. In this ultimate phase the help of his mathematician friends was essential; above all Besicovitch, who also insisted that Sraffa publish his work. Sraffa commented at the time: 'The fact that I was able to forsee interesting mathematical results shows that there must be something in the theory'; and this may well have induced him to put

aside his doubts and publish. Correcting the galley-proofs, he still carefully analysed suggestions from the mathematicians–Watson was of great help to Sraffa concerning corrections–though he did not always follow their advice. As Kurz and Salvadori show, situations arose in which Sraffa interpreted these suggestions as indicative of the fact that his presentation needed to be changed in order to avoid possible misunderstandings; at the same time there were also cases where he either had difficulties in understanding the concerns of the mathematicians or considered these concerns uninteresting from the point of view of an economist. In the circumstances of the latter, he simply set aside the problems. As Kurz and Salvadori conclude, the material presented from Sraffa's unpublished papers and correspondence testifies to the independence of Sraffa's mind and his skepticism with regard to any rational proposition he himself could not master.

The outcome of the programme of research: *PCMC* or *A Prelude to a Critique of Economic Theory*

The content of the book

PCMC is a book of a hundred pages, dense in concepts, which Sraffa introduced as no more than 'a prelude to a critique of economic theory'. In fact it enters into little direct discussion of marginal theories of value and distribution, yet the book's propositions, so Sraffa wrote, 'have nevertheless been designed to serve as a basis for a critique of that theory'. The work is divided into three parts. Part I deals with single-product industries and circulating capital. It considers a single economic system consisting of many industries in which the absolute levels of all the physically specified inputs and outputs are given as data. Assuming uniformity of the wage rate, the rate of profit and commodity prices, Sraffa examines how relative commodity prices and the real wage will change as the rate of profit varies. In particular he demonstrates how the rate of profit is inversely related to the real wage. In the course of the discussion he introduces the distinction between basic and non-basic commodities, showing that basic commodities have the fundamental role in determining the relations between prices and distributive variables. Dropping the assumption of the given real wage, Sraffa then admits a degree of freedom into the analysis: the closure of the model requires that either the real wage or the rate of profit be given. At this point, 'in order to give transparency to a system and render visible what was hidden', Sraffa constructs a completely auxiliary function, the 'standard system', a hypothetical economy in which the composition of the means of production and the net product are the same: here, the inverse relationship between the wage and the rate of profit, expressed in purely physical terms, becomes linear. The final chapter of Part I deals with the notion of 'dated labour', by means of which the price of any commodity is represented as the

weighted sum of the series of wage payments incurred in the production of that commodity, the weights being different powers of (1 +r) where r is the rate of profit. Using this concept, Sraffa presents a numerical example to prove that the neoclassical idea of the period of production 'cannot be reconciled with any notion of capital as a measurable quantity independent of distribution and prices'. Part II of the book extends the analysis to multi-product industries, fixed capital and land: a number of complications are introduced but the basic structure of the analysis is substantially preserved. Part III develops the examination of the relationship between prices and distribution, considering the case in which changes in distribution lead to changes in the technique of production: contrary to the neoclassical theory of capital, Sraffa demonstrates that the capital intensity of production is not necessarily an inverse function of the rate of profit. In fact it shows that as the rate of profit is increased from zero, a particular technique may be chosen, then abandoned and then chosen again, with the result that it is not possible to have a physical measure of capital intensity of techniques such that, as the rate of profit increases, the measure of capital intensity always falls as the technique changes.

Given the extreme conciseness of Sraffa's method of exposition it is no wonder, as Pasinetti notes, that the book seemed to its first readers disconcertingly cryptic and obscure. Something which caused considerable puzzlement was Sraffa's statement that 'the investigation is concerned exclusively with such properties of an economic system which do not depend on changes in the scale of production', the hypothesis according to which the quantities produced in the various industries are assumed as given.

A crucial assumption: the given quantities

The given quantities assertion has made many readers of the *PCMC* consider it only half (the supply side) of a system of general economic equilibrium. Roncaglia deals with this much-discussed question by introducing a distinction between the classical (and Sraffa's) approach on one side and the marginalist approach on the other in relation to the analytical structure and 'vision' of the economic process. The classical and marginalist approaches, Roncaglia asserts, are two 'paradigms' expressing two different conceptions of the way the economic system works. It is a difference that Sraffa pointed out in the conclusion of his book, in Appendix D, 'References to the literature'. There Sraffa contrasts 'the picture of the system of production and consumption as a circular process', as he characterises the classical approach, with 'the view presented by modern theory, of a one-way avenue that leads from "Factors of production" to "Consumption goods"'. According to the classical approach, the 'problem of value' does not consist in determining the equilibrium values for prices and quantities exchanged (and produced, where the model includes production) at the same time but, rather more simply, in identifying the

exchange ratios that satisfy the conditions for the reproduction of the economic system. The classical approach separates the problem of 'reproduction prices' from that of quantities produced and exchanged, enabling us to distinguish various logical stages in the economic process. Similarly, Roncaglia argues that in his book Sraffa rigorously defines the object of his analysis and thus the data necessary to bring it to a conclusion. On this basis, without there being any need for reference to demand, Sraffa determines production prices and the distributive variable, and analyses the movements of these variables in relation to the exogenous distributive variable.

Roncaglia underlines the fact that this procedure–i.e. the rigorous delimitation of the problem, reduced to the interplay of relationships between a limited number of variables–stands in contrast to the approach dominant in modern economic theory. Breaking down the representation of the operation of the economic system into different 'theoretical pieces' corresponds, according to Roncaglia, to a methodological direction which Sraffa seems to have developed in his exchanges with Wittgenstein.[10] Roncaglia's opinion on Sraffa's methodology is similar to that shared by Salanti and Signorino. The latter suggest that Sraffa's peculiar choice of what has to be considered 'given' may be justified as a rational strategy of research if one accepts 'piecemeal theorising' as a fruitful approach to the explanation of economic phenomena. In other words, they believe that the theory of value and distribution developed in *PCMC* should be regarded as a first approximation.[11]

Sraffa, Keynes and §44 of PCMC

In an often-quoted passage of his book, §44, Sraffa writes: 'The rate of profits…is…susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest.' Some authors have interpreted this passage as an implicit reference to Keynes' theory, in particular to chapter 17 of the *General Theory* where Keynes refers explicitly to Sraffa's notion of commodity rate of interest, attributing to the money rate of interest 'the predominating practical importance' in determining the volume of output and employment, and advancing a theory on the relationship between the money rate of interest and the rate of profits. Although Roncaglia and Lunghini consider such a reference obvious,[12] Ranchetti (see also Bellofiore's comment on Panico) maintains that this interpretation is questionable and certainly more complex. In any case, Sraffa's strong objections to some of Keynes' notions prevent us from reading this passage definitively as an endorsement of Keynes' way of relating the rate of interest and the rate of profit. What may be said, as both Ranchetti and Panico maintain on the evidence of documents from the *Sraffa Papers,* is that Sraffa and Keynes both strongly argued for the development of a conventionalist theory of distribution.

Concluding remarks

At the end of his 'archival excursus' of Sraffa's theoretical evolution, Pasinetti emphasises that only a fraction of Sraffa's original research programme has ever been realised. Above all else, reassessment and the progressive restriction of horizons came to affect Sraffa's major interest, that of the critique of marginal economic theory. In the end, paradoxically, Pasinetti notes, little remains in the nature of an explicit critique, even though it had been Sraffa's primary objective since the beginning.

Some authors claim that, from a methodological point of view, Sraffa's entire work seems to have been continuous. Continuity, first of all, in the necessity of logically sound theoretical arguments, of which Sraffa was undoubtedly convinced throughout his entire intellectual life. Yet also, according to many interpreters, continuity in the adoption of a method of 'piecemeal theorising', to use Salanti and Signorino's term; an attitude to the relation between logical consistency and empirical relevance which Sraffa already showed in his 1925–6 articles. That is to say that Sraffa's book would represent a 'first approximation' to the problem of value and distribution: Sraffa's theory should be read as just one piece of a larger coherent framework in which many theories would have come together, each defining and solving specific problems. However, such a statement obviously raises the question of the creation and existence of this framework.

'If the foundation holds, the critique may be better attempted later, either by the writer or by someone younger and better equipped for the task', Sraffa wrote in the preface to *PCMC;* and, as Pasinetti says, many economists from the younger generations have not disappointed him. Sraffa's constructive role in the analysis of the relations between value and income distribution, in the most general production economic system, has by now been recognised; his analytical results concerning the 'standard system' and the relations between prices and income distribution have also been widely illustrated; and many of the proofs concerning the properties of his system of equations have been reformulated with the help of powerful mathematical tools (such as Perron-Frobenius theorems). Sraffa's critique has also been applied to other fields–in this book Steedman's chapter and Baldone's comment offer an extension to the open economy. Furthermore, Sraffa's analysis of the switching of techniques has been at the centre of a vast debate in capital theory which, thanks to the work of P. Garegnani, L.Pasinetti and others, has become a general critique of the logical foundations of the neoclassical theory of capital, and which as a result has revealed some of the neoclassical parables of aggregate production and distribution designed for a simple one-commodity world to be theoretically unrobust.[13]

By now, neoclassical authors fully accept the Sraffian arguments. What instead remains controversial, as Steedman (1988) notes, is the question 'of what type of capital, value and distribution theory (or theories) ought to be

maintained and developed' (Steedman 1988, I:7): in fact some authors believe that it is possible to accept Sraffian arguments and at the same time deny that they affect the Debreu version of neoclassical theory. Contrary to this there are others, associated with the development of the critique of neoclassical economics, who maintain that the neoclassical theory of capital exhibits the same 'paradoxes' in its traditional versions as in its more recent formulations, and consider the return to classical and Marxist modes of economic analysis as the logical consequence of Sraffa's work.[14] It should not be forgotten, however, that Sraffa's writings also contain destructive criticism of some of the central aspects of the classical and Marxist theories.[15] What is certain is that Sraffa's work has induced a revival of the theoretical approach based on the classical surplus concept, and the emergence of a Sraffian economics has been responsible for some interesting theoretical developments.

As we can see, Sraffa's work is the source of a widespread and ongoing discussion, one which prevents the formulation of a complete assessment of his work. However, it is possible to wonder what legacy Sraffa has left to economic science. We believe that Sraffa's fundamental (and problematic) legacy may be found in his lesson on absolute theoretical rigour with regard to the nature and limit of economic assumptions–as he said at the Corfu conference on capital theory, 'the theoretical measures required absolute precision. Any imperfections in these theoretical measures were not merely upsetting, but knocked down the whole theoretical basis' (Sraffa 1961:305).[16] This is probably a major reason for his prolonged impact on the economic science of the twentieth century.

Notes

1 I would like to thank Terenzio Cozzi, Giancarlo de Vivo, Pierangelo Garegnani, Jeoff Harcourt, Luigi Pasinetti and Fabio Ranchetti for their helpful comments on a previous version of this introduction.

2 The period of Sraffa's formal education is examined in some biographical studies (see Pasinetti 1985a and b; Potier 1991; Naldi 1998a and b). In this book it is further investigated in two chapters by Naldi and D'Orsi.

3 In his chapter Ranchetti recalls that Keynes was struck by the sharpness of mind which the young Sraffa showed in discussing speculation and hedging on future markets.

4 Although Marshall was 'not' economics in Italy, after 1889 the English economist was probably the most authoritative representative of the marginal school of economics in Italy: two distinct centres of influence of Marshallian economics were the University of Rome, with Maffeo Pantaleoni and Enrico Barone, and the *Laboratorio di economia politica* in Turin (see Gallegati 1990; see also Cavalieri's chapter below).

5 With regard to this, it is interesting to note that both D'Orsi and Naldi hypothesise that the relationship with Antonio Gramsci was also important in the development of Sraffa's own approach to political economy. Sraffa met the Marxist philosopher and political activist Antonio Gramsci during his university years (probably in 1919) and became a close friend. Sraffa attended the political debate that took place within Gramsci's *Online Nuovo* circle between 1919 and

1925. When Gramsci was incarcerated by the Fascists in 1927, Sraffa provided practical support to him and his family and indirectly corresponded with him (through Tatiana Schucht, Gramsci's sister-in-law). After Gramsci's death, Sraffa helped to ensure the safe transfer of his notebooks to the Soviet Union (on the Sraffa-Gramsci relationship see Potier 1991 and Fausti 1998).

6 Marchionatti notes that, before Sraffa, the American economist Frank Knight, who in turn took and developed criticism by Allyn Young and Joseph A. Schumpeter, was probably the most important critic of Marshall's and Pigou's theories of value. As is well known, Arthur C.Pigou was Marshall's successor as Cambridge Chair of Political Economy.

7 In his years at Cambridge Sraffa did have some important intellectual relationships: two of these deserve to be remembered, namely with Wittgenstein and Keynes. The friendship with Ludwig Wittgenstein probably began in 1929. Although very little is known about the exact nature of their discussions, many scholars have assumed that there existed an important intellectual exchange between the two (and between them and Keynes on methodological issues, see Coates 1997). Roncaglia and Lunghini discuss some aspect of this relationship. The relationship with Keynes is better known. It is partly discussed in Ranchetti's and Roncaglia's chapters below: though Skidelski (1992) speaks of it as 'a case of no communication', it would seem rather to have been instead a case of communication, although difficult.

8 The fact that Sraffa could have elaborated his theoretical position autonomously should be no surprise: the appearance (or re-appearance) of similar theoretical positions in different places independently is certainly not unusual in the history of ideas. What is theoretically relevant are the close similarities between Sraffa and some of those economists in the interpretation of classical economists and the analytical translation of their vision of the economic process. For an examination of the theoretical relationship between Sraffa's central proposition and the Bortkievicz-Dmitriev model see Marchionatti and Fiorini (2000).

9 An interesting outcome of Sraffa-Hayek debate is the impact of Sraffa's criticism on Hayek, examined by Zappia and De Vecchi below: Zappia maintains that it was crucial on the development of Hayek's thinking and contributed to the critical review of the equilibrium theory which Hayek undertook in 1930s; however, according to De Vecchi, such a proposal suffers from an excess of emphasis.

10 Lunghini notes that *PCMC* and Wittgenstein's *Tractatus* show significant agreement, and there is no reason to suppose that Sraffa should not have modelled the epistemology of *PCMC* upon the *Tractatus* rather than on the *Untersuchungen,* on whose development he was so influential. Keeping in mind Wittgenstein's summary of the *Tractatus* ('Its whole meaning could be summed up somewhat as follows: What can be said at all can be said clearly; and whereof one cannot speak thereof one must be silent.'), Sraffa, so Lunghini maintains, would emend both the grammar and logic of classical political economy.

11 Of course this approach seems appealing but, as Bianchi notes in her comment below, it raises various questions about the relationship with other contributions and theories.

12 Roncaglia maintains that Sraffa's and Keynes' analyses refer to a largely shared conceptual framework. In particular, Roncaglia notes, they reject the price-quantity equilibrium associated with the full employment of resources. Lunghini prefers to speak of two different strategies of radical criticism of orthodox economic theory.

13 Harcourt (1972) offered an in-depth analysis of the history of the capital controversy which was also important in alerting the profession as a whole. See

also Eatwell, Milgate and Newman (eds) (1990) and in particular Garegnani's and Pasinetti's and Scazzieri's papers.

14 This position is clearly expressed in Garegnani's works. See, for example, Garegnani 1998b and 2000.

15 See Napoleoni (1976).

16 At the Corfu conference Sraffa intervened in the discussion of Hicks's paper, 'The measurement of Capital in relation to the measurement of other economic aggregates'. Sraffa emphasised the distinction between the two types of measurement, that made by statisticians and that by theorists: whilst in the first case approximation is acceptable, the second requires 'absolute precision': 'if one could not get the measures required by the theorists' definitions, this was a criticism of theory, which the theorists could not escaped by saying that they hoped their theory would not often fail. If a theory failed to explain a situation, it was unsatisfactory' (Sraffa 1961:306).

A memoir

Sergio Steve

Professor Sayers once told me he had proof that Keynes brought Sraffa to Cambridge in order that the young Italian challenge the Marshallian orthodoxy which Keynes considered so oppressive there at that time. And Keynes' plan worked: Sayers distinctly recalls, when he was a student, how Sraffa's arrival altered the study of economics at Cambridge. That insular, isolated orthodoxy was replaced by a new critical awareness and a genuine interest in continental economists.

Keynes clearly understood Sraffa's formidable critical capacities. And Sraffa himself was well aware of his own overtly critical intelligence. He claimed, for example, to have given up teaching because he was incapable of offering his students anything except negative propositions, and this he saw was wrong (and when, due to a shortage of teachers during the war, he began again giving lessons, he taught applied and not theoretical economics). I know from personal experience that this conviction, his critical persuasion, troubled Sraffa. I once accepted his advice not to publish one of my works; and afterwards he reproached himself, telling me with sadness in his eyes, 'that's another example of entirely negative criticism'.

Although Sraffa never rejected attempts to draw positive conclusions from *Production of Commodities by Means of Commodities,* I believe that this work always had critical intentions. And if this is true, it would help resolve the apparent ambivalence in Sraffa's thought between the idea that theory must have a logical, watertight structure and the notion that economic reality is such that it may be understood only in a very general or gross way.

Sraffa was not interested in theory if it was not rigorously precise: he refused to develop the study on intermediate market forms because he said he had quickly understood that it was impossible to formulate a unitary, coherent theory about them. Similarly, in a brief intervention at the International Economic Association Convention in Corfu, Sraffa outlined a distinction between statistical measurements, which could be approximate, and theoretical measurements, which instead had to be exact (Lutz and Hague 1961).

Yet at the same time, Sraffa still believed in the potential, in the understanding of economic affairs, of research that did not necessarily satisfy

the strictures of such exacting theory. This is evident above all in his advice to young research students to deal with concrete problems, using even homemade methods, rather than highly theoretical problems. I remember he wrote to me once that he had appreciated a thesis entered for the Bank of Italy Stringher Grants because it was 'a limited but first-hand research work and, it seems to me, the kind of thesis to encourage, unlike many others which rewrite the theory of the universe' (personal letter, 23 February 1970). And I think the reply he gave me when I asked his opinion of Keynes, in 1946 or 1947, may be interpreted in much the same way: 'There are many things wrong in Keynes' work, many things which are unnecessary or unnecessarily complicated. However, he has changed the direction of economic thought.' He explained: 'Because anyone is capable of sitting at a table and listing all the hypotheses possible, but the real economist is the one who can identify those which are actually relevant.' Clearly the work of Keynes, despite its errors and defects, was fundamental.

The ambivalence in Sraffa's thinking may be explained if we see the objective of *Production of Commodities* to demonstrate that marginal theory falls short in its logical structure and is unsatisfactory as an interpretation of economic actuality. And this criticism of the dominant theory may easily be extended, in Sraffa, to any mechanistic conception of economics. Thus, the notion of an irrefutable, rigorously logical theory ultimately does not represent a concrete alternative to an empirical knowledge, which is by definition not rigorous, of economic reality.

Sraffa tried to involve his close friend Blackett, Nobel prize-winner and President of the Royal Society, in the problems surrounding the measurement of capital. However, Blackett was not interested, saying that in physics one studied things which were much more general than those which Sraffa had already rejected in the theory of capital. Although Sraffa did not agree with Blackett's position, this did not mean that he believed that economics boasted a greater precision than physics. The situation may again be explained if we see Sraffa's aim as unequivocally critical.

(I should say that there is at least one argument against such conclusions: that is, the little importance which Sraffa gave to the theory of uncertainty. I have myself at least one recollection with regard to this, from 1947 or 1948, which would support the conviction that economic theory can still be formulated even considering as inessential one of the factors most opposed to the requirement for coherent logic. However, I should add that perhaps some twenty years later I told him that, for me, the difficulties of economic theory arose from having a primary material made of both facts and ideas. He made no objection to this, remarking only: 'Medicine is the same.')

Sraffa's critical stance influenced all aspects of his life. From pure theory to an article in the newspaper, nothing could be taken as read; everything had to be discussed and studied in order to discover its limits and its defects. This is where his capacity to challenge and destroy the thought of others came

from, with regard to which Wittengenstein has a notable recollection. Yet this was not born from an opposition to the ideas of others but rather an acute perception of the thoughts of those with whom he might be discussing. And allied to this was his willingness to help others, an aspect of his generosity, about which there exist many examples elsewhere.

His critical conviction sometimes got in the way of his work. In his introduction to the work on Ricardo, Sraffa used to say that he had had to follow Dobb, because when Dobb found himself in front of a difficult problem he would temporarily put it aside and go on. Sraffa instead would stop until he had resolved it.

His critical principles did not stop him completing the book on Ricardo, however; a book noted for its scholarly perfection and depth of analysis. Yet perhaps the labour behind the book still remains underestimated. Unending was the research of texts and the verification of those facts which could help reconstruct the life of Ricardo; and the effort to command an understanding of the social and cultural life of Great Britain and the continent at that time, in order to provide a complete picture of the world which Ricardo inhabited. It was for work like this, and Sraffa's competence, that Keynes was able to say of him: 'Mr Sraffa from whom nothing is hid' (see Keynes 1933).

The work on Ricardo is in itself sufficient to disprove the common assumption that Sraffa was a lazy and inconclusive man. And at the same time there exist many more, little-known examples of his work: on the witches' trials, on the curate Meslier's testament, and above all on Saint-Simon and the Saint-Simonians–research conducted in the French archives for years before being abandoned because the authors were considered too tedious.

There was also Sraffa's extraordinary decision-making capacity: following a quick yet studied evaluation of a situation, he always arrived at the most effective solution. An example of this exists in Wittgenstein's biography, concerning Sraffa's advice about what Wittgenstein should and should not do after the Anschluss in 1938 (Monk 1990). This was typical of Sraffa, at his best when taking decisions and helping friends in need.

I would like to conclude with the words written for the occasion of Sraffa's commemoration service, in Rome in October 1983, by his cousin Paola Pellizzi: 'Piero, dear, modest, resolute in allowing no one to give him any importance. It would have been more his style to disappear into thin air.'

The rejection of power and success, to an almost incomparable extent, represented the real Piero Sraffa. Yet this did not prevent him from leaving behind a deep and lasting impression.

Part I

On Sraffa's biography

1 A child of *Cultura Positiva*

Turin and the education of Piero Sraffa

Angelo d'Orsi

On 29 November 1920 in the faculty of law at the University of Turin, student number 7150 Piero Sraffa presented his degree dissertation: the title was *L'inflazione monetaria in Italia durante e dopo la guerra* and the supervisor was the professor of public finance, Luigi Einaudi. This was the conclusion to Piero Sraffa's university education and the beginning of a brilliant career as both teacher and researcher. A career which kept Sraffa in Italy, via Perugia and Cagliari, only until 1927; clearly a short period but equally one which has been described as 'probably decisive'.[1] The intention of this chapter is to analyse that adjective 'decisive' which Faucci (1986) used (qualifying it by a doubt which may simply have been rhetorical) to describe the influence of Turin, and more generally Italy, on Piero Sraffa. In later years, according to Luigi Pasinetti among others, Sraffa would never speak in flattering terms of his 'garzonato universitario' (university apprenticeship)–to use the words of another Turin University student and close friend of Piero, Antonio Gramsci.

Gramsci's own university education, characterised by unfinished studies and difficult personal experiences, certainly differed from that of Sraffa, who, beside his intellectual ability, was the son of a university lecturer and thus a student in a privileged position. The son of a university lecturer, it should be said, from the same faculty at the same university. Born in Pisa in 1865, Angelo Sraffa graduated in law in 1888 and became a professor of commercial law at the University of Parma a few months after the birth of his son. He remained at Parma until October 1913, when he transferred to the faculty of law at the University of Turin, the city where he already resided with his family and indeed where Piero had been born on 5 August, 1898. From Turin, Sraffa senior went to Milan where, in 1919, he became Rector of the *Università Commerciale Luigi Bocconi.* There, one year later, he founded the *Istituto di Economia Politica Ettore Bocconi* and appointed Luigi Einaudi as its director; Einaudi had been a faithful student of Salvatore Cognetti de Martiis who, almost thirty years earlier, had founded a sort of archetype in the *Laboratorio at Economia Politica.*

In general terms it would appear that beyond their individual specialisations the staff of the law faculty of Turin shared a certain awareness

of real circumstance and a willingness to communicate to society at large rather than solely to academia. The historical importance of that time should not be ignored in this respect: the immediate post-war when all educated men seemed totally committed to their work. Nevertheless this awareness does seem to have been a peculiar characteristic of the University of Turin as a whole, particularly in the period between the last decade of the nineteenth century and the first two of the twentieth. During that time in a city whose culture was above all academic there was a genuine exchange of knowledge: between different disciplines and between the world of the university and the surrounding militant culture, from the formation of associations to the actual political arena. The period nicknamed the 'socialism of the professors' was indeed testament to this (see Spriano 1972; Pogliano 1979; Bergami 1993). The social direction of the cultural activity would seem very much to characterise the academic world of this city, starting from, naturally, the humanist faculties, and first and foremost the faculty of law.[2]

The teaching of law, economics, history and political philosophy, although to differing extents depending on the subject and not always in a entirely distinct way, seemed to distance itself from the formalist tradition. If I wished to be generous, I might say that the law lecture theatres tended to advocate a society based on social awareness and civil participation rather than simply providing a professional education and supplying the job market with aspiring solicitors and legal officials. This would have been the case in particular with the fundamental courses: Constitutional Law, Public Finance, Philosophy of Law, Political Economy, History of Italian Law, Ecclesiastical Law, History of Roman Law and International Law. The faculty of law in Turin produced not only lawyers or economists or political experts, but also philosophers, men of letters, patrons of the arts and sciences, and statesmen.

Law, the chosen subject of Piero Sraffa, was the leading faculty of the university in terms of the number of admissions; it remained so until 1917–18, when it was superseded by medicine–temporarily until 1923–24, definitively so from 1928 onwards (see Schiavone 1993). Equally in terms of the quality and notoriety of the teaching staff, law and medicine were historically the most distinguished faculties. Both schools had undergone what could almost be called a renaissance in the period immediately before the unification of the country, due in part to the influx of foreign professors, some of whom were highly renowned. This renewal, which acted as an impetus to the educational establishment as a whole, was aided and in turn helped by scientific and didactic publications on a national scale. All this took place under the general principles of a sort of positivism (although I prefer to term it *cultura positiva* or 'positive culture'), of which Turin was a vital centre and even perhaps, as has been suggested more than once, the authentic capital. In the last two decades of the nineteenth century and up to the Great War (and even the immediate post-war), the academic world of this city in its widest sense, in close relationship with other civic and cultural institutions,

produced its greatest results. The period immediately following, in which Piero Sraffa attended the university, saw in some sense the beginning of a slow decline; in its entirety, though, Turin still remained one of the most important university cities in the country. With regard to the faculty of law, the importance of Turin did not diminish and would not for a long time (Pene Vidari 1980; Schiavone 1993).

Much more than the faculty of literature, which was noticeably behind in terms of education, the faculty of law was a veritable intellectual centre. It should not be forgotten that many students at that time followed joint degrees (in law and literature) or frequented courses in other faculties as listeners. For a faculty somewhat outside the sphere of law, the above mentioned *Laboratorio di Economia Politica* and its courses were still much sought after. Cognetti's creation crossed over the confines of the university faculties, initially working with the *Museo Industriale* which then became the Polytechnic in 1906 after its union with the *Regia Scuola d'Ingegneria*. The motto of the *Laboratorio* was considered to be *'Haec placet experientia veri'*–a declaration of scientific spirit and intention in the best positivist tradition. The economists of the *Laboratorio* identified with the *Istituto Superiors di Studi Commerciali*–founded in 1913 by the university, based on the *Istituto Bocconi* in Milan–in its practical endeavour to prepare young people for employment in commerce and related professions. More than a generation of students, and not necessarily all economists (much like the law graduates were not all destined to be lawyers), would receive an important supplementary education from the *Laboratorio*.[3] There is perhaps little need to emphasise that for the whole of the nineteenth century and obviously not only in Turin, economics, within the bourgeois system of knowledge, had 'the character of a principle science, in a certain sense a universal social science' (Bulferetti 1951:122). In Turin, interest in the science of economics noticeably increased within the *Accademia delle Scienze* after the creation of Cognetti's *Laboratorio,* which went on to employ many of Cognetti's followers, from Luigi Einaudi to Pasquale Jannaccone.

Cognetti's temporary successor as director of the *Laboratorio* (1901–3) was Gaetano Mosca, a 'constitutionalist' well-versed in political science and open to the influence of both history and economics. Following the illness and sudden death of Cognetti, Mosca, in memory of its founder, concentrated on the function of the institution: the *Laboratorio* was to be not only a school of erudite economists but also a centre for the collection of data and documents, a vital reference point for all those involved in the social sciences. An institution characterised–as Einaudi noted–by the coexistence of different political and scientific persuasions in a climate of peaceful confrontation was in fact the explicit wish of its founder (Einaudi 1901).

Two essential features emerge from *Laboratorio di Economia Politica*–as they do also from the faculty of law as a whole and the university in general–which appear significant in our consideration of the influence of Turin on

Piero Sraffa. The first is an attitude towards society, the outside world, which may be described as interventionist, from the extra-curricular cultural activities to real political action. The second is the *cultura positiva:* a collection of attitudes towards learning and the formation of collective knowledge based on rigour, systematic organisation, completeness and scientific verification. A noticeably Piedmontese trait of this culture was historical-philological: verification rather than judgement, reconstruction before conceptualisation; and precise verification, exact reconstruction. Economists, lawyers and social scientists from the faculty that would also admit Piero Sraffa all placed a particular emphasis on a knowledge of history as the ineluctable basis of science (Grosso 1971).

Having started university in 1916, Sraffa was in some senses fortunate to belong for at least the first half of his degree to the corps of soldier-students: it was sufficient to attend examinations in uniform to gain the patriotic sympathy of the board of examiners, which in practice meant easy questions and high marks. A glance at Sraffa's university career would seem to confirm something of the sort.[4] He sat three examinations in his first year–Civil Rights Institutions, Political Economy and Statistics–which was by no means unusual, except that they were all taken on the same day, 22 October 1917, and all received maximum marks. The board of examiners was also the same each time. Its chairman was Giampietro Chironi, professor of civil rights who also taught Civil Rights Institutions. Although a disciple of formalist method and a virtual stranger to that 'socialism of the professors', Chironi was concerned enough about civil society and the social question to have been given political appointments alongside his academic role. Unfortunately Chironi's credentials were not matched by the other two members of the board: Antonio Castellari, professor of Civil Procedure (a second-year course which Sraffa passed in May 1918, once again with full marks), and Francesco Cosentini, an untenured Philosophy of Law professor. In other words, the professors of two of the courses were missing for the final examination: Jannaccone (Statistics) and Loria (Political Economy), two course teachers and two economists. Thus two economic courses were examined by a board composed of three law professors on a board absent of economists.

In his second year, Sraffa sat Ecclesiastical Law and Civil Procedure on the same day, 28 May 1918. Ecclesiastical Law would seem of little significance were it not for its professor, Francesco Ruffini, conscientious historian and excellent lawyer who in the first thirty years of the century was one of the leading personalities at the university and in Piedmont society in general. Ruffini was a public personage of immense influence who, after his initial involvement in the nationalistic fervour that greeted the end of the First World War, progressively became an adversary of fascism, refusing in 1931 to swear an oath to the regime–required of all professors at that time–and thus losing his position (a fate shared by his son Edoardo, graduate of Turin and professor at the University of Perugia). Ruffini though was not

part of Sraffa's examining board on that day, which was composed instead of Castellari, as chairman, Cosentini and Federico Patetta, another formidable figure at the university who went on to become chairman of the *Reale Accademia d'Italia.* A fascinating character, considered a humanist–though one 'brought up in the complete affirmation of historical method'–with a rather over-erudite conception of knowledge, Patetta was professor of history of Italian law, a course which Sraffa would pass in July 1919.

A month later, in April 1918, Sraffa sat two, more closely-associated examinations, Institutions of Roman Law and History of Roman Law, again both on the same day and before the same examiners, namely chairman Giovanni Pacchioni, professor of Roman Law and history of Roman Law, the by now ever present Castellari, and Cesare Civoli, professor of Law and Penal Procedure. In 1919, under Pacchioni, Sraffa sat one of the principal examinations, Roman Law, passing yet again with full marks. Pacchioni, who replaced Vittorio Brondi as dean of the faculty in 1919, was a notable scholar and historian of both Roman and civil law. He could perhaps best be defined as a humanist, ever ready to repulse any formalist conception of the law, of which he retained 'the aspect most human, most profound', according to one of his students. Within both the faculty and the entire university Pacchioni was one of the few professors with foreign teaching experience, having worked alongside Chironi at Innsbruck before coming to Turin in 1904. In 1925 he became professor of civil law at the newly-established state university in Milan; this was at the behest of Angelo Sraffa, to whose person and family he was attached by strong bonds of affection.

Sraffa senior, as already said, taught at the University of Turin; his subject, Commercial Law, was a compulsory one and thus Sraffa junior had to sit the examination. As was customary in such circumstances, Sraffa senior was not on his son's examining board, which was composed of Pacchioni as chairman, Gino Segrè, then professor of institutions of Roman Law, and Gioele Solari, recently appointed professor of Philosophy of Law. Out of flattering respect or the subterfuge of the examiners, or perhaps simply due to the candidate's outstanding ability in that particular subject, Piero passed with honours; somewhat disconcerting in that it represents the only honours classification in a degree curriculum which nevertheless was, as should by now be clear, still replete with maximum marks in practically every subject.

Piero Sraffa clearly learned from his opportune period as a soldier-student, and even after 1918 continued successfully to pass several examinations together. On the day he sat Commercial Law he also took full marks for Roman Law before the same examining board (Pacchioni, Solari and Segrè): it would again seem to be the case of two subjects at a single sitting. And evidently not content with his success, the following day Sraffa sat Civil Law, again before the same board; and thus Gino Segrè, one of the greatest Romanists of the time, did not actually chair, that is preside directly over, any of Sraffa's examinations. Sraffa would have had to follow Roman Law taught

by Segrè in the 1919–20 academic year, although it obviously cannot be established whether or not he actually attended classes. It is worth noting how Segrè's work was characteristic of the repeatedly identified cultural and educational mentality of the faculty in its attention to historical fact and its defence against any formalistic abstraction.

There was nothing formalistic either, except in the sense of the exercise of formal logical reasoning, about the subject of Luigi Einaudi, Public Finance. As well as the subject's conceptual-theoretical part, Einaudi also considered history, examining social-economic thought within concrete solutions to the fiscal and financial problems of countries, with reference to actual current situations. Einaudi was an intellectual with wide interests; besides his university career he wrote for *La Stampa* and then *Corriere della Sera,* a position certainly more important at the time than a parliamentary seat and even some ministerial posts. An authority within the *Laboratorio di Economia Politica,* Einaudi re-launched the journal *Riforma Sociale* which became practically the voice of the *Laboratorio* (Giva 1986). Sraffa passed Public Finance with maximum marks in front of an examining board made up of Einaudi, Castellari and Carlo Toesca di Castellazzo.

In 1920, the final year of his degree, Sraffa sat six examinations, all of them with the professors of the respective courses. The first of these was Philosophy of Law, which was much more than simply a course in law. Under Gioele Solari, professor of the course since 1918, it was somewhere between history and philosophy, with an emphasis on those elements related to *cultura positiva* and philological-critical method. This was taught not in place of but rather in a unique and productive combination with a political-philosophical component, which took account of Idealism (from Hegel to Gentile) and the particular social thought that had found such a vibrant home in Turin in the preceding years. Solari did not become a socialist, preferring to speak of his own position, precisely for its philosophical-political affiliations, in terms of 'social idealism'. Nevertheless, it was an idealism which did not forget the positivist tradition (with explicit reference to Comte) in both its content and its method. Luigi Firpo, student of Solari at the beginning of the 1930s, described the Solarian system as follows:

> 'From the historian of ideas he took the mistrust of abstraction, dogmatism and simplifying schemas; he took the sense of the complexity of historical debate, the countless interventions and revisions, and the inexorable problematic surrounding questions of human society; he had an indignant aversion to hasty judgement, which entailed his own meticulous analysis of texts and extensive bibliographical research.'
>
> (Firpo 1983:271)

It is worth concentrating on this description, for perhaps one can read something which goes beyond the life of a single scholar. Unlike the majority

of the law faculty, Solari was not a public figure, moreover he completely refused any other appointment within the university beyond his actual teaching position. Being a supporter of anti-fascism during the regime, he would never even become dean of the faculty. Yet he taught several generations of men who, though they were mainly professional scholars, would not be alien to the concept of civil and sometimes political duty; almost all of them joined the anti-fascist movement and, some, the Resistance. Solari was a teachers' teacher, and among his students (from Piero Gobetti to Norberto Bobbio) one clearly sees the 'civil function of university teaching' to which Bobbio refers (Bobbio 1986). In fact this was a characteristic of the whole law faculty, which remained one of the main centres of the intellectual life of the city, regularly co-involving other faculties through educational, scientific, political and personal discussions.

Gaetano Mosca could hardly have been more different than Gioele Solari: Mosca was the public man par excellence, yet he would not actually form his own school of thought. He arrived at the faculty in 1896, the year of the publication of his most important work *(Elementi di scienza, politica),* which virtually defined the theory of the political class, but he was not an academic creature like Solari. His roots were in civil society, and he easily established useful relationships with the social and intellectual life of the city; and with his election first to the chamber of deputies then to the senate, he contributed to the political life of the nation as well, becoming under-secretary in the Salandra government. Mosca transferred to Rome in 1924, where he became the first professor in Italy of history of political doctrines (and institutions).

On the same day as the examination of Constitutional Law (Mosca's original discipline), Sraffa also sat International Law, obtaining the usual result in both. Some days before, Sraffa passed the examination of Legal Medicine with another luminary of the university–Mario Carrara, son-in-law of and successor, in his position in the faculty of law, to Cesare Lombroso. Like the two Ruffinis and Sraffa himself, Carrara would give up his university position after the introduction of the fascist oath in 1931, each man avoiding it in different ways (Goetz 1983). Carrara's subject was by far the most foreign for a student of a social science such as law, and indeed is the only one in which Sraffa did not pass with full marks, obtaining 27 out of the maximum 30.

In June Sraffa passed Administrative Law with Vittorio Brondi and in July he sat his final examination, Penal Law and Procedure, which was taught by Cesare Civoli. Among the professors of these last examinations, Brondi was the most distinguished. Dean of the Faculty from 1916 to 1919, thus during Sraffa's degree, Senator of the Kingdom and University Rector from 1922 to 1924, Brondi spent his whole career at Turin. He was an exponent of scientific method, and his research tended towards civil concerns, in particular dealing with charitable institutions and abandoned children. Within this field he occupied positions in national and international organisations.

Knowing he would graduate in the autumn, Sraffa, one would imagine, wrote his thesis in the summer of 1920. *L'Inflazione monetaria in Italia durante e dopo la guerra*–sixty-four typed pages, twenty to twenty-two lines per page and about sixty letters per line–a work rich in dates and data, full of contemporary references from the war and post-war period but also containing historical examples from the nineteenth century and also much earlier. I cannot say if Sraffa's work properly follows the regulations for degree theses as written by Gaetano Mosca when he was dean; probably not formally, at least not in relation to the 'propositions or questions around a controversial point of positive law or political economic science' which the candidate should pose and then resolve with an answer that is not an 'evident solution'. However, it cannot be denied that Sraffa's thesis was an 'original and comprehensive dissertation, in relation to the chosen theme, on a subject which will become teaching material in the faculty of law' (*Norme per l'esame di laurea in giurisprudenza,* in Historical Archive, University of Turin (Archivio storico dell'Università di Torino), X C 95: the text is signed 'The Dean Gaetano Mosca'). The few, short written observations made by Luigi Einaudi on the original copy, kept in his library, show that the Public Finance professor found himself in front of the finished product; they are the comments of a reader discussing an essay. The hypothesis has already been put forward that Einaudi's contribution to the preparation of the thesis was minimal; perhaps one can go further, with the suggestion that his role was even more modest, practically nil.

The degree-awarding commission met under the chairmanship of Giovanni Pacchioni, who was dean of the faculty, which was normal practice at that time. The commission included: Gaetano Mosca, Pasquale Jannaccone, Giulio Diena, Alessandro Garelli, Gioele Solari, Luigi Einaudi, Riccardo Fubini, Giuseppe Prato, Francesco Cosentini and Valerio Cottino. Thus Achille Loria was absent; as he had also been in the examination of Political Economy. It is somewhat difficult to imagine an unexpected, alternative commitment for this notable member of the Italian senate. It would seem rather that his absence was premeditated, considering that Einaudi's copy of the work has three other names–Einaudi, Mosca, Fubini–hand written beside the candidate's name and the forthcoming date of the viva. In all likelihood these were the names of those who were to hold the viva, ensuring the examining board awarded maximum marks with honours. However, Sraffa's chosen discipline was not in fact taught by Luigi Einaudi but by Achille Loria.

Despite his apparently Marxist leanings, Achille Loria was professor of political economy in the faculty of law and became dean of the faculty in 1913. Undoubtedly on account of political and ideological prejudices, however, he would not be accepted into the *Accademia delle Scienze*. This was also due to a widely held scientific opinion about him that was far from favourable. As a youth, Einaudi had been enthusiastic about Loria, who had

showed him respect from the very beginning of their relationship. Loria it was who would encourage the student Einaudi to 'persevere along the difficult path of economic studies' (Einaudi to Loria, 4 February 1900, in Archivio di Stato di Torino–Carte Loria, hereafter AST-CL); an active and effective encouragement, considering that the role of Loria would become important in the academic career of Einaudi.[5] Only later did Einaudi distance himself from the 'Italian Marx', together with the end of his attendance at the socialist youth meetings; yet he would keep a not insignificant heritage, beginning with the idea of the class struggle as the driving force of both the economy and the entire society. Nevertheless, the relationship between the two men remained formally close, based on mutual respect and support. There is indeed no reason to doubt Loria's sincerity when, giving thanks for some books or extracts sent to him by the youngest member of the faculty, he maintains that he will 'learn a lot' from them; even more so when we remember that he frequently had serious theoretical discussions by letter with his correspondent.[6] Similarly, one believes Einaudi was not simply flattering Loria when he thanked him for the present of one of his books:

> I've already flicked through it; just browsing through it I had the desire to read it all, for its great interest in the problems and for that combination of thought and style which always make your books so profoundly appealing.
>
> (Einaudi to Loria, 13 November 1909, in AST-CL)

No surprise then that Einaudi himself, in 1932, should write the bibliography of his oldest colleague, receiving profuse words of thanks for it (Einaudi 1932). And on Loria's death, in 1945, Einaudi would also–thanks to the mediation of Sraffa–write Loria's obituary for the *Economic Journal*.[7]

Yet, since the beginning of the century when he started teaching at the university, Einaudi had been travelling in a different direction from Loria, towards an 'attempt to delineate an economic programme for the Liberal Party' on the one hand, and to 'monopolise the economic culture of the workers movement' on the other (Giva 1986:25). With regard to the latter, the effects were notable, not only the liberal tendency of Piedmontese socialism. It is probably mainly thanks to Einaudi that the local workers movement (and certainly not only the local one, considering the increasingly political and intellectual influence of the academic economist, who would become such an effective 'opinion maker' and, later, one of the most noteworthy national senators) discussed such ideas as Europeanism, federalism, decentralisation, the anti-bureaucracy controversy and the rise of the social struggle as the engine of progress in human society. Under the government of Giolitti, Einaudi insisted more and more on his conception of the absolute 'autonomy of industrial relationships from the political system'

(Berta 1986:75). In the Einaudi school of thought, the development of the organisation of the working class and the bosses on one side, and a constant increase in production on the other, are the prerequisites of ordered social development, founded on the management and control of social conflict, in a situation where the intervention of the state is the least possible.

This was the Einaudi that Piero Sraffa asked to be the supervisor of his thesis, for which he obtained full marks and for which he was congratulated by the same Einaudi. However, the liberal-conservative Einaudi should not have been the supervisor of Sraffa but rather the Marxist Loria, of whom Gramsci said: 'how many perfumed pearls' fall from his lips in a 'rich banquet' of nonsense (Gramsci 1980 [1915]:34). In effect, the discipline chosen for Sraffa's degree thesis was not Public Finance but Political Economy, which had been taught by Achille Loria since 1903. It is difficult to ascertain how much the young Piero Sraffa knew of the unflattering opinions which many people held towards Loria even before the war; the Loria for whose 'science', made up of 'words, words, words', Gramsci begged 'pity' (Gramsci 1980 [1916]:58). Although Sraffa met Gramsci later, it is likely that he already read *Avanti!* or *Grido del Popolo* during the war years, which contained some of Gramsci's ferocious criticisms (although published anonymously). If such were the case, one must imagine that the young Sraffa could not help but be affected. Although precisely how much he remained so we cannot know, especially since Loria also had his supporters, and first among them was Sraffa's own father. Certainly we have the testimony of Attilio Cabiati who, in a letter to Einaudi, explicitly refers to the 'intelligent' son of Angelo Sraffa being 'annoyed' by the idea of discussing his thesis (whose subject, Cabiati says, he personally suggested) with Loria.[8] The most likely explanation of Sraffa's rather negative attitude towards Loria is to be found in the influence of Gramsci, whom by then he knew, and the *Ordinovisti* group, which he frequented since 1919 (see Potier 1991; Fausti 1998). One imagines that the young student decided the subject of his thesis (political economy, which must have seemed more suited to him than financial science and financial law) and then perhaps in order not to waste time–throughout his university career there emerges a rush to finish–had no wish to transfer to another. We may not know the answers Einaudi gave to Cabiati's questions–'Would you have anything against being his supervisor [of the thesis]? And do we have to do anything in order to arrange this?'[9]–but we can imagine their tone. Einaudi would have answered that he was ready to discuss (I emphasise the word 'discuss' rather than 'supervise') the work of 'Sraffa junior' (Cabiati's expression); and possibly he may well have dissuaded the student from changing subject, perhaps relying on the likely absence of Loria during the graduation session.

One might object that at that time Gramsci, founder and director of *Ordine Nuovo,* also expressed severe criticism of Einaudi. Quite true but always within terms of an intellectual respect, which he had accorded Einaudi since

the time he was a literature student and–according to the evidence of another law student, Palmiro Togliatti, who graduated under Einaudi in 1915–had followed, out of his own interest, Einaudi's course in Public Finance (Togliatti 1967 [1949]). Gramsci, at the time when Sraffa was finishing his degree and the relationship between the two was just beginning, considered Einaudi as the constructor of a 'liberal utopia', where economic theory came up against the reality of men and things. The young socialist leader closely followed the theoretical movements of Einaudi: when he unfairly criticised Marx, denying even the merit granted him by another debunker of historical materialism, Benedetto Croce, namely 'to have established economic research within the study of history', Gramsci argued that the 'abstractionism' of which Marxism was accused belonged to liberal economic science, which 'has only the appearance of seriousness, and its experimental rigour is only an illusion' (Gramsci 1987 [1929]:40). In reality, respect for Einaudi, unlike Achille Loria, united men of different generations and ideas, most likely including Piero Sraffa.

Attacked by the young Gramsci, rejected by the *Accademia delle Scienze,* criticised from all quarters, Loria was still very good terms with Sraffa's father, even before they began working together in the same university faculty. And their relationship would continue even after Sraffa's move to the Luigi Bocconi University, in whose conferences–organised by Sraffa senior–Loria would participate (for example, two lessons on Ricardo and one on Marshall in spring 1925, within a series dedicated to the English economists, and the following year at the inauguration of the series dedicated to 'The present conditions of the British economy') (see the letter from A.Sraffa to A.Loria, 9 and 18 March 1925, and 7 April 1926, in AST-CL). It was Loria who signed two letters of introduction for the young graduate Sraffa on his first trips to England; although this was not on the direct request of Piero but through his father,[10] a clear sign that the relationship between Loria and Sraffa junior was not quite so close. Only when he had obtained what he wanted did Piero write to his 'illustrious professor', who he never actually met face to face at the university, to thank him.[11]

Considering all of this, one assumes then that in the preparation of his thesis, more than from his supervisor or the professor of the subject, Sraffa received help from other members of the Laboratorio, such as Attilio Cabiati. Cabiati, who we remember helped Sraffa switch supervisors for his dissertation, still regarded Loria as a role model 'from my youth' (A. Cabiati to A.Loria, 17 June 1931, in AST-CL). Untenured professor of political economy, he was a welcome associate of the *Istituto Superiore di Studi Commerciali* and notable contributor to *Riforma Sociale*. He was a lively member of economic and socio-political discussions in Turin in the first twenty years of the century, partly thanks to his position as deputy editor on *La Stampa*. Yet he got even closer to the socialist movement, collaborating with both *Critica Sociale* and *Grido del Popolo*. On behalf of

Alfredo Frassati, proprietor of the Turin newspaper, Cabiati asked Loria to collaborate with *La Stampa,*[12] from which Cabiati himself resigned definitively in 1918 after ongoing disagreements with Frassati. He then moved to Genoa, where he taught political economy, collaborated with *Secolo* and edited financial and banking magazines, following the *Bollettino* of the Italian Banking Association through its transformation into *Rivista Bancaria Italiana.*

Two more possible sources of help for Sraffa, both notable names on the degree examination commission, were Pasquale Jannaccone and Giuseppe Prato. The first was the successor to Cognetti de Martiis as editor of *Biblioteca dell'Economista* (the fourth series), in which Loria would publish his thoughts on currency and Einaudi his translation of Bagehot's *Lombard Street.* Between Loria and Jannaccone there always existed a certain distance and difference, but Jannaccone did admit his gratitude for the 'ferment of ideas' he had received from Loria's writings.[13]

In comparison to that of Jannaccone, Einaudi and Sraffa's father, the university career of Giuseppe Prato–who we know signed the degree statement of Piero–was modest; in the law faculty of Turin he would get no further than assistant professor (in Political Economy). He became a professor (also teaching Public Finance) in the less-important *Istituto Superiore di Studi Commerciali* of Turin (which would only become part of the university in 1935), and at the same time, thanks to Sraffa senior, obtained the position of assistant professor at the Bocconi University of Milan. As well as having been chosen by Einaudi as editor of *La Riforma Sociale,* he was admitted to the *Accademia delle Scienze.* A committed liberal, Prato–confirming the multiplicity of positions that existed under the liberal flag–also flirted with nationalism, then fascism, and collaborated, when Piero was no longer a student, with the political-theoretical journal of Mussolini, *Gerarchia* (in the good company of Croce and Gentile), though in terms of economic thought he would never be near fascism. Perhaps more than the liberal culture, Prato was influenced by a positivist education, in particular in his way of teaching economic history, his discipline of choice. Born in 1873, he died somewhat prematurely in 1928; and perhaps the words written about his friendship and work by Angelo Sraffa to Prato's widow, Emma Pozzi, are slightly exalted but certainly not without justification:

> 'Especially productive in a field which, in Italy, never had experts equal to him, nor even close: the good fortune and rare ability of the historian and the economist made him an irreplaceable master!'
>
> (A.Sraffa to E.Pozzi Prato, 27 August 1928,
> in AFLE-Fondo Prato)

A widespread opinion: for example Attilio Cabiati also commented, on receiving the book *Problem monetari e bancari nei secoli XVII e XVIII:*

> 'When I see the amount of work which you always produce with such originality, I envy you (I mean in the best sense): because I completely lack the fortitude that you demonstrate, enriching our economic literature with works of much greater value than small and overly specific theoretical disquisitions.'
>
> (A.Cabiati to G.Prato, 7 March 1916, in AFLE-Fondo Prato)

Riccardo Fubini–another from Sraffa's examining board–also congratulated Prato: 'for the marvellous direction of your studies, in which you excel in historical criticism, using the most modern research methods.' Prato also received thanks from the son of Riccardo, Renzo Fubini, a student of Jannaccone. Renzo was beginning his studies in economics when his father, on the suggestion of Einaudi, engaged him as an assistant of Prato at the *Istituto Superiors di Studi Commerciali*.

On a political level, Prato was conservative, and at times not far from certain reactionary positions; however, reaffirming the social and intellectual homogeneity of this group of intellectuals, he would admit to being grateful to the Marxist Loria–who helped him amid the vicissitudes of state exams and in 1904 appointed him as his assistant. With the passing of time and the succession of events, the conservative Prato and the prosocialist Loria became closer, especially over the Great War. To Loria–who closely followed Prato's first works and, according to what Prato himself said, was full of 'continuous and kind encouragement'[14]–Prato would dedicate a work and not be short on compliments.

The judgement of Gaetano Mosca, who in Prato saw an 'honest conscience guided by a lucid mind',[15] corresponds ultimately to this widespread opinion of Prato, of which the young Sraffa could not have been unaware in his relationship with him. The precise role of Prato in Sraffa's thesis has yet to be determined, but I believe that Sraffa was influenced by Prato's work. Though severe in his judgement of mediocrity, Prato was ever ready to acknowledge the worth of people, as we see in his friendship with the young Sraffa, a friendship facilitated by Prato's relationship with his Sraffa's father. In effect, Sraffa senior was instrumental in Prato's appointment in the faculty as assistant professor of Industrial Law. Angelo Sraffa's offer of the use of his library for Prato's preparation of the course was, even given his respect for Prato, quite uncommon. Prato's course, Angelo emphasised, was 'very much desired by us';[16] he also found the course programme 'simply splendid':

> I feel a certain pride to have encouraged a man of your great worth and wonderful preparation to study a branch of the new law which, in a part that I believe can really distinguish itself and organise itself autonomously, that is the part you will teach in the coming year, may receive because of your merits a worthy treatment.
>
> (A.Sraffa to G.Prato, 17 September 1914, in AFLE-Fondo Prato)

However that may be, Piero did not find Prato's course interesting enough to include it in his course plan.

On Sraffa's graduation, Cabiati would initially again act as mediator, this time between Sraffa, who left for Cambridge after a period in London ('a fond memory!'[17]), and Einaudi.[18] This was a relationship which began in 1921 and quickly intensified. For Einaudi–and for his friend 'Nino' (that is Gramsci), imprisoned by the fascist regime–Sraffa would frequently procure antique books, recent publications, missing numbers in journal collections, rare pamphlets; and he continued to do so from Paris, during his stay there in 1923. Indeed Einaudi, unlike Gramsci, nurtured a genuine passion for the book as artefact, a passion largely shared by his student Sraffa who fully appreciated the 'treasures' of the Einaudi library,[19] and willingly helped with the practical problems involved in the research of books and journals. The young graduate would also provide Einaudi with other assistance, news, addresses. In turn, the 'dear and illustrious professor' (as Sraffa regularly called him; the alternative being 'illustrious and dear', this to a man of by then high public office) did not spare his help in the requirements of his ex-student.[20]

Above all, Einaudi remained a source of advice for Sraffa in his research work: when the Royal Economic Society appointed him editor of the collected works of Ricardo, Sraffa wrote to Einaudi: 'I have begun the work, which will be very slow but very interesting. I will have many things to ask you, but for the moment I would be grateful for any advice which you may have' (P.Sraffa to L.Einaudi, 18 March 1930, in AFE-Fondo Einaudi). Judging from the rest of the correspondence, Einaudi too found this work appealing (a work which was followed from a distance, with avid interest, also by Gramsci[21]), offering many suggestions, which were always gratefully and attentively received by the ex-student. Here I would add that we also see a dimension emerging in Sraffa: the tremendous passion for knowledge, the constant philological attention; Sraffa the historian of ideas, in whom the astuteness of the researcher meets the zeal of the scholar. Like any good student with his master, Sraffa would end up having a scientific disagreement with Einaudi in relation to the Ricardian work.[22] His Turin schooling clearly showed its influence.

Among the relationships that Piero maintained in Turin after his move to England was that with Mario Lamberti Zanardi, which emerges in November 1931. Born in 1900, graduating in 1923, Lamberti was destined to an early death at 45 after a history of illness. He frequented the environment of Gobetti, collaborating with both *Rivoluzione Liberale* and *Baretti*. He then studied in Germany, in Heidelberg, where Lederer makes him fall in love with economic science, and he also dedicated himself to Bacon, Marshall and Schumpeter. 'Little but well thought out,' Einaudi would say of his work. Later, Lamberti began studying the Italian classics, Ferrara and Pantaleoni; and later still, whilst working for *Rivista di Storia Economica,* he would wish to edit *Prefazione* by Ferrara, without success though due to his premature death. Einaudi

supervised Lamberti's career, asking his ex-student Sraffa, who was by now well-established in the English academic environment, if Lamberti could not be 'considered mature (he is a graduate) and admitted to some higher institutional body', instead of being regarded 'like a simple undergraduate'? Although not hiding the difficulties of the situation, Sraffa was well-disposed, soliciting Einaudi for information.[23] Despite the silence of Lamberti himself, Sraffa persevered and obtained Einaudi's objective. In February 1932 Sraffa announced that Lamberti 'had been accepted definitely for next autumn'.[24] Lamberti arrived in England at the end of May and met Sraffa; and later he would admit that Piero 'had been of great courtesy and help to me' in an environment of 'great difficulties'. Nevertheless Lamberti, who could not stop thanking Professor Einaudi, admitted: 'Certainly I could not find a better study environment.' Most likely, though rather more problematic, it seems to have been a similar process of acclimatisation to that which Sraffa underwent some years earlier.[25]

Sraffa would be amongst the first readers of one of the few studies by Lamberti, the 'little work' dedicated to Ricardo which, thanks to Sraffa's own encouragement, Lamberti would send to *Rivista di Storia Economica*. Sraffa's judgement ('It is simply magnificent. I have never read anything equal to this for a long time.') was enough to convince the author to summarise the work for their common professor. Lamberti commented:

> Sraffa does not give praise easily, and I do not really know what to think. What do you think? That Sraffa is fooling himself?
>
> In any case, Sraffa's letter (I enclose two detailed criticisms: a quotation perhaps badly translated and a correction in the last sentence) gave me courage, and I reconsidered the reviews.
>
> (M.Lamberti to L.Einaudi, 12 May 1937, in AFLE-FE)

The friendship between Piero and Einaudi was of course preceded and then accompanied by the one between Einaudi and Sraffa senior. A friendship which began before Angelo Sraffa became rector of the Bocconi University in Milan, and developed in the years he was editor-in-chief of *Rivista del Diritto Commerciale*. It was precisely when Angelo Sraffa was rector that Einaudi began his courses at the Bocconi University, where both Giuseppe Prato and, on Einaudi's explicit recommendation, Gaetano Mosca would also teach. Later, Angelo joined the Italian free-exchange group, begun in 1932 on the initiative of Luigi Einaudi and Edoardo Giretti and Jannaccone. Piero Sraffa's words remain significant when, after the death of his father, he wrote to Einaudi thanking him for his letter of condolence:

> My father conserved the profound friendship he had with you, which years of separation did not weaken and which was only increased by your

kindness towards me and by the devoted and grateful affection he saw I feel for you.

(P.Sraffa to L.Einaudi, 20 December 1937, in AFLE-FE)

Another noteworthy friendship was that between Angelo Sraffa and Giuseppe Prato which grew also thanks to their co-operation at the Bocconi University. This went far beyond the boundary of a professional relationship, and when Piero suffered an acute attack of appendicitis which required an emergency operation, his father wrote emotionally to Prato about the 'days of anxiousness' spent at the bedside of his son. Piero also received the attention of his father's friend at that time, and once the worst was over Angelo wrote to Prato: 'My son wishes me to express his gratitude for the kindness you showed towards him.' Piero's own relationship with Prato began only after his graduation when he started work as an employee of the *Ufficio del Lavoro,* for which Prato's correspondence–according to Piero Sraffa's own affirmations–proved helpful. Writing from Milan in reply to a letter, Sraffa said significantly: 'Your words encourage me in my work and I hope that, going on, I will be able to show myself as not unworthy of your affectionate interest.' It would seem even more that Prato's encouragement was decisive in pushing Sraffa to Perugia.[26] The two men were already linked by the esteem they both felt for Maffeo Pantaleoni, whose death was communicated to Sraffa by Keynes himself. On hearing the news, the young Italian wrote an obituary, confessing to Prato that the words he had thrown down came from his heart, 'in the sadness of such a loss'.[27] In ensuing years, Prato would continue to follow the activity of the young scholar, and similarly Piero, although from a distance, willingly cultivated such an personal friendship.[28] At Prato's death, in the telegram sent together with his father Angelo to Prato's widow on 20 August 1929, Sraffa would speak of the 'terrible misfortune'.

In identifying the cultural environment in which Piero Sraffa was educated, I have neglected to mention his high school; an error on my part which Sraffa himself might well have judged severely if we assume to be true the testimony of Luigi Pasinetti, who insisted that Sraffa's high school period counted more than the university, at least in creating his interest in social-economic problems. Although the same Pasinetti invites us generally not to exaggerate 'in paying too much attention to what Sraffa said'. Further research into Sraffa's high school years may well prove to be productive; I will give only a short account, underlining the most notable name among the high school teachers: Umberto Cosmo. An 'Italianist', but from a school of thought that was outside of, if not to say foreign to, historical method, Cosmo went back to the ideas of De Sanctis and Croce, although his collaboration with *Giornale Storico della Letteratura,* that is the organ of the historical school, should equally be noted. Cosmo was active in socialist politics, and a police report of July 1911 describes him thus:

> 'Cosmo Umberto…besides literature teacher at the *Liceo Gioberti* he is untenured professor of Italian literature at the University of Turin. He is highly regarded generally for his moral conduct and outstanding intellect.–He openly admits his socialist sympathies, and reformist tendencies, and makes propaganda among state employees in his position as president of the *Camera Federale Impiegati Civile*–He should in no way be considered a threat to public order.'
>
> (*Archivio Centrale dello Stato,* Rome, c.p.c. fasc. 8474)

As a teacher at the *Liceo Gioberti* Cosmo taught Angelo Tasca, who would stay in contact with Sraffa through collaboration and argument even after the latter had left Turin. Later, as a teacher at the *Liceo D'Azeglio,* where his pupils included Leone Ginzburg and Norberto Bobbio, Cosmo was accused in parliament of working against the government by Vittorio Cian–the intellectual leader of nationalism in Turin. As a result, he was suspended from teaching and his position at the university annulled. In June 1929 this reformist professor would suffer even further, at the hands of fascist law, for having 'willingly' signed the telegram sent by a group of students in support of Benedetto Croce, who had been insulted by Mussolini for his speech at the senate on the signing of the *Patti Lateranensi* and the *Concordato.* This cost him five years internment (Antonicelli 1961). Already during the war, after Caporetto, Cosmo, whose position was now more pro-Giolitti than socialist though still decisively anti-nationalist, aroused the anger of the same Cian. On the pages of *La Stampa* (of which Cosmo had been editor since 1917, soon becoming 'one of the most appreciated and respected writers' (Vittoria 1988:788)), Cosmo published a number of articles in spring 1918 in which he argued against Francesco Ruffini, who had compared Caporetto to Novara and attributed both defeats to internal opposition groups. Unprejudiced and without ideological intentions, Cosmo justly accused the High Italian Command; and this was enough to be accused of defeatism by the nationalists. Well-known by Gramsci, Cosmo having substituted Arturo Graf in the faculty of Italian literature in his last two years of university teaching (1911–13), Cosmo was defended in such circumstances by his ex-student, who later, however, would fall out with him. Gramsci was to take offence at Cosmo for referring to the militants of a 'socialism of swindlers' as 'hedonist students' (this was in November 1920, the month Sraffa graduated). Gramsci retaliated by first writing about the merits and admirable idealistic struggles of Cosmo then concluding with a harsh dismissal of his professor, friend and comrade now become adversary and ally of the enemy: 'We tell him frankly that all the esteem and affection of his socialist students has turned into enormous pity and profound contempt' (Gramsci 1987:760). The mature Gramsci–the one who via his sister-in-law, Tatiana Schucht, corresponded with Sraffa the Cambridge professor–would repent of such a bitter feud, and several years later the two men would be

reconciled and the same Gramsci would recall their moving embrace in the Italian embassy in Berlin where Cosmo was then working. Almost it seems in an attempt to seal their friendship, Gramsci wrote:

> Anyway, I hold of Cosmo a memory full of affection, and I would also say of veneration if this word did not bear a meaning unsuitable to my feelings: he was, and I believe he is still, a man of great sincerity and moral fortitude, with streaks of instinctive ingenuousness which are typical of great men of learning and intelligence.
>
> (Letter of 23 February 1931, in Gramsci 1965:410)

It was precisely this professor, a model as both a man and a scholar, who introduced Gramsci to his pupil from the *Liceo D'Azeglio,* Piero Sraffa, who in time would become the go-between, via Tatiana, for Gramsci and Cosmo; Sraffa provided each with news of the other, even on mutual subjects of interest such as Dante, about whom Gramsci particularly felt passionate when he was in prison (see Gramsci 1975). In the same year that Gramsci recalled his reconciliation with Cosmo, that is 1931, Sraffa wrote to Tatiana (who would in turn write to Gramsci) practically copying out his comments on the merits of a study of the intellectuals which Gramsci intended to abandon. Intended to abandon because in prison Gramsci was without adequate means to continue (books, documents, materials, etc.). But his friend reasoned:

> With regard to your studies, and the abandonment of the programme you had made: certainly to write a complete history of the intellectuals one would need to start with the Roman Empire and have at one's disposal a great library: but why not write an incomplete one for the moment, completing it when you will be free and have access to the libraries? Once upon a time it was Nino who berated me for the excess of scientific scruples which prevented me from writing anything: I have never recovered from this disease but is it possible that ten years of journalism have not cured him?
>
> (P.Sraffa to Tatiana, 23 August 1931, in Sraffa 1991a:23)

No, ten years of journalism had not cured Gramsci from the disease he had contracted in Turin: the seriousness of work, the severe revision, the meticulous research. Sraffa openly confessed to be incurable and at the end of the same letter, writing about some recent publication, he offered further explication:

> The philosophers believe that their work is done when they have proved that scientists are shameful failures in philosophy. Thus the natural sciences have remained in the hands of the positivists, with well-known consequences. At the present moment some scientists… seem to have left positivism for a species of gross mysticism.
>
> (Ibid: 24)[29]

Beyond the theoretical-ideological background (which is difficult to verify for both), the political sympathies (doubtful) and the personal affection (certain), Sraffa and Gramsci appear united here also by the common history of the University of Turin and its *cultura positiva,* which meant respect for science, attention to methodology and commitment to research. This remains, then, the first and perhaps most significant influence of Turin on the intellectual persona of that tenacious, rigorous and passionate scholar that was Piero Sraffa.

Notes

1 R.Faucci, 'Tra Einaudi e Gramsci: Sraffa nella cornice politico-culturale della Torino anni '20', a paper presented at the conference: 'Un intellettuale europeo del XX secolo: Piero Sraffa, 1898–1983', Turin, 28–29 May, 1984. In truth, the expression does not appear in the volume of the conference's papers (Bellofiore 1986). Biographical details can be found in Potier (1991) and in Pasinetti (1985a and b).
2 I have recently outlined this argument in various articles: see in particular d'Orsi 1997–8.
3 On Cognetti de' Martiis see Faucci (1982) and Pogliano (1993).
4 For the university studies of Piero Sraffa, I have relied on the documents in *Archivio Storico dell'Università di Torino* (hereafter *ASUT*): IX A 432 (register of university studies); X C (verbal exams); and X C 98 (law degree exams).
5 With regard to this see the letters from Einaudi to Loria of 20 September 1901 and 16 April 1902, in AST-CL.
6 See letters from Loria to Einaudi in Archivio Fondazione Luigi Einaudi di Torino, Fondo Luigi Einaudi, hereafter AFLE-FE
7 Cf. the letters from Piero Sraffa to Einaudi, 1945 (in AFLE-FE, 'Sraffa, Piero'), and from Einaudi to Piero Sraffa, 18 October 1945 and 10 May 1946 (the original copies are held in the Sraffa Papers at Trinity College, Cambridge). The article in question is Einaudi (1946).
8 A.Cabiati to L.Einaudi, 17 May 1920, in AFLE-FE, 'Cabiati, A'; the letter is published in Pino Pongolini (1995).
9 See note 8.
10 Angelo Sraffa wrote to Loria (21 September 1924): 'My son Piero wrote to me from London asking if you would send him two letters of introduction to Bonar and Higgs. Would you be so kind as to send them to me here then I may send them on to him in London? I would be very grateful' (AST-CL, XVIII, b. 5).
11 Cf. A.Sraffa to A.Loria, 9 and 18 March 1925, and 7 April 1926 in AST-CL, XVIII, b. 5, f. 1.
12 Cfr. A.Cabiati to A.Loria, 18 July 1908, in AST-CL, IV, b. 11, f. 1.
13 See letter from Jannaccone to Loria, 12 June 1907, in AST-CL. It is not unreasonable to imagine that Jannaccone was most grateful to Loria for his assistance in a state exam at the beginning of the century: see letter from Jannaccone to Loria, 7 October 1907, in AST-CL.
14 See letter from G.Prato to A.Loria, 22 April 1920, in AST-CL.
15 See letter from G.Mosca to G.Prato, 28 December 1917, in AFLE-Fondo Prato.
16 See letter from A.Sraffa to G.Prato, 30 March 1914, in AFLE-Fondo Prato.
17 See letter from P.Sraffa to G.Prato, 16 June 1922, in AFLE-Fondo Prato.
18 See letter from A.Cabiati to L.Einaudi, 5 April 1921, in AFLE-FE.
19 See letter from P.Sraffa to L.Einaudi, 2 October 1958, in AFLE-FE.

20 This is based on various letters and postcards sent from Piero Sraffa to Einaudi between 1921 and 1925 (all in AFLE-FE).
21 See letter from Gramsci to Tatiana, 7 September 1931, in Gramsci (1965).
22 See letters from P.Sraffa to Einaudi of 18 August and 6 September 1951, in AFLE-FE; Einaudi (1951) and (1953).
23 See the letters from, respectively, L.Einaudi to P.Sraffa, 23 November 1931, in *Sraffa Papers* and P.Sraffa to L.Einaudi, 28 January 1932, in AFLE-FE.
24 See letter from P.Sraffa to L.Einaudi, 7 February 1932, in AFLE-FE.
25 See letters from M.Lamberti to L.Einaudi, 7 June and 12 October 1932, and 1 January 1933, in AFLE-FE.
26 See letter from P.Sraffa to G.Prato, 10 January 1925, in AFLE-Fondo Prato.
27 See letter from P.Sraffa to G.Prato, 10 January 1925, in AFLE-Fondo Prato.
28 See for example the letter from Angelo Sraffa to G.Prato, 17 June 1926, which expresses regards to Prato on Piero's behalf (in AFLE-Fondo Prato).
29 On the relationship between P.Sraffa and Gramsci, beside Potier (1991), see Fausti 1998, Sraffa (1991a) and Spriano (1977).

2 Piero Sraffa's early approach to political economy

From the *gymnasium* to the beginning of his academic career

Nerio Naldi[1]

From the *Gymnasium Giuseppe Parini* to the University of Turin

We know almost nothing of Sraffa's earliest contacts with economic science or with economic themes but we may guess that they date back to the period between 1911 and 1913, when, according to what he related to Alessandro Roncaglia, Domenico Re–his professor of Latin and Greek at the *Ginnasio Giuseppe Parini* in Milan–led him towards socialist ideals (Roncaglia 1980:171). Of course, also within his own family Sraffa could have had such an early approach: his father, Angelo Sraffa, a professor of commercial law, was certainly sensitive to the importance of the sphere of economic activity and of economic science;[2] he was a good friend of important economists, Rector of the Bocconi University, and particularly active in the foundation of the *Istituto di economia e scienze sociali* of that university.[3] Most probably, however, it was in Turin, between 1912 and 1916, with his schoolmates of the *Liceo Massimo D'Azeglio,* that Piero Sraffa approached economic themes and Marxist issues in particular somewhat more deeply.[4] On this point we may refer to two testimonies. First, Paolo Vita-Finzi recalled the long discussions with Sraffa and other schoolmates and friends in Turin implying that many of them were oriented towards Marxist positions (never shared by Paolo Vita-Finzi) and came to support Soviet Russia (Vita-Finzi 1989:27, 318, 324–5). Second, Geoffrey Harcourt (who reported to the author of this chapter that the source of the information was Krishna Bharadwaj and that he is fairly certain that the events she related to him took place when Sraffa was not yet at the university but still at school) wrote:

> Many of [Sraffa's] student friends were Marxists but his teachers would not allow Marx or Marxist issues to be discussed explicitly in class. However, as a student, Sraffa read Ricardo's *Principles* and discovered that much of what Ricardo had to say bore a close resemblance to what he had been reading in Marx's work. As Ricardo was eminently respectable and

> so acceptable to their teachers, Sraffa and his fellow students took to discussing Marxist issues under the guise of a study of Ricardo.
>
> (Harcourt 1983:118)

These approaches to economic themes may be seen as a young man's attempts to explore the world, but they certainly were not unimportant to Sraffa, if, as related by Gaia Servadio (1993:71), he had decided to matriculate in Turin at the Faculty of Law because in Italy at that time a Faculty of Economics did not exist and that Faculty of Law could have been the best place to study economics; a course of studies completely devoted to economic studies was offered only by a number of *Scuole Superiori* and by the Università Commerciale Luigi Bocconi (of which Angelo Sraffa was the Rector).[5]

Student and soldier

Sraffa's own position on the First World War is described by Sraffa himself as *pacifist socialism* in a letter that he sent to Antonio Gramsci in 1924 (Gramsci 1971:175–81; Gramsci 1978:229–36; Naldi 2000). However, notwithstanding this position, from 1917 he served in the army.

A few years later, just after the war, Sraffa might have had a special opportunity to study themes in industrial economics, money and banking and government control. This opportunity might have arisen when, between November 1918 and March 1920, he was assigned to serve as a member of the secretariat of the *Reale commissions d'inchiesta sulle violazioni del diritto delle genti commesse dal nemico* (Royal commission of inquest on the violations of the law of nations committed by the enemy), which was based in Rome (*SP* A1/2/8). The sixth volume of the commission's report indicates that between mid-January and early February 1919, Sraffa had been travelling with higher members of the commission in the provinces of Venice and Trieste, collecting witnesses' reports on acts of brutality perpetrated by the German and Austrian armies.[6] However, the seven-volume report also includes a chapter on *La Cassa Veneta dei Prestiti* (a bank which is referred to also in Sraffa's *tesi di laurea*–Sraffa 1993:9–set up by the Austrian government in the territory occupied after the 1917 *Caporetto* retreat of the Italian army), a chapter on compulsory subscriptions of Austrian bonds, several chapters on the damage caused by the Austrian army to the economies of the areas which had been under its control, and an appendix on the economic regulations issued by the Austrian army. We have no proof that Sraffa was involved in the preparation of these chapters, but we may note that by the time the commission started its work he had already passed the exams of *Economia politica* and *Statistica* (ASUT IX.A. 432)[7] and that he seems to have been less often employed in collecting the kind of reports mentioned above than other junior officers attached to the secretariat of the commission.

If the work for the above-mentioned commission may have provided the young Sraffa with a very special opportunity to approach economic questions, he certainly had a more typical approach to economics through his student work at the university. As is well known, Piero Sraffa graduated from the University of Turin with a degree in Law on 29 November 1920 with a thesis on *L'inflazione monetaria in Italia durante e dopo la guerra* (Sraffa 1993).[8] The supervisor *(relatore)* of this thesis was Luigi Einaudi, who, after the fall of fascism and the end of Second World War, was to become governor of the Bank of Italy, Minister of the Budget and, later, President of the Italian Republic. However, the text of a letter sent to Einaudi by Attilio Cabiati,[9] the content of the notes in Einaudi's handwriting that we can find in his own copy of the thesis (kept in AFLE-FE), and Luigi Pasinetti's testimony all suggest that Einaudi's actual supervision was confined to a reading of the final text of the thesis. In fact, according to Pasinetti, Sraffa managed to discuss his thesis with Einaudi only when it was already completed; and the relevant passage of the above-mentioned letter (first quoted in Pino Pongolini 1995) sent by Cabiati to Einaudi reads:

> I suggested to Sraffa's son that for his thesis he study the problem of currency revaluation and whether reconversion to the gold standard would be advisable for continental Europe. The youth, who is bright, liked the theme very much, but was annoyed at the idea of developing it with Loria: would you mind if he discussed it with you? And, in order to do so, must he do anything in particular? The younger Sraffa is now in Turin, if you want to contact him, write to me and I will send him to you.
>
> (AFLE-FE 17.05.1920)[10]

The text of another letter from Cabiati to Einaudi suggests that in 1920–1 Piero Sraffa's relationship with Cabiati was closer than with Einaudi. In fact, in the margin of a letter dated 5 April 1921, Cabiati asked Einaudi: 'Sraffa's son, Dr. Piero, is going to London and would appreciate a letter of introduction to any of the great economists. If you could produce one for him, his father, too, would be most grateful' (AFLE-FE).[11] Hence it does not seem unlikely that at least some early steps of Sraffa's research for his thesis had been guided by Cabiati himself.[12] Piero Sraffa's relationship with Einaudi probably became closer during the period that he spent in London from April 1921 to June 1922, when Sraffa searched for old books and journals for him (AFLE letters from Piero Sraffa to Luigi Einaudi). Einaudi's esteem for Sraffa however certainly originated with the discussion of his thesis. In fact, in a short note written after the death of one of his former students, Einaudi wrote: 'Along with those of a few others (Cesare Jarach, Gino Borgatta, Piero Sraffa, Aldo Mautino), [Mauro Fasiani's thesis] was the sudden revelation of remarkable theoretical aptitudes' (Einaudi 1950:199). According to Pasinetti

(1985b:319), Sraffa believed that after having read and discussed his thesis Einaudi shifted his support from a policy aimed at improving the exchange rate of the Italian lira through a reduction of the quantity of banknotes in the country (even though he was well aware of the risk of economic crisis that such a policy implied) to one which–in line with what Sraffa had been arguing in his thesis–gave more importance to the stabilisation of the exchange rate, and, in particular, of internal prices independently of their level, pursuing that objective by reducing to zero the issue of banknotes. Sraffa's opinion is supported by a reading of Einaudi's articles published in the newspaper *Corriere della Sera* during the years 1919–21.[13]

April 1921–June 1922

Before graduating Piero Sraffa completed his training as a lawyer in Turin (*SP* A1/3/1–2); then, according to what he told several Italian students and professors, he spent another period of training in a bank in a small town near Milan.[14] Shortly afterwards, in April 1921, he moved to England (*SP* A1/4) where he enrolled as a research student at the LSE.[15] This was probably the first time he had visited England, and he remained there until 3 June 1922 (AFLE letter from Piero Sraffa to Giuseppe Prato 02.06.1922).

This period can be seen as very important to his formation as an economist; but two episodes alert us to his already exceptional qualities: his first meeting with J.M.Keynes and his relationship with H.S.Foxwell. Of the first meeting with Keynes, which probably took place in Cambridge in August 1921, we are informed through testimonies originating from various sources. According to Pierangelo Garegnani, Luigi Pasinetti and Fabio Ranchetti on that occasion Keynes was particularly impressed by Sraffa's considerations on the forward markets for currency (Ingrao and Ranchetti 1996:520), i.e. by what was to become the essential framework of the analysis based upon the concept of *commodity rate of interest* that he later developed in his 1932 review of Hayek's *Prices and Production,*[16] which appeared in Keynes' *General Theory* as *own rate of interest.* However, an analysis of the writings published by Keynes in the years immediately before and after his first meeting with Sraffa has also suggested that on that occasion or approximately during the period that Sraffa spent in London they might have discussed the arguments developed by Sraffa in his undergraduate thesis *(tesi di laurea)* on the reasons why domestic monetary stabilisation should be preferred to revaluation of the exchange rate of the national currency and should be distinguished from exchange rate stabilisation. Hence, the development of Keynes' views on those themes might have been influenced by Sraffa's arguments (Roncaglia 1984:107–8; Roncaglia 1994:2; Ginzburg 1986:62–7).

Of the relationship between Sraffa and Foxwell[17] we are informed from a letter written by Foxwell in October 1923 in order to support Sraffa's

application for a lectureship in Genoa at the *Scuola Superiore di Commercio*.[18] It may be worth reproducing the whole text of this letter:

> Cambridge, Oct. 1 1923
> My friend Dr. Piero Sraffa tells me that he is proposing to apply for a post as Assistant in Economics at the School of Commerce in Genoa; and it is with equal pleasure and confidence that I write a few words in support of his application.
>
> Dr. Piero Sraffa first became known to me in the Session 1921–1922, when he was a student at the London School of Economics. He attended all my higher courses on Currency and Banking there, including a course on the history of these subjects; and he also attended a Seminar, which enabled me to get more intimate personal relation with him and his powers of work. From the first I formed a very high opinion of him. His judgement and acuteness were very marked; he had a wide acquaintance with the academic literature of economics; to this he added, what I venture to think even more important, a keen interest in the practice of business and finance, and a close observation of the markets and the course of affairs from day to day.
>
> When in Milan,[19] last spring, I had many opportunities of conversation with him on the latest developments of business and banking; and the favourable impression he had left on me in England was distintinctly strengthened. I consider him one of the ablest students I have had of recent years, and one of the keenest and most enthusiastic. I cannot doubt that he could prove an inspiring teacher; and his personal qualities are such as could make him a very agreeable collegue. So far as I am concerned, our relations have been extremely pleasant.
>
> I did what I could in London to give Dr. Sraffa some insight into English banking practice and into the peculiar atmosphere of the London market: but he is so fully informed as to Italian practice and methods, and so able and willing to impart his knowledge to others, that it is only bare justice to say that I have learnt more from him than he can have learnt from me. I do not think it will be easy to find a candidate who combines in the same degree as Dr. Sraffa practical knowledge and sound judgement with theoretical insight and acuteness: a combination most valuable for such a post as the one he seeks.
>
> (*SP* B5/1)

If we consider the period that Sraffa spent in London, however, we may see that his interests were not directed only towards monetary economics. He certainly attended other courses besides Foxwell's; but what is more important is that he also engaged himself in other activities: in particular, he wrote three articles on the British and American working classes for Antonio Gramsci's newspaper *L'Ordine Nuovo*[20] and worked as a researcher at the *Labour Research Department:* an important research body founded in 1913 as

Fabian Research Department, which studied labour, industry and agriculture problems with a strong focus on forms of economic organisation which might have offered an alternative to pure capitalism and became a trade union research body.[21] Sraffa's activity at that institute seems to have been substantial and is documented by a formal statement by the director of its international section, Rajani Palme Dutt (a member of the Communist Party of Great Britain since its foundation in 1921):

> 6th January, 1922
> During the past year Mr. Piero Sraffa has been assisting in the work of the Labour Research Department as well as conducting investigation of his own into labour problems of this country.
>
> His technical knowledge of labour organization and conditions has been of very great value to the Department and his own investigations have been marked by a real insight and grasp in comprehending the complex situation in this country.
>
> (*SP* B4/18)[22]

If we examine the motivations which might have led Sraffa to undertake the different activities in which he was involved in London, we may guess that at that time his main interests were directed towards politics, labour movement and labour economics, money and banking and political economy in general. When he went back to Italy he certainly did not abandon monetary economics and political economy; in fact, in June 1922 and December 1922 he published in the *Economic Journal* and in the *Reconstruction in Europe* supplement to the *Manchester Guardian Commercial* two articles on the crisis of the Italian banking system. But the nature of the job that he took up in Milan–his first appointment as far as we know–suggests that his interest in politics and labour issues was particularly strong. In fact, in June 1922, when he returned to Italy he took up the post of director of the labour office of the Province of Milan.[23] Moreover, a manuscript fragment in Gramsci's hand which was part of a document prepared in March 1923 in Moscow by Antonio Gramsci and Egidio Gennari and probably sent to members of the Italian Communist Party based in Italy contains the suggestion that Piero Sraffa could have been proposed for the task of setting up a centre for economic research and a bulletin on economic and labour problems based in Italy and covertly controlled by the Italian Communist Party and that he considered that project with interest:[24]

> We believe that it would be worthwhile to create an office devoted to economic research which would work for the party and gather all the elements necessary for its struggle and for its intellectual preparation. This office could be legal, run by elements controlled by the party, who do not necessarily have to be party members. Its purpose could be: to compile a

> monthly or biweekly bulletin discussing the national and international situation of the working classes (unemployment, salaries, union struggles, organization) in the face of capitalist organization. It would be a smaller version of the English Labour Party's labour research section.... We would like to suggest two candidates for this assignment: Piero Sraffa, an acquaintance of Togliatti's, who in England worked for the Labour Party's Labour Research Office and is a specialist in banking matters. Gramsci could write him a letter. Some time ago Sraffa favourably discussed just such a project with Gramsci. He has worked indirectly in Turin, and has given L'Ordine Nuovo a great deal of material on confidential subjects, dipping into the files of his father, who is a bigwig in the Masons and the Banca Commerciale, and his Communist opinions are known only by a small circle of acquaintances. The other element could be Molinari, the one that worked with Nicolini in 1920 and who until recently was employed at the labour office of Municipality of Milan. He was a communist sympathizer, whatever his anarchist origins in '21?? he began contributing to «L'Ordine Nuovo».
>
> (Gramsci 1992:114–16)[25]

According to what we can read in this fragment, Gramsci, having been in touch with Sraffa some time before March 1923 (but we do not know exactly how long before), had already discussed the project with him, and Sraffa considered it with interest. Again, according to the content of the fragment. Antonio Gramsci himself could have got in touch with Piero Sraffa shortly after March 1923 in order to take a further step towards the definition of the project. A typewritten paper on the economic policy of the Italian (fascist) government written in Italian, dated in Sraffa's hand *Aprile 1923* and kept among the *Sraffa Papers* (SP D3/3) might have been written in relation to Gramsci's request to set up such a centre for economic research.

If in summer 1922 Sraffa's professional activity was quite strongly oriented towards labour problems and money and banking, the subsequent train of events led him towards political economy and pure economic theory: the fields among those listed above which up to that time Sraffa seems to have been pursuing relatively less actively. How this happened seems to have been deeply entwined with the events which characterised Italian political life in 1922 and 1923, the period which saw the success of the fascist assault on Italian political and social institutions. In the next part of this chapter I will consider how these events seem to have influenced Sraffa's private and professional life.

Director of the *Ufficio Provinciale del Lavoro* in Milan

Piero Sraffa was appointed by the Province of Milan director of the local labour office on the 26 April 1922 and, as we already know, he took up the post in

June 1922. At the moment, the only document known to us illustrating the nature of the work done by Sraffa at the *Ufficio Provinciale del Lavoro* is a copy of a programme for the activity of the office dated 21 October 1922 that he–being its director–was to present to the *Consiglio provinciale* (*SP* B4/1–7; B4/8–14). However, as far as we can see from the minutes of the *Consiglio provinciale,* the programme was never officially presented; on the contrary, according to what Sraffa stated in a letter to Keynes dated 13 January 1923, he was forced to resign from his position at the beginning of December 1922 (*KP* L/S/9).[26] We do not know with certainty what prompted this resignation and if Sraffa had been subject to direct attack or menace. On the grounds provided by the available information we may speculate that it was influenced by the general pressure exerted by the fascists after Mussolini was designated Prime Minister and by other events more specific to the administration of the Province of Milan which we can reconstruct through the minutes of the *Consiglio provinciale* and other documents. From November 1920 to November 1922 the *Consiglio Provinciale* of Milan was for the first time led by a socialist majority. During that period the president of the *Deputazione Provinciale* (the executive commitee of the *Consiglio Provinciale*) was Nino Levi: a socialist lawyer and university professor born in 1894 who had been president of the commission which selected Sraffa for the post of director of the *Ufficio Provinciale del Lavoro* and who was called to join the staff of the University of Cagliari when Piero Sraffa was there; with Raffaele Mattioli he was one of Sraffa's closest friends. On 30 November 1922 Levi, together with the whole *Deputazione,* resigned his position because the *Consiglio Provinciale* had not approved the budget for the year 1921;[27] and on 2 December the whole socialist group resigned from that council.[28] On that very day, according to what we have seen in the *registro di protocollo* of the *Provincia di Milano* for the year 1922, Sraffa sent the letter with which he resigned his position. If we bear in mind the political climate of the period (anti-fascist individuals and organisations suffered daily physical assaults; the Mussolini government had been elected at the end of October and political institutions were being occupied by fascists), the sequence of the events more strictly connected to the life of the *Consiglio Provinciale* of Milan and the fact that the labour and statistics office of the *Comune di Milano* had just been closed,[29] we may guess that Sraffa's decision reflected his awareness that the possibility of doing serious work at the labour office was going to be severly restricted, but also the fact that he might have felt himself particularly closely connected with the socialist administration (Sraffa had been elected to his office with thirty-one votes–approximately the same number which supported that administration). However, according to what Sraffa himself related to Pierangelo Garegnani, as director of the labour office, he was also engaged in coordinating the efforts of co-operatives and of other workers' organisations to defend themselves from fascists' assaults; in this sense Sraffa's resignation from that office would have been a direct effect of that of the socialist administration.

On 7 December 1922, less than a week after his resignation from the labour office, the eleventh issue of the *Reconstruction in Europe* supplement to the *Manchester Guardian Commercial,* which was edited by Keynes, published Sraffa's second article on the Italian banking system. This article was to provoke a hostile and menacing reaction by Mussolini who, on 20 and 21 December, sent two telegrams to Angelo Sraffa warning him that he was to demand that his son answer for the article and see to it that he published a retraction.[30] The text of those telegrams was known only through their English translation reproduced by Sraffa in a letter to Keynes dated *Milan Christmas 1922* and posted in Switzerland to avoid police censorship (*KP* L/S/5–6); but we have now been able to locate the imprint of Mussolini's handwritten telegram left on a paper containing a short biographical note on Piero Sraffa probably prepared for Mussolini himself, Angelo Sraffa's answer to it, and Mussolini's second telegram (for the texts see Naldi 1998b, 1998c).[31] These documents, together with a copy of the relevant issue of the *Reconstruction in Europe* supplement are preserved in the archives of the Italian *Ministero degli Affari Esteri* in a folder which bears the title *Sraffa Piero Articolo contro le banche italiane* (Article against Italian banks) and this, in turn, is kept in the larger folder *Calunnie contro l'Italia* (Slanders against Italy) together with newspapers clippings and other reports on Italian credit and finance and on economic stability and social conflicts in Italy relating mainly to the years 1919–21. Mussolini's strong irritation could have been determined by the fact that Sraffa's article contained an extremely clear and perceptive analysis of the critical state of the three largest Italian commercial banks and by the fact that it was published when, as Prime Minister, he was already involved in the attempt to avoid a serious bank crisis by rescuing the *Banco di Roma,* the bank whose situation was the most critical. In the end Mussolini does not seem to have carried out his threats. Most probably he decided to follow this course of action because the general public and the markets did not seem to have noticed or to have attached any importance to Sraffa's article. It must also be recorded, however, that Grazia Servadio relates that Piero Sraffa told her that on that occasion his family managed to get in touch with Mussolini through the high magistrate Mariano D'Amelio, Piero's uncle (Servadio 1993:71).[32]

Sanctuary in France and academic career in Italy

Shortly after the exchange of correspondence between Mussolini and Angelo Sraffa, on 8 January 1923, Giuseppe Toeplitz, the *amministratore delegate* of the *Banca Commerciale Italiana,* another bank whose position had been discussed in Sraffa's article, asked Piero Sraffa to meet with him (ASBCI CPT vol. 24, f. 416). Sraffa surmised that Toeplitz's initiative had been influenced by Mussolini (*KP* L/S/9); but documents in the archives of the *Banca Commerciale Italiana* suggest that this was not the case and that the relatively long delay with which that initiative followed the publication of the article was

accidental (ASBCI CPT vol. 24, f. 431). After having met both Angelo and Piero Sraffa, Toeplitz, on 9 January, decided that 'the only possible measure [against Piero Sraffa is] sending a formal denial directly to Keynes, with the request that it be published in the *Manchester Guardian Commercial*' (ASBCI CPT vol. 24, f. 431–2; see also f. 223);[33] but on 13 January 1923 Sraffa was not yet aware of Toeplitz's decision and, as an answer to the invitation to move to Britain that he had received from Keynes, who certainly was alarmed by what Sraffa had reported to him, he could only write: 'I cannot leave this country at present, owing to the possibility, for which I have received a hint, of legal action being taken by the bank' (KP L/S/9). On 22 January, however, he sent another letter to Keynes, from Lugano, announcing that '[since] it seems that the *Banca Commerciale,* after having menaced, is unable to set on foot a law-suit, as there is no ground for it…, there is no more the reason which retained me in Milan. I accept your kind proposal and shall leave shortly for London' (*KP* L/S/10–11). But on 26 January 1923 he was refused permission to land in Dover. It is generally believed that this was the result of an official request by the Italian government; but Sraffa's relationships with Italian and British Marxists during his first stay in Britain might have been known also to the British Home Office, which issued the order (*SP* A1/5).[34] Stamps on Sraffa's passport suggest that, having been refused entry in Britain, the news from Italy of the large number of arrests among members of the Communist Party and of the socialist faction closer to the communists (government sources numbered the arrested at 2,000; communist sources at 5,000) induced Sraffa to remain in France at least until mid-March and that–like many other Italian anti-fascists–he did not find that country hostile.[35] The arrests continued also into April and May; but many of those arrested were soon released and in the end none was convicted: probably because the repression had been conducted in clear violation of Italian law, which had not yet been modified by the fascist regime (Spriano 1967:260–3).

Under these circumstances, the perception that Italian universities could still offer relative freedom to academics might have prompted Sraffa to decide to pursue a career as professor of economics. This decision might have been taken by Sraffa after he was refused permission to land in Britain and might have prompted him to shift the focus of his attention from labour and monetary economics[36] towards political economy in general and its theoretical foundations in particular. In fact, a notebook preserved among the *Sraffa Papers* bearing the date *Aprile 1923* which mainly contains critical notes on the text of Marshall's *Principles* (*SP* D1/2)[37] shows that in that period he was involved in a careful reading of that book. When, in November 1923, in Perugia, Sraffa started his career as a university professor he used Marshall's *Principles* as textbook for his course of *Economia politica.*

The decision to use Marshall's *Principles* as a textbook for the course in *Economia politica* shows that even as a teacher Sraffa wished to directly tackle one

of the highest expressions of marginalist economics. Similarly, it can be argued that knowing that Sraffa used Marshall's *Principles* as a textbook shows that his analysis of marginalist economic theory was guided by its Italian interpretations or elaborations–in particular those developed by Pantaleoni and Barone–to a lesser extent than we could be led to believe considering the number of references contained in his 1925 article (even if such references are fewer than those to Anglo-saxon sources). However, that we should not underate Sraffa's attention to the works of Pantaleoni and, in general, of Italian economists is confirmed by the fact that the course on *Economia politica* contains a lecture on *Critica della teoria delle proporzioni definite:* a theory which had been long discussed by Italian economists and which was introduced with great emphasis in Pantaleoni's *Principii di economia pura*. From the point of view of establishing a relationship between the article published by Sraffa in November 1925, the two lectures on *La tendenza alla produttività decrescente nell'agricoltura* and *Industrie che presentano una tendenza alla produttività crescente* can be of some interest. In these lectures Sraffa might have presented to his students some of his arguments on the shape of the cost curves in agriculture and on the difficulties which arise when we try to draw an upward sloping cost curve for an individual firm. Moreover, the fact that these lectures followed closely those on the division of labour while those on cost of production and those on Marshallian variable costs were given more than a month later suggests that he introduced his students to the distinction between classical and later approaches to the theory of costs that we can find in his 1925 article.[38]

The importance of the relationship with Antonio Gramsci: a conjecture

Concluding this chapter we would like to touch upon the question of the importance of the relationship between Piero Sraffa and Antonio Gramsci to the development of Sraffa's own approach to political economy.[39] Piero Sraffa probably first met Antonio Gramsci between February and September 1919, and their friendship developed quite quickly (Vita-Finzi 1989:136). Many years later Sraffa recalled that his discussions with Gramsci touched upon many subjects;[40] and it is a rather obvious guess if we presume that they also touched upon political and economic themes. But when we come to the specific question of Gramsci's influence on the development of Sraffa's approach to political economy an assessment becomes much more difficult as the information available mainly rests upon the testimonies of two close friends of Sraffa who, after his death, recalled that his decision to concentrate his research interests on classical political economy had been influenced by Antonio Gramsci. Nicholas Kaldor wrote:

> It was partly due to Gramsci's influence which led him away from his early concentration on problems of money and banking to an interest in

> the issues raised by the classical theory of value in the version developed by Ricardo, and to discover new methods for overcoming the problems which Ricardo himself left unresolved.
>
> (Kaldor 1984:149)

And Krishna Bharadwaj:

> It is believed that it was at Gramsci's suggestion that Sraffa turned to classical political economy as his abiding interest (particularly Ricardo) from his previous preoccupation with monetary affairs.
>
> (Bharadwaj 1984:300)

The most obvious documents known to us do not provide any direct support to this thesis;[41] but if we want to give credence to these testimonies, we must locate this influence between 1919 and November 1926, when Gramsci was arrested, and consider them in the context of the development of Sraffa's thought and activity. The period that we have indicated may be divided into four shorter periods: the period between 1919–20 and April 1921, when Gramsci was living in Turin and Sraffa, being demobilised in the early months of 1920, was living first in Turin and later in Milan; the period between April 1921 and May 1922, when Gramsci was in Italy and Sraffa in Britain, probably returning to Italy during vacations; the period between June 1922 and April 1924, when Sraffa was in Italy and Gramsci first in he USSR and then, from December 1923, in Austria; the period between May 1924 and November 1926, when they met quite frequently, probably in Rome and Milan. To attain some insight, however, into the meaning of the two testimonies we must also observe that Sraffa's preoccupation with monetary affairs never ceased but around the year 1922 it probably lost relative weight within his activities; and that in 1925 the critique of Marshallian theory emerged as a crucial element of the research project he had started (as his manuscripts show) at least as early as Spring 1923. Furthermore, we may recall that, in London in 1921–2, he probably attended and was much impressed by Cannan's lectures on the history of political economy (Pasinetti 1985b:319); and that his already mentioned intention to meet James Bonar and Henry Higgs, in September 1924, may be seen as a sign of an already strong interest in classical political economy: a fact which is confirmed by some statements contained in his December 1925 article.[42] On the basis of these presuppositions we would concentrate our conjecture on the period ending with 1922, or at most with 1923; and we would interpret Kaldor's and Bharadwaj's statements as implying that, if discussions with Gramsci influenced the development of Sraffa's economic thought, this influence might have contributed to leading Sraffa to formulate a distinction between objective and subjective approaches to political economy, and to consider in this light the difference between classical and marginalist approaches.[43] Certainly the manuscripts grouped by Sraffa

himself as 'Notes up to 1927' and other manuscripts dating to 1927 suggest that the distinction between objective and subjective approaches to political economy might have moulded Sraffa's own critique of mainstream economic theory, his reading of classical economists and, in general, his approach to political economy; and Gramsci's remarks on Pantaleoni's *Principles* describe the part of the book where the *hedonistic principle* is discussed as more appropriate to a cookery book or to the Kama Sutra than to political economy (see Gramsci 1975:1268).[44]

Notes

1 I wish to thank Giandomenica Becchio, Livia Beux, Antonietta Campus, Elio Cerrito, Giancarlo De Vivo, Angelo D'Orsi, Pierangelo Garegnani, Paolo Garofalo, Alfredo Gigliobianco, Giorgio Gilibert, Geoffrey Harcourt, Grazia Marcialis, Maria Cristina Marcuzzo, Ettore Molinari, Manuela Mosca, Luigi Pasinetti, Mimma Paulescu Quercioli, Francesca Pino Pongolini, Giovanni Ramella, Fabio Ranchetti, Maria Pia Re, Marzio Achille Romani, Alessandro Roncaglia, Roger Simon, Jonathan Smith and Sergio Steve for the various kinds of assistance and advice. I also wish to thank the following institutions and their staff (brackets indicate abbreviations used in the text): Archivio Provincia di Milano, Milano; Archivio Storico Banca Commerciale Italiana, Milano (ASBCI); Archivio Storico Diplomatico, Ministero degli Affari Esteri, Roma; Archivio Storico Università degli Studi di Torino, Torino (ASUT); Biblioteca Isimbardi, Milano; British Library of Political and Economic Science, Archives Division, London; Centro studi e documentazione Piero Sraffa, Roma (CS); Fondazione Luigi Einaudi, Torino (AFLE); Modern Archive Centre, King's College, Cambridge; Ufficio Ricerche Storiche, Banca d'Italia, Roma; Wren Library, Trinity College, Cambridge. *SP* and *KP* refer, respectively to *Sraffa Papers* and *Keynes Papers*. (The Sraffa Papers are located in the Wren Library, Trinity College, Cambridge, and the Keynes Papers in the Modern Archive Center, King's College, Cambridge.) Financial support from MURST and from Facoltà di Scienze Statistiche (Roma 1) is gratefully acknowledged. Testimonies originally based on conversations with Sraffa and later collected by the author of this paper in private conversation have been subsequently checked by the people concerned. All translations in this article are my own. I am responsible for any remaining deficiency.

2 His attention to economic themes was an essential basis to his understanding of commercial law (see his 1894 paper on *La lotta commerciale,* Pisa, Spoerri; and Turati and Kuliscioff 1977:276). According to what Piero Sraffa related to Pierangelo Garegnani, Angelo Sraffa used to discuss *cases* of firms' behaviour with his son, as an exercise.

3 That institute was opened on 2 February 1920 (Romani 1997:140).

4 In 1914 Piero Sraffa campaigned for the Socialist candidate in the race for a seat in the Italian Parliament in Turin.

5 With reference to that decision Pierangelo Garegnani also stresses the role played by Piero Sraffa's love for law as an intellectual discipline and the importance of the fact that Sraffa's father was an eminent jurist. However, Garegnani also recalls that Piero Sraffa seriously considered the possibility of matriculating in the Faculty of Mathematics.

6 *Relazioni della Reale commissione d'inchiesta sulle violazioni del diritto delle genti commesse dal nemico,* Casa editrice d'arte Bestetti e Tumminelli, Milano-Roma: 335–45,

361–367 (the report is not dated; but most probably it was printed between the end of 1919 and early 1921).

7 It may be interesting to note that Sraffa took those exams on 22 October 1917, two days before the Austrian and German armies started the attack which led to the Italian *Caporetto* retreat.

8 The minutes preserved at the University of Turin (ASUT) show that to accomplish the ordinary requirements for graduation Sraffa also discussed three shorter theses in civil law, administrative law and civil proceedings; a written version of these shorter theses did not have to be submitted and has not been found.

9 Attilio Cabiati–at that time professor of economics in Genoa at the *Scuola Superiore di Commercio* and in Milan at the Bocconi University–was a good friend of Luigi Einaudi and of Piero Sraffa's father, Angelo.

10 Incidentally, we may note that this letter suggests that Sraffa prepared his thesis in the six months between mid-May and mid-November 1920; and that what appears to be the main contribution contained in that thesis (the distinction between domestic monetary stabilisation and exchange rate stabilisation) could have been the result of a reflection triggered by Cabiati's suggestion. Unfortunately we do not know why Sraffa preferred to avoid the supervision of Achille Loria, who held the chair of *Economia Politica,* in Turin. Loria was not a member of the commission which examined Sraffa's thesis even though it was recorded as belonging to the field of political economy. Other economists in that commission were Giuseppe Prato and Pasquale Jannaccone (Riccardo Fubini was another member; not Renzo Fubini, as erroneously reported in Potier 1991:8). Approximately during the same period, in Genoa, Cabiati supervised Raffaele Mattioli's thesis on a subject very similar to Sraffa's. Unfortunately, every copy of that thesis–whose title was *Note storico-critiche intorno al progetto Fisher per la "Stabilizzazione" delta moneta* (Historical-critical notes upon the Fisher project of monetary stabilisation)–seems to have been lost (Ranchetti 1986:231, 236 n. 17; Pino Pongolini 1995:14). Raffaele Mattioli became one of Sraffa's closest friends (most probably the closest). From 1925 and for the greatest part of his life he worked at the *Banca Commerciale Italiana* and he occupies an important place in the history of Italian economics, politics and culture of this century.

11 We do not know if Einaudi wrote any such letter. The letters sent by Sraffa to Einaudi from London (AFLE) only suggest that he might have met T.E.Gregory on his behalf. In any case, the letter of introduction which, as we shall see, allowed Sraffa to meet Keynes does not seem to have been written thanks to Einaudi's good officies. In September 1924, during the second period that Piero Sraffa spent in Britain, Angelo Sraffa asked Achille Loria if he could write letters to introduce Piero to James Bonar and Henry Higgs. As shown by a letter written by Piero Sraffa to Loria in October 1924, thanks to Loria's letter he actually met Bonar (AST, Fondo Loria, letters from Angelo Sraffa to Achille Loria and from Piero Sraffa to Achille Loria 21.09.1924; 29.10.1924; these letters were brought to my attention by Angelo D'Orsi).

12 In this context we may also mention the name of Giuseppe Prato (professor of economics in Turin at the *Istituto Superiore di Commercio* and of custom law and of history of economic doctrines in Milan at the Bocconi University); he was a good friend of Angelo Sraffa and in that period he showed a very warm interest in the progress of the work of the young Sraffa (AFLE letters from Angelo Sraffa to Giuseppe Prato 05.08.1920, 17.08.1920, 31.12.1921). He most probably was the first to review a work by Piero Sraffa. In fact, in the July-August 1922 issue of *La Riforma Sociale* Giuseppe Prato published a very favourable review of Sraffa's June 1922 article on the Italian bank crisis.

13 See in particular the articles published on 23 November 1919, 3 February 1920, 3 December 1920, 3 March 1921 and 23 August 1921 and subsequently reprinted in Einaudi (1961–3).

14 According to what Piero Sraffa related to Sergio Steve, it was the *Banco, di Legnano and Busto A*.

15 Sraffa's name does not appear among the list of the students taking exams, hence we may presume that he enrolled as a research student; this supposition is confirmed by the content of a 1923 letter from Keynes to J.C.C.Davidson (*KP* L/S/14).

16 A similar reconstruction is contained in the obituary of Sraffa which appeared in *The Times* on 6 September 1983; here we can read that 'on a visit to England in 1921 he met Keynes and took his fancy with a discussion of hedging on the forward exchanges'. Pasinetti's and Ranchetti's views are based upon conversations with Sraffa himself; Ranchetti, however, collected the same information also in a 1970s interview with Giovanni Malagodi. Malagodi had been one of the top managers of *Banca Commerciale Italiana* and was acquainted with Keynes; he had been told about the content of the first meeting between Sraffa and Keynes by Keynes himself. According to Pasinetti's testimony, Sraffa had formed his views on the hedging operations on forward markets for currency during the period of training that he had spent in the bank shortly after graduation.

17 Foxwell, who was born in 1849 and died in 1936, at that time was close to the end of his academic career; like Sraffa, he was a bibliophile.

18 In October 1923 Sraffa was appointed for a lectureship in Perugia and probably decided not to apply to Genoa. Foxwell's letter is preserved among the Sraffa Papers (*SP* B5/1).

19 In spring 1923 Foxwell was in Milan to deliver lectures at the Bocconi University.

20 The articles were published in July and August 1921. In a letter to Sergio Gaprioglio written in 1965 Sraffa referred to two of these articles as *compilations* (CS C17). It is not unlikely that Sraffa also translated some articles for *L'Ordine Nuovo*.

21 See Pugh (1987:124–32); *Dictionary of National Biography;* entry *Cole G.D.H.* I am grateful to Roger Simon of the *Labour Research Department* who discussed with me this part of the chapter (but I did not always follow his advice).

22 Sraffa's activity at the *Labour Research Department* is also mentioned in a document prepared in Moscow in March 1923 by Antonio Gramsci and Egidio Gennari that will be considered below (Gramsci 1992:115).

23 According to what we can read in the letter sent by Piero Sraffa to Giuseppe Prato on 2 June 1922 (AFLE) in London he also did some research at the Ministry of Labour in order to acquire information which could have been useful to his subsequent work. With reference to the sort of activity that Sraffa wished to engage in after his return from Britain we may recall what Andrea Viglongo reported to Mimma Paulesu Quercioli: 'When [Piero Sraffa] went to England to study, he requested and obtained the post of Italian correspondent for the journal *Il lavoro* [most probably *The Labour Monthly*]. However the difficulty was that he was not up on current Italian problems so felt unable to produce serious, well-argued articles (Sraffa is a very serious person!). Therefore he asked Gramsci to indicate an appropriate person to write these articles. I also know that at a certain point Sraffa told me: «You just worry about writing the articles, I'll take care of the English translation.» I was invited to dine at his house and we dealt with the problem on that occasion, over dinner, when his father was also present' (Paulesu Quercioli 1977:127; Potier 1991:23, 83 n. 6).

24 Such a centre, as far as we know, was not actually set up. A similar project was also mentioned by Gramsci in a letter written in Vienna in December 1923 (Gramsci

1992:137). A few years later Carlo Rosselli (an Italian anti-fascist who was to be killed by fascist agents together with his brother Nello in France in 1937) founded the *Giacomo Matteotti* centre for social studies; but this centre was closer to the Socialist than to the Communist Party. Piero Sraffa offered to support this initative also, but we do not know if he actually did any work for it (Turati and Kuliscioff 1977:280, 284; Tranfaglia 1968:135, 168, 191; Potier 1991:14). With reference to these projects, it may be worth reproducing Giorgio Amendola's opinion on the state of the research on economic and social conditions of Italy at that time: 'a recognition of the state of our country in the 1919–20 period was lacking from all sides. There was a book going round when I was a boy, by two English authors, Bolton King and Thomas Okey, *L'Italia d'oggi* (Today's Italy). But it had been written in 1900. There was nothing like it for 1919–20. The socialists, populists, Giolitti, Nitti, etc. laid out programs, indicated objectives, but never with the recognition of the actual state of things in Italy as their point of departure' (Amendola 1976:34).

25 The information on Angelo Sraffa does not seem to be accurate and a discussion of the data used by Piero Sraffa in his December 1922 article on the Italian banking system does not suggest that he had access to confidential sources (Naldi 1998c). 'Molinari' most probably was Alessandro Molinari (born in 1898 he graduated in 1920 at the Bocconi University with a thesis on the Soviet economy and later acquired world-wide renown as a statistician) who was the director of the labour office of the municipality of Milan. The monthly bulletin *Città di Milano. Bollettino municipale mensile di cronaca amministrativa e di statistica* shows that at least until 1922 he was well known for his left-wing political stance (*Città di Milano* vol. XXXVIII:159, 336–7; *L'Avanti!* 2.12.1922). In 1929 he became *direttore generale* of the *Istituto Centrale di Statistica* and, in 1948, *direttore generale* of the *Associazione per lo sviluppo dell'industria nel Mezzogiorno.* After the end of the war he lost his position at the *Istituto Centrale di Statistica* because he was (most probably because of his strong hand against his employees who did not carry out their duties and not because of his actual political involvement) judged to have been too closely connected with the fascist regime: actually he was never a member of the Partito Nazionale Fascista (Lenti 1984:57). In one of Piero Sraffa's pocket diaries, on 3 January 1929 we read the following entry: *Molinari* (*SP* E2). According to Ettore Molinari (son of Alessandro Molinari), the appointment of his father to the *Istituto Centrale di Statistica* was personally approved by Mussolini; Mussolini knew Alessandro Molinari from the time when Mussolini himself was a socialist and, living in poverty in Milan, was often fed in the home of Ettore Molinari (an anarchist and professor of chemistry who also taught at the Bocconi University, and father of Alessandro).

26 Among the *Sraffa Papers* we can find a letter of the *Commissario Prefettizio* of the Province of Milan (SP B4/1/31) dated 15 December 1922 which states that Sraffa's resignation, referred to as 'spontaneously presented', had been accepted by the *Deputazione Provinciale.*

27 The budget did not pass because the president and the *Deputazione,* following a tradition of that council, did not vote.

28 In October 1922 the Italian Socialist Party had split into a left-wing and a right-wing: the *Partito Socialista Italiano* and the *Partito Socialista Unitario*–Nino Levi joined the second. We may wonder if this division influenced the decision of the socialist group at the *Consiglio Provinciale.*

29 The *Comune di Milano* (Municipality of Milan) had also been governed by a socialist administration; but in August 1922 the city council had been dissolved by an act of the police authority.

30 Mussolini, who had been appointed Prime Minister at the end of October 1922, certainly knew Angelo Sraffa as Rector of the Bocconi University; but he also

got directly in touch with him in February 1922, when Angelo Sraffa, who had been assailed by young fascists at the entrance of the Bocconi University, asked him–as he was the editor of a newspaper which on that occasion had strongly attacked Angelo Sraffa and his university–to join a commission which would have assessed the propriety of Sraffa's own behaviour (Romani 1993).

31 The imprint does not allow a complete reconstruction of the text; but a comparison with the translation contained in Sraffa's letter to Keynes leaves no doubt on its nature.

32 This testimony is confirmed by what Livia Beux related to the author of this chapter.

33 A more detailed discussion of Sraffa's article and of Mussolini's reaction is contained in Naldi 1998c, but since that paper was published Luigi Pesinetti has also been able to trace the letter sent by Toeplitz to Keynes. That letter was published in March 1923 in 'The European Reconstruction Supplement' to *The Manchester Guardian Commercial*.

34 In order to ascertain the reason which caused the refusal further research in Italian and British archives is required, but the research accomplished up to now has been fruitless. However, it may be interesting to note two episodes which might be relevant. The first is that according to what has been reported by Andrea Viglongo 'in 1921 Piero Sraffa [had] brought a booklet to *L'Ordine Nuovo* from England containing guerrilla war instructions, published by Irish revolutionaries, with precise details, for example, on how to put a locomotive out of service' (1978:136). The second is that the connections between Sraffa and socialist or communist circles during his first stay in Britain were known to Milanese fascist quarters; in fact in 1927 those connections were referred to in an article published in *Libro e Moschetto* (Book and Gun) (Naldi 1998b, 1998c). The period that Sraffa spent in London between 1921 and 1922 saw the development of a very active and organised fascist movement within the Italian community in London (Bernabei 1997:53).

35 Most probably it was on this occasion that Sraffa sought refuge abroad; not during the days immediately after Mussolini's telegrams, when he seems to have been in Switzerland in order just to avoid police censorship of his correspondence (Kahn 1984:4; Ingrao and Ranchetti 1996:523).

36 Keynes had written to Sraffa that in London he would have found a job for him and we may presume that it would have been in a field relating to money and banking (*KP* L/S/9). Among the *Sraffa Papers* are preserved some notes on questions relating to labour economics which appear to have been written in Paris in February 1923 (*SP* D1/67, 69).

37 Some of these notes are closely related to the origin of the reflection which was to lead to some of the arguments developed in the 1925 article (*SP* D1/2/9–14). A very short note on Marshall's *Principles* preserved among the *Sraffa Papers* is written on a card used by Sraffa in his capacity of director of the *Ufficio provinciale del lavoro* (*SP* D1/40/7); this fact suggests (but certainly does not prove) that he might have started his detailed reading of that book when he still was director of that office, i.e., before December 1922.

38 For a more detailed presentation and discussion of the content of this course and of the course on *Scienza delle finanze* see Naldi (1998a).

39 A more accurate discussion of this theme should certainly dig deeper into Gramsci's writings and into the *Sraffa Papers;* in particular into Sraffa's manuscripts grouped by Sraffa himself as 'Notes up to 1927' and into related papers.

40 Sergio Steve reported to the author of this chapter that Piero Sraffa told him that 'he and Gramsci had great discussions; they would get together in the evening and discuss something all night long, and then carry on the following day as well' (also Servadio 1993:69–70).

41 See Gramsci and Schucht (1997:1015–16, 1039–41) and Sraffa (1991a:72–5).

42 In this context it may be worth recalling that Paolo Vita-Finzi, who was a very close friend of Piero Sraffa, referring to the years 1920–1, wrote: 'at that time my political opinion was mainly influenced by the lectures of Luigi Einaudi and by the reading of classical economists' (Vita-Finzi 1989:139).

43 The recognition of a radical difference between the old classical approach and the new marginalist approach could not be taken for granted at all in the early 1920s. The Marshallian school clearly denied such a radical difference, and that school exerted an important influence also on the socialist movement. Consider for instance what Maurice Dobb wrote about his first book (*Capitalist Enterprise and Social Progress,* published in 1925): 'an unsuccessful and jejune attempt to combine the notion of surplus-value and exploitation with the theory of Marshall' (Dobb 1978). And in an article probably written by Palmiro Togliatti and published by Gramsci in *L'Ordine Nuovo* in 1919, we find an approving reference to 'that part of economic doctrines which properly deserves the name of science and which is restricted to the study of the action of economic forces and to the way in which they balance together to create an equilibrium' (*L'Ordine Nuovo* n. 13, 9 August 1919).

44 Giorgio Gilibert has put forward a different interpretation of those testimonies and he conjectures that they might refer to the impetus which in Autumn 1927 led Sraffa to the crucial change of direction in his research which about thirty years later resulted in the publication of his *Production of Commodities*. According to Gilibert the source of that impetus might lie in the fact that in 1922–3, in the USSR, Gramsci had an opportunity to appreciate the role that Soviet economists were attributing to Quesnay's *Tableau* and to Marx's schemes of reproduction (Gilibert 1999). On the grounds offered by the information available we cannot rule out the validity of this conjecture.

Part II

Sraffa's contribution to the Cambridge debates in the 1920s and 1930s

3 Sraffa and the criticism of Marshall in the 1920s

Roberto Marchionatti[1]

1 Introduction

In the 1920s in the international economic literature, and principally on the pages of *The Economic Journal,* many economists were involved in a long debate on the Marshallian theory of value and competition: known as 'the controversy on costs', it contributed substantially to the foundation of the contemporary theories of value and competition. Schumpeter wrote that this debate was a 'striking instance of the slowness and roundaboutness of analytical advance' (Schumpeter 1954:1048). This feature is not strange if we know that, especially in the English speaking countries, in that debate the core of economics in its most diffused version was at stake. As is well known in England, the period between 1895 and 1914 is known as 'the Marshallian Age' and in the perhaps most prestigious university of the world of that time, Cambridge, 'Marshall was economies', as Joan Robinson effectively said. In any case, the relevant intellectual effort produced in that period of theoretical invention deeply influenced successive developments in economic theory: this fact justifies the recurrent interest of the economists and the historians of economics in that period.

Another well-known fact is that a relevant position in that debate was held by the Italian economist Piero Sraffa, with 'his brilliantly original performance', as Schumpeter wrote, constituted by the 1926 article on the *Economic Journal,* and anticipated by an Italian article in 1925. But, surprisingly, seventy years after those contributions, historians of economics still haven't come to a general consensus on the full significance of Sraffa's anti-Marshallian articles of 1925–6. Some authors, the majority, suggest that Sraffa constructed a 'sturdy' intellectual foundation for the English branch of the theory of imperfect competition and particularly for Joan Robinson's 1933 book (see for example Schumpeter 1954; Samuelson 1967; Shackle 1967; Blaug 1968; Roll 1973); others see Sraffa as a part of a new mainstream, marked by the tacit agreement that is better to have a poor, useful theory than a rich, useless one (for example Machovec 1995). Other authors stress the fact that Sraffa was essentially a critic of partial method and

an advocate of a simultaneous equation approach, the outcome of which was the 1960 book; in other words they emphasise the methodological issue: partial versus general equilibrium (for example Maneschi 1986; Newman and Vassilakis 1988; Panico 1991; Roncaglia 1991; Panico and Salvadori 1994). The more recent contributions tend to an internal rational reconstruction of Sraffa's articles, substantially ignoring the historical context of Sraffa's theoretical reflection: the question of the relationship between Sraffa and the wide criticism of Marshall in that period remains obscure, and consequently also the possibility of defining the *differentia specified* of Sraffa's work. But we must not forget that Sraffa himself said that his article was an attempt to coordinate previous critical material. Sraffa maintained that the foundations of the supply curve based upon the laws of increasing and decreasing returns were not only less solid than those of other portions of the modern theory of value but 'so weak as to be unable to support the weight imposed upon them' (Sraffa 1926a:536). Its scarce solidity, Sraffa continued, was generally recognised and its weakness was doubted by many, but not openly expressed:

> With the lapse of time, the qualifications, the restrictions and the exceptions have piled up, and have eaten up, if not all, certainly the greater part of the theory. If their aggregate effect is not at once apparent, this is because they are scattered about in footnotes and articles and carefully segregated from one another.
>
> (Sraffa 1926a:536)

So Sraffa's purpose was 'to attempt to co-ordinate certain material, separating what is still alive from what is dead in the concept of the supply curve and its effects on competitive price determination' (ibid.) in order to cancel the 'tranquil', but mistaken, view which 'the modern theory of value presents us' (ibid.). Actually, if Sraffa's judgement of tranquillity was perhaps correct for the English situation of the early 1920s, it was scarcely applicable to the international situation of that period, definable as anything but tranquil. On the other hand, Sraffa's criticism was far from being simply an attempt to coordinate critical materials: those articles revealed a great critical originality and acuteness, which hardly escaped his contemporaries, and cannot be escaped now.

The purpose of this chapter is to contribute to the reconstruction of the general framework of the criticism of the Marshallian theory of value in competitive conditions, to comparatively evaluate Sraffa's criticism and its significance. The chapter is organised as follows. In Section 2, Marshall's legacy is briefly presented; Section 3 is the central part of the chapter: it is devoted to the examination of the most important contributions to the debate between 1921 and 1930, and particularly to Sraffa's contribution; in conclusion some remarks on the role and significance of Sraffa's contribution are presented.

2 Marshall's legacy on value and competition

The analysis of the criticism of the Marshallian theory of value in the 1920s needs preliminary and careful examination of the theory of competition in Marshall's *Principles,* traditionally an object of wide discussion, but rarely examined in its entirety. I suggest that a correct analysis of Marshall's theory of competition requires a combined analysis of some parts of Book IV and V of the *Principles,* not only the latter. If we follow such an approach, we may note that in Book IV: first, Marshall's notion of competition appears as a process in which elements of partial and temporary monopoly exist, and competition rests fundamentally on the 'openness of markets' not on atomistic price-taking behaviour; second, the analysis of competition goes along with the theory of the firm's and industry's growth; last but not least, we must emphasise that Marshall's analysis of the firm and the competition process in Book IV was strictly connected to that of classical economists, particularly Adam Smith.[2] On the other hand, Book V represents a (substantially unsuccessful, as we will see) attempt to maintain at least in part the dynamic character of industrial competition, as described in Book IV, in a stationary context.

Marshall used the Smithian division of labour concept as a starting point in his analysis of industrial competition. In Book IV, chapter IX, after introducing the basic relation between the division of labour and the extent of the market, Marshall said that the chief advantage of the division of labour is the fact that constantly machinery supplants purely manual skill and the chief effect of improvement of machinery 'is to cheapen and make more accurate the work which would anyhow have been subdivided' (Marshall 1961 [1890]:255). Moreover, the division of labour, generating increasing returns, tends 'to increase the scale of manufactures and to make them more complex; and therefore to increase the opportunities for division of labour of all kinds' (ibid.: 256). In Smithian words we can say that division of labour produces a further division of labour limited by the extent of the market. The economies of production permitted by the division of labour were classified by Marshall in two classes, internal economies–'those dependent on the resources of the individual houses of business engaged in it, on their organisation and the efficiency of their management' (ibid.: 266)–and external economies–'those dependent on the general development of the industry' (ibid.).

External economies are discussed in Book IV, chapter X, in connection with 'the concentration of specialised industries in particular localities'. Marshall described the 'external economies' which induce an industry, when it has chosen a locality, to stay there for a long time. They are internal to the industry or inter-industrial–the distinction between the two definitions is not clear. They are: a) dissemination of skill and know-how in the district-areas; b) the diffusion of inventions and improvements in machinery, in processes and the general organisation of business, all 'promptly discussed' in those

districts; c) the growth of subsidiary trades in the neighbourhood; d) the increasing availability of entrepreneurial ability and a local market for special skill. The external economies which are by their own nature essentially inter-industrial are the variety of employment and skills given by the existence of different but complementary industries, localised in the same area. An important point Marshall stressed about external economies is their partial irreversibility, of which he speaks in the Appendix H, which therefore makes external economies a function of production level *and* time.

Internal economies are discussed in Book IV, chapters XI and XII. They are achieved by the firms through the production on a large scale: 'the chief advantage of production on a large scale are economy of skill' and 'economy of machinery' (ibid.: 278), as well as the capability of exploiting economies of buying and selling. As regards the economy of skill, Marshall stressed the fact that 'the large manufacturer has a much better chance than a small one has, of getting hold of men with exceptional natural abilities, to do the most difficult part of his work, that on which the reputation of his establishment chiefly depends' (ibid.: 283). As regards the economy of machinery, Marshall emphasised the fact that a large factory has advantages, in comparison with small factories, in the use of specialised machinery which are of 'growing variety and expensiveness of machinery' (ibid.: 279). As regards the economies of buying and selling, Marshall said that:

> A large business buys in great quantities and therefore cheaply; it pays low freight and saves on carriage in many ways.... It often sells in large quantities, and thus saves itself trouble; and yet at the same time it gets a good price, because it offers conveniences to the customer by having a large stock from which he can select and at once fill up a varied order; while its reputation gives him confidence. It can spend large sums on advertising by commercial travellers and in other ways; its agents give it trustworthy information on trade and personal matters in distant places, and its own goods advertise one another.
>
> (Marshall 1961 [1890]:282)

Marshall recognised that a firm, taking substantial advantage of internal economies and increasing its efficiency, that is exploiting increasing returns, could become a monopoly. In effect this had been Cournot's conclusion: increasing returns and competition cannot co-exist. But Marshall disagreed with Cournot's view on increasing returns: in a letter to A.W.Flux dated 7 March 1898, Marshall clearly explained his disagreement:

> You say that, à propos of increasing returns, you are inclined to lay stress on the incomplete utilisation of existing production resources. That is of course one of my chief hobbies. My confidence in Cournot as an economist was shaken when I found that his mathematics re I.R.led

> inevitably to things which do not exist and have no near relation to reality. One of the chief purposes of my *Wanderjahre* among factories was to discover how Cournot's premises were wrong. The chief outcome of my work in this direction, which occupied me a good deal between 1870 and 1890, is in the 'Representative Firm' theory…the supplementary cost analysis; as well as the parts that directly relate to supply price for I.R.
>
> (Marshall 1925:406–7)[3]

The representative firm–a multifaceted concept as we will see–which is considered the fundamental device Marshall used to solve the 'Cournot's dilemma', is introduced in Book IV, chapter XIII. The term appears after Marshall synthesised the typical 'cycle of life' of a firm. The latter is so presented–this passage is of such relevance that it is to be extensively cited:

> An able man, assisted perhaps by some strokes of good fortune, gets a firm footing in the trade, he works hard and lives sparely, his own capital grows fast, and the credit that enables him to borrow more capital grow still faster; he collects around him subordinates of more than ordinary zeal and ability; as his business increases they rise with him, they trust him and he trusts them, each of them devotes himself with energy to just that work for which he is specially fitted, so that no high ability is wasted on easy work, and no difficult work is entrusted to unskilful hands. Corresponding to this steadily increasing economy of skill, the growth of his business brings with it similar economies of specialised machines and plants of all kinds; every improved process is quickly adopted and made the basis of further improvements; success brings credit and credit brings success; credit and success help to retain old customers and to bring new ones; the increase of his trade gives him great advantages in buying; his goods advertise one another, and thus diminish his difficulty in finding a vent for them. The increase in the scale of his business increases rapidly the advantages, which he has over his competitors, and lowers the price at which he can afford to sell. This process may go on as long as his energy and enterprise, his inventive and organising power retain their full strength and freshness, and so long as the risks which are inseparable from business do not cause him exceptional losses; and if it could endure for a hundred of years, he and one or two others like him would divide between them the whole of that branch of industry in which he is engaged…. But here we may read a lesson from the young trees of the forest as they struggle upwards through the benumbing shade of their older rivals. Many succumb on the way, and a few only survive; those few become stronger with every year, they get a larger share of light and air with every increase of their height, and at last in their turn they tower above their neighbours, and

> seem as though they would grow on for ever, and for ever become stronger as they grow. But they do not. One tree will last longer in full vigour and attain a greater size than another; but sooner or later age tells on them all. Though the taller ones have a better access to light and air than their rivals, they gradually lose vitality; and one after another they give place to others, which, though of less material strength, have on their side the vigour of youth. And as with the growth of trees, so was it with the growth of businesses.... In almost every trade there is a constant rise and fall of large businesses, at any moment some firms being in the ascending phase and others in the descending.
>
> (Marshall 1961 [1890]:315–17)

As it appears clearly in this passage, Marshall maintained that, as much as factors such as skill, inventiveness and entrepreneurial energy, needed to exploit potential internal economies, exist, a firm can grow rapidly, yet the tendency to monopoly is not inevitable because the rise of diminishing returns in the life cycle of the firm opposes it. Monopolisation of the market on behalf of a firm can at the most be partial and temporary.[4] The concept of equilibrium appropriate to the representation of this continuous change, according to Marshall, is the *'biological equilibrium',* so defined: 'A business firm grows and attains great strength, and afterwards perhaps stagnates and decays; and at the turning point there is a balancing of equilibrium of the forces of life and decay' (ibid.: 567)

So, this 'business firm' is typical, or *representative,* from a *'biological point of view',* in the sense that it represents the typical growth path of a firm. Marshall continued saying that 'these results will be of great importance when we come to discuss the causes which govern the supply price of a commodity' (ibid.: 317). In that context (Book V) the analysis regards the normal cost of production of a commodity relative to a *given* aggregate volume of production in a competitive and stationary (or static) context of partial equilibrium. For this purpose, Marshall said introducing the term 'representative': 'we shall have to study the expenses of a representative producer for that aggregate volume' (ibid.: 317). This firm is representative from a *'mechanical point of view'*. In fact in Book V Marshall introduced a *mechanical equilibrium* concept which he considered simpler and conceived as a useful approximation to prepare the way 'for the study (advanced) of the equilibrium as resembling a balancing of forces of life and decay' (ibid.: 323).

This 'representative firm' is 'normal'–in the sense that, as Marshall said, it 'must be one which has had a fairly long life, and fair success, which is managed with normal ability, and which has normal access to the economies, external and internal, which belong to that aggregate volume of production' (ibid.: 317)–or 'average'–'that particular sort of average firm, at which we need to look in order to see how far the economies, internal and external, of production on a large scale have extended generally in the industry and

country in question' (ibid.: 318),[5] which increases with an increase in the aggregate volume of production. This latter 'will generally increase the size and therefore the internal economies possessed by such a representative firm' and 'will always increase the external economies to which the firm has access' (ibid.).

What is the function of this representative firm in the context of the theory of value? According to Marshall, the reference to a representative firm is *necessary* 'especially when we are *considering industries which show a tendency to increasing return*' (ibid.: 376; note, our italics). In fact, when Marshall introduced the supply curve he assumed that the supply price increases with the production increase because 'cases in which the supply price falls as the amount increases involve special difficulties of their own; and they are postponed to chapter XII of this Book' (ibid.: 345). At first, in chapter XII, Marshall said, 'there will be found a more detailed study of that extremely complex notion, a marginal increment in the process of production by a representative firm' (ibid.). Chapter XII examines 'some difficulties connected with the relations of demand and supply as regards commodities the production of which tends to increasing returns' (ibid.: 455). Therefore the representative firm is, according to Marshall, at first a useful tool for studying the equilibrium of an industry then a necessary tool when the fact of increasing returns is introduced. The necessity of the representative firm for Marshall is essentially of methodological nature. Marshall observed that the tendency to a fall in the price of a commodity as a result of a gradual development of the industry by which it is made is 'quite a different thing from the tendency to rapid introduction of new economies by an individual firm that is increasing its business' (ibid.). The reason is that, as he had said in Book IV, a firm grows and decays, makes up against difficulties in selling, and so on (see also Marshall 1890:458 note). If this is not taken into account there is the risk of falling into 'Cournot's error':

> Abstract reasoning as to the effects of the economies of production, which an individual firm gets from an increase of its output are apt to be misleading, not only in detail, but even in their general effect…. Some, among whom Cournot himself is to be counted, have before them what is in effect the supply schedule of an individual firm; representing that an increase in its output gives it command over so great internal economies as much to diminish its expenses of production; and they follow their mathematics boldly, but apparently without noticing that their premises lead inevitably to the conclusion that, whatever firm gets a good start will obtain a monopoly of the whole business of its trade in its district.
>
> (Marshall 1961 [1890]:459, note)

The device of the representative firm permits avoiding this methodological error. With the representative firm, Marshall tried to bring together the

assumption of the equilibrium of the industry and of the disequilibrium of the individual firms of that industry, in which some firms are rising and others are declining. So the representative firm provides a representation 'in miniature', to use Frisch's expression (1950), of the supply curve of the industry. This 'construction of the mind" is representative with respect to size and unit cost: it is like a typical tree of a virgin forest, because it is always representative of the average life cycle of the firm in the industry. The adoption of the assumption of a stationary state permits maintaining the link with the problem of the firm's growth, in which Marshall was chiefly interested. The process of growth is explained in the language of stationary equilibrium. Hence, according to Marshall, the representative firm represents the application of the correct method of inquiry. This device is considered necessary by Marshall essentially from a methodological point of view, descending of his interpretation of the role of the 'Theory':

> In my view 'Theory' is essential. No one gets real grip of economic problems unless he will work on it. But I conceive no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics 'proper'. It seems to me an essential but a very small part of economics proper: and by itself sometimes even–well, not a very good occupation of time…. General reasoning is essential, but a wide and thorough study of facts is equally essential…. A combination of the two sides of the work is *alone* economics *proper*. Economic theory is, in my opinion, as mischievous an impostor when it claims to be economics proper as is mere crude unanalysed history.
>
> (letter from Marshall to Edgeworth, in Marshall 1925:437)

As Chamberlin (1961:541) clearly noted, 'a fine distinction between "theory" and "real life" in Marshall's economics is impossible to draw because Marshall himself did not draw it, and never tired of warning others against drawing it'.[6] For Marshall the abstract reasoning, the chain of theoretical deductions, has to be limited: absolute rigour means neglecting time and irreversibilities and so coming to the wrong conclusions.[7] Therefore, it is not simply because of the problem of 'realism' that he wanted to consider the fact that in practice firms and industries operate on falling supply curve most of the time, as Schumpeter in his *History* maintains.[8]

3 The criticism of Marshall in the 1920s in the USA and UK

The pre-Sraffian phase

'The action of increasing returns in a competitive regime has proved a stumbling-block even to expert economists', wrote Edgeworth in his 1925 review of the 1924 new edition of Pigou's *Economics of Welfare,* and continued: 'It is justly

considered by Mr. Keynes that "this is the quarter in which the Marshall analysis is least complete and satisfactory, and where there remains most to do" (Keynes)' (Edgeworth 1925:30). Actually the problem 'increasing returns and competition', as a part of the most general problem of the definition of a complete theory of competition, did not cease to absorb economists' minds after the publication of the *Principles*. But it was at the beginning of the 1920s that dissatisfaction for the state of the existing price theory arose: an intellectual ferment developed, first in the USA and then in the UK, which concentrated on the questions Marshall had raised and demanded a consistent logical rigor in the oldest theories. Marshall's methodological proposal was perceived as inconsistent between the static base and the dynamic superstructure. In the Marshallian field Pigou followed this path of rigour in its attempt of completion and refinement of Marshall's theory, but this required a methodological turning, which Marshall thought risky and therefore did not make: and indeed it was Pigou who made that turning. The question of the coexistence of increasing returns and competition is identified with the other aspect of the problem: the 'bewildering vagueness of the term competition' (Moore 1906).

Some premises. Pigou's formal definition of competition, the external economies and Young's criticism

Pigou was Alfred Marshall's successor in the Cambridge Chair of Political Economy and a leading figure in Anglo-American economists who in his 1912 book *Wealth and Welfare* and then in the successive *Economics of Welfare* (1920 and 1924)–two books which, we may say *a posteriori,* represent a 'methodological break' as regards the Marshallian tradition[9]–developed the welfare side of Marshall's theory. In particular, in *Wealth and Welfare* Pigou contributed to the formal definition of competition (see chapters IV and V of part II) offering an allocative interpretation of free competition. In this context Pigou faced the question of trying to reconcile Marshall's falling long-run supply curve with the price-taking aspect of perfect competition using the concept of external economies as already treated by Edgeworth (1905). Pigou wrote in *Wealth and Welfare:*

> Provided that certain external economies are common to all the suppliers jointly, the presence of increasing returns in respect of all together is compatible with the presence of diminishing returns in respect of the special work of each severally. And this is sufficient to permit a stable equilibrium.
>
> (Pigou 1912:177)

In the first edition of *Economics of Welfare* (1920) he wrote that, if we consider increasing returns, the equilibrium is unstable–it would seem that one of the suppliers must drive all the others out of the market–but:

> 'In real life, however, when the commodity is one the production of which on a large scale is associated with 'external economies', the separate sources are not entirely independent. Consequently, the presence of increasing returns in the market as a whole does not *imply,* though, of course, it is compatible with, its presence in the parts.... The constituent of the commodity...may obey the law of diminishing returns in all the sources for any aggregate of production, while the other constituent obeys the law of increasing returns rapidly enough to give the character of increasing returns to the supply schedule of the two constituents jointly. It is, thus, seen that the apparent conflict between mathematical analysis and experience, which has on occasions perplexed the treatment of increasing returns, may in some circumstances–not, of course, in all–be resolved even without reference to the time element.
>
> (Pigou 1920:744–5)

Increasing returns and competition are compatible using the device of considering the external economies generated internally to an industry 'without reference to the time element': in this way the industrial supply curve can be decreasing whilst the firm's supply curve is increasing. By doing so Pigou necessarily gave up the inter-industrial characteristic of external economies and their irreversibility, therefore renouncing to consider the dimension of time. This exclusion represents the most evident difference between Marshall's treatment of the problem and Pigou's. But this formal solution was immediately criticised, particularly for its lack of realism: in his review of Pigou's *Wealth and Welfare*–which had a remarkable impact on the profession–the American economist Allyn Young hinted that increasing returns are not of the same character as diminishing returns and are not subject to analysis of the partial-equilibrium type (Young 1913).[10] He wrote:

> I imagine...that cases of increasing returns in this sense (i.e. diminishing aggregate expenses per unit of product as production increases) must be rare, if not altogether lacking, in competitive industry, unless an increase in the size of the representative establishment be taken into account as a natural concomitant of increased production in the industry in question.
>
> (Young 1913:678)

In the note he refused Pigou's concept of external economies as adequate to solve the Cournot dilemma:

> The economies of large scale production affect industry at large (if competitive) only by reducing the expense per unit in individual establishments. It is scarcely logical to treat these economies in the same

> general manner as the increasing expense of agricultural production, which arises from causes external to the individual undertaking.... I cannot imagine [Professor Pigou's] 'external economies' adequate to bring about this result.
>
> (ibid.)

Knight's 'refinement' of the perfect competition theory, 1921–5

The concept of perfect competition–according to the authoritative judgement by Stigler–received its complete formulation in Frank Knight's *Risk, Uncertainty and Profit* (1921a) (Stigler 1957:256), originally his doctoral dissertation at Cornell under the supervision of Allyn Young then revised at Chicago. Actually, between 1921 and 1925, Knight, building explicitly on the basis of Marshall, J.B.Clark and Schumpeter, without forgetting Moore[11] and the mathematical school contributions by Walras and Pareto, offered what at that time was perceived as the most complete reformulation–a 'refinement, not reconstruction' he wrote–of the perfect competition theory.[12] In this book, as well in his article of the same year, 'Cost of production and price over long and short period' published in the *Journal of Political Economy,* Knight defined the concept of perfect competition in rigorous terms and argued for a sharp separation between the static and the dynamic problems. Knight regarded the Marshallian concept of external economies and increasing returns as falling within the dynamic area. Thus, it was logically inconsistent to use it, as Marshall had, in the static price theory.

Knight (1921a) presented the problem economics faced at that time as follows:

> In the great mass of economic literature there is certainly still waiting the evidence of a comprehensive grasp of general principles and even more of the meaning and importance of general principles in a scientific program.
>
> (Knight 1921a:14–15)

Indications of progress in this field was furnished, according to Knight, especially by the discussion centring around 'the concept of normality' in the work of Marshall and the related notion of the static state 'exposed in particular in this country by J.B.Clark' (ibid.: 23).[13] But, in his attempt to offer a clearer formulation of premises, a perhaps more important role, according to Knight himself, was that of J.A.Schumpeter (1908 and 1912), 'who has carried the static analysis further in some respects than Professor Clark' (Knight 1921a:33). Knight assumed Schumpeter's judgement on Marshall, according to whom Marshall's theory is old-fashioned, eclectic and unable to recognise the fundamental difference existing between statics and dynamics. Knight

maintained that, although the meaning and bearings of the fundamental concepts are much better worked out by Marshall than by any other writer generally read, Marshall himself had adopted a cautious, almost anti-theoretical attitude towards fundamentals; he refused to lay down and follow rigidly defined hypotheses, but insisted on sticking as closely as possible to concrete reality and discussing 'representative' conditions as opposed to limiting tendencies. 'The gain in concreteness and realism', Knight wrote, 'is in our opinion much more than offset by the obscurity, vagueness, and unsystematic character of the discussion, the inevitable consequence of burying fundamentals in an overwhelming mass of qualification and detail' (ibid.: 15). Consequently Knight thought it was necessary to adopt 'a sharper separation of the theoretical portion of economics from the empirical portion, and towards the clearer formulation of premises' (ibid.: 14). He stressed the scientific, necessary, role of static method in economics, maintaing that: 'The static method in economics does merely this. It inquires what conditions exist and studies the results which recognisable forces at work…tend to produce under those conditions' (ibid.: 17). As from these methodological premises, Knight presented the assumptions necessary to have perfect competition: complete 'rationality'; 'perfect mobility in all economic adjustments', no cost involved in movements or changes; 'perfect, continuous, costless intercommunication between all individual members of the society', free and independent individuals. Under these conditions the long-term supply curve must have an increasing shape. Decreasing cost as a long-run tendency is indeed impossible under a natural competitive adjustment of industry and incompatible with long-run competitive conditions:

> Decreasing cost (or increasing returns) is alleged to result in several ways…. The most important is the technological economy of largescale production. When the output of a commodity is increased, the cost of the productive services used to produce it will be higher; but this increase in their cost per unit may, it is held, be more than offset by economies in utilisation, made possible by larger-scale operations, which increase the amount of product obtained from given quantities of materials and resources consumed…. If competition is effective, the size of the productive unit will tend to grow until either no further economies are obtainable, or there is only one establishment left and the industry is a monopoly. When all establishments have been brought to the most efficient size, variation in total output is a matter of changing their number, in which no technical economies are involved.
>
> (Knight 1924:598)

In his 1924 article 'Some fallacies in the interpretation of social cost' and in the successive 1925 article, Knight dismissed the Pigouvian idea of external economies:

> The rejoinder to the above argument is the doctrine of 'external economies', which surely rests upon a misconception. Economies may be 'external' to a particular establishment or technical production unit, but they are not external to the industry if they affect its efficiency…. External economies in one business unit are internal economies in some other, within the industry. Any branch or stage in the creation of a product which offers continuously a chance for technical economies with increase in the scale of operations must eventuate either in monopoly or in leaving the tendency behind and establishing the normal relation of increasing cost with increasing size.
>
> (Knight 1924:598)

In his rejoinder to Graham, echoing Young (1913), he wrote:

> In spite of the weight of authority which may be cited for such [external] economies, I have never succeeded in picturing them in my mind, or finding any convincing reason to believe they exist. I can imagine conditions in which the production of a good may be tied up with a monopolistic industry like transportation, operating below capacity, in such a way as to produce some tendency in the direction argued. But I cannot believe such conditions general enough to justify a special law in economic theory.
>
> (Knight 1925:322)

Finally we have to remember a neglected aspect of Knight's work of these years which partly anticipates Sraffa's positive suggestions of the 1926 article. He wrote in his 1921 book that the competitive theory may be a good analytical device, a first approximation, but it does not exist in the real world, so it has to be supplemented so that the theory would be more applicable, and in his 1921 article:

> One of the most serious oversights in the discussion of decreasing cost is the neglect of the mixture of competition and monopoly which is a general characteristic of the type of business supposed to exhibit this type of cost function…. The correct approach to the explanation of price in the case of partial monopoly would seem to be to apply the theory of monopoly, not that of competition.
>
> (Knight 1921b:332)[14]

The controversy on the 'empty economic boxes', 1922–4

In his 1924 article Knight held that 'until a plausible example is brought forward, the category of decreasing cost under stable competition remains an *empty economic box*' (Knight 1924:333): this expression seems to echo the debate raised in the pages of the English *Economic Journal* in those same years,

a controversy inside the Marshallian school which timidly but explicitly opened a criticism against Pigou.

J.H.Clapham, Marshall's student in the 1880s who became Professor of Economic History at Cambridge in 1928, in his article 'Of empty economic boxes' (1922a), expressed the economic historians' dissatisfaction towards the analytical tools available, describing the difficulties of an economist, well-educated in the dominant British school, in practically putting on the shelves of his mind the boxes containing the real industries in accordance with the categories of increasing, decreasing or constant returns:

> He tries *The Economic of Welfare* to find that, in nearly thousand pages, there is not even one illustration of what industries are in which boxes, though many an argument begins–'when conditions of diminishing returns prevail' or 'when conditions of increasing returns prevail', as if everyone knew when that was.
>
> (Clapham 1922a:305)

But 'how should he conceive his unit of resources?', or 'how is he to conceive of 'an industry'? Substantially Clapham's paper maintained that there were difficulties in the conception of a rate of returns in industry, and particularly of a rate of increasing returns, and that there were difficulties in deciding which particular industries are at the present time being conducted under conditions of increasing or conditions of diminishing returns–difficulties which kept these economic boxes empty. Pigou (1922) replied, without convincing Clapham: he recognised the existence of difficulties but maintained that the problem was not if these concepts can directly help in the practical conduct of affairs, but whether the concepts of increasing and diminishing returns are instruments of service in the construction of a realistic economic science:

> These boxes…are not merely boxes; they are also elements in the intellectual machinery by which the main part of modern economic thought functions. If then it be granted that this thought, as a whole is able to render any practical service…these particular elements in that machinery cannot be singled out from the rest and condemned as useless; they are an organic and inseparable part of that machinery. But there is a further consideration of a more direct kind. Even regarded as boxes, and empty ones at that, the categories od i. and d, r. are not mere ornaments… It enables us to discover…what assumptions are implicit in the statement about economic causation that politicians and other such persons are accustomed to make for the guidance of the public.
>
> (Pigou 1922:462)

Hence the solution of the problem, according to Pigou, was not to renounce the theory but rather to enrich the empirical portion of economics. The

controversy between Pigou and Clapham made evident the problem of the realism of the analytical tools emerging from Pigou's theoretical 'departure' from the more concrete Marshallian approach in favour of a more elaborated and formal analysis.

Two years later, Dennis H.Robertson, Pigou's pupil, echoing 'certain criticisms which have been wafted across the Atlantic on the analytical mechanism elaborated by Pigou in his *Economics of Welfare* for dealing with these conceptions of diminishing and increasing returns' (Robertson 1924:16), criticised the fact that in Pigou, 'all the improvements in organisation from which decreasing cost arises are of the nature of external economies …the familiar internal economies…having vanished into thin air' (ibid.: 23). In a note on the same page, in which he positively referred to Young's (1913) criticism, he wrote:

> So determined is the Professor to banish these old friends that, disturbed by the apparent theoretical incompatibility of pure competition with the prevalence of decreasing cost at all, he seems to hold (p. 192, *Economics of Welfare,* 1920) that each firm is (or would be if it were isolated) working under conditions of increasing costs while the industry as a whole is working under conditions of decreasing costs. I would prefer to offend the mathematical theory of competition than to follow him through this logical hole in his own logical net.

These initial skirmishes in Cambridge, together with the several criticisms of Marshall from Knight, Schumpeter, and others from outside the UK, prepared the ground for the most radical criticism of the period, Piero Sraffa's.

Sraffa's contribution, 1925–6

Sraffa's articles went along with the line of critical rigour in the analysis of the concept of competition but distinguished themselves by a general criticism of Marshall as a neoclassic economist. In other words, Sraffa represented a new and different level of the criticism: the question was no longer the rigorous refinement of Marshallian pure economics but rather discarding that theory. The impact of the 1926 article, published in the *Economic Journal,* on the Marshallianism-permeate Cambridge environment was 'tremendous' (Kahn 1984:23),[15] in comparison to the internationally weaker impact of the previous 1925 Italian article–of which the more concise 1926 English article represented a summary–on the Italian academy. Actually Sraffa came from a country where Marshall's doctrine was well known and widely diffused, but not dominant–'Marshall was not Economics' in Italy–but subject to sometimes strong criticism–above all Pareto's[16]–and where the limits of partial equilibrium analysis were well known.[17] Sraffa opened up a debate that in England had come to a standstill on the problem of classification of the

industries according to the fact that they belong to the categories of increasing, decreasing or constant returns raised by Clapham, holding that the difficulties did not depend on the insufficiency of data or incompetence of scholars, as Pigou had said, but rather on the nature of the criterion of classification itself. The difficulties in classifying the various industries depend on the heterogeneousness of the two laws of increasing and decreasing returns. Such heterogeneousness depends on the fact that those laws (a) originate from different parts of classic theoretical apparatus[18] and (b) are connected, one, the law of decreasing returns, to changes in factor proportions, the other, the law of increasing returns, to changes in the scale of production.

The coordination of the two laws in a single law of variable costs in order to explain the value of competition–which emphasises the functional connection between cost and quantity produced–was a neoclassical operation. But, Sraffa noted, in order to reach this result:

> It was found necessary to introduce certain modifications into the form of the two laws. Very little was necessary as regards the law of diminishing returns, which merely required to be generalised from the particular case of land to every case in which there existed a factor of production of which only a constant quantity was available. The law of increasing returns, however, has to be subjected to a much more radical transformation: the part played in it by the division of labour–now limited to the case of independent subsidiary factories coming into existence as the production of an industry increases–was greatly restricted; while consideration of that greater internal division of labour, which is rendered possible by an increase in the dimensions of an individual firm, was entirely abandoned, as it was seen to be incompatible with competitive conditions. On the other hand, the importance of 'external economies' was more and more emphasised–that is, of the advantage derived by individual producers from the growth, not of their own individual undertakings, but of the industry in its aggregate.
>
> (Sraffa 1926a:537–8)

Hence Sraffa emphasised a solution of continuity between classical and neoclassical economics, at the same time criticising Marshall's interpretation of classics. The following step of Sraffa's criticism was on the analytical difficulties of Marshallian theory:

> The really serious difficulties make their appearance when it is considered to what extent the supply curves based on the laws of returns satisfy the conditions necessary to enable them to be employed in the study of equilibrium value of single commodities produced under competitive conditions.
>
> (Sraffa 1926a:538)

This point of view, Sraffa continued, 'assumes that the conditions of production and the demand for a commodity can be considered, in respect to small variations, as being practically independent, both in regard to each other and in relation to the supply and demand of all other commodities' (ibid.). This assumption is legitimate if a variation in the quantity produced by the industry under consideration has no collateral effects or slight ones. Unfortunately, however, Sraffa said, it so happens that in the great majority of cases the applications of the laws of returns fall into the case in which the collateral effects are such as to upset the conditions of the particular equilibrium which it was intended to isolate.

In the pre-Sraffa phase of the debate, as we saw, the decreasing supply curve was the *punctum dolens*. On this crucial point Sraffa essentially assumed Young and Knight's criticism of Pigouvian external economies, and, in the 1925 article, tried to reconstruct how Marshall himself adopted this notion of external economies. Sraffa recognised that Marshall definitely clarified that the cases in which productivity increases with the increase of firm size 'cannot have a seat in the theory of price under competitive conditions because…if a firm can decrease its costs endlessly increasing its production, it will continue to reduce the selling price until it will have conquered the whole market, thereby violating the assumption of perfect competition' (Sraffa 1986:43). There is a way of escaping from this difficulty and constructing an industry decreasing supply curve 'perfectly correct, at least from the formal point of view' (ibid.): to explain increasing returns by external economies, external to the individual firm but internal to the industry, but:

> Those economies which are external from the point of view of the individual firm, but internal as regards the industry in its aggregate, constitute precisely the class which is most seldom to be met with. As Marshall has said in the work in which he has intended to approach most closely the actual conditions of industry 'the economies of production on a large scale can seldom be allocated exactly to any one industry: they are in great measure attached to groups often large groups, of correlated industries'. In any case, in so far as external economies of the kind in question exist, they are not likely to be called forth by small increases in production.
>
> (Sraffa 1926a:540)

Sraffa did not limit himself to this logical criticism, but wanted to show that Marshall created that particular notion of external economies discarding his originary point of view. Sraffa considered the evolution of Marshall's thought from the *Economics of Industry* (1879) to the *Principles*. First, in the *Economics of Industry,* Sraffa said, Marshall derived the law of increasing returns directly from the division of labour which in turn depends on the firm size, 'therefore assuming as a cause of the cost decreasing a circumstance not compatible

with the assumption of perfect competition, he borders on that error that later, he himself confuted' (Sraffa 1986:44). Second, Sraffa continued, Marshall considered the industry localisation as another assumption necessary in order to have increasing productivity of the type deriving from the advantages of the existence of developed subsidiary industries. Sraffa commented:

> As we can see, in this first formulation the external economies, then considered as a fundamental cause of cost diminishing, may be found only at an early stage and as secondary elements. The fact that their influence depended on the industry's localisation makes clear that they could not be at the basis of a tendency to increase productivity connected exclusively with the increase of production… As regards to the other type of external economies, that is the improvements in methods of production permitted by the increase of the industry size, Marshall excluded that cost decreases dependent on such improvements could be considered as an exclusive effect of the increase of output.
>
> (ibid.)

Sraffa continued:

> When he realised that a cost decrease, depending on the increase of firm size and division of labour, was not compatible with free competition, he abandoned his former point of view and greatly developed the theory of external economies to the point of considering them as the only cause of decreasing costs under competitive conditions. It is in the *Principles of Economics* that his theory appeared in its definite form. The radical change produced by the *Principles* on the laws of cost variations is passed quite unobserved…. He could make it acceptable as a tacit compromise between the requirements of the theory of competition, incompatible with the decrease of individual cost, and the requirement of realism–the real world, being far from perfect competition, presents many cases of decreasing cost of that type.
>
> (ibid.)

Sraffa continued: 'The fact that the "external economies" peculiar to an industry, which make possible the desired conciliation between scientific abstraction and reality, are themselves a purely hypothetical and unreal construction, is something that is often ignored' (ibid.) (see also Sraffa 1926a, p. 540, quoted above).

Sraffa's interpretation of Marshall's theoretical evolution did not seem philologically correct to Becattini (1975) (see also Hart 1996): when Marshall wrote *The Economics of Industry,* it seems that he was already aware of Cournot's problem of incompatibility between increasing returns and competition and had elaborated an explanation of compatibility essentially

through the life cycle of the firm. Probably Sraffa attributed to Marshall a passage that Pigou really wrote, but what we have to stress here is that the passage was *inevitable* in the perspective of a rigorous static analysis.

Sraffa did not criticise only the decreasing cost supply curve but more generally criticised the supply curve based on variable costs under competitive conditions. He emphasised the artificial conditions of constructing an increasing supply curve and concluded: 'The imposing structure of diminishing returns is available only for the study of that minute class of commodities in the production of which the whole of a factor of production is employed' (Sraffa 1926a:539). In this case too, Sraffa's criticism was not new: Sraffa himself, in the 1925 article, wrote that he accepted Barone's conclusions (1894) on the inadmissibility of the increasing supply curve of a commodity for the production of which factors are used which are also employed in other productions. Sraffa concluded that the conditions, which a supply curve must satisfy in partial equilibrium and under competitive conditions, make the laws of variable costs applicable in a very limited way: 'Reduced within such restricted limits, the supply schedule with variable costs cannot claim to be a general conception applicable to normal industries; it can prove a useful instrument only in regard to such exceptional industries as can reasonably satisfy its conditions' (1926a:540). In his 1925 article he wrote:

> There are strong reasons…for saying that, in a static system of perfect competition, in the determination of the particular equilibria of commodities, non-proportional cost curves cannot be traced, if not in exceptional cases, without introducing hypotheses which are contrary to the nature of the system. The necessary condition is to perfectly isolate the industry producing a given commodity from all other industries: in the case of the increasing costs, it occurs to take in account the whole group of industries which employ a given factor of production; in the case of decreasing costs, it occurs to consider the whole group of industries which take advantage of certain external economies. These causes of variation of costs, of very great importance from the point of view of general economic equilibrium, must be considered unimportant in the study of the particular equilibrium of an industry.
>
> (Sraffa 1986:45)

Hence the supply schedule with variable costs is not a general conception. More general, as a first approximation, it seems to be the Ricardian constant cost hypothesis, according to which the cost of production of commodities produced competitively 'must be regarded as constant in respect of small variations in the quantity produced' (Sraffa 1926a:541).[19] This is the outcome of the logical difficulties met by variable costs and it was stressed by Sraffa in a letter to Keynes (Milan, 6 June 1926): 'Ricardo's assumption is the best available for a simple theory of competition (viz. A first approximation)', but,

he continues, 'of course in reality the connection between cost and quantity produced is obvious', but 'simply cannot be considered by means of the system of particular equilibria for single commodities in a regime of competition devised by Marshall.' The routes available were (a) the simultaneous equilibrium of all industries (Pareto's point of view), or (b) the abandon of assumption of perfect competition. According to Sraffa the Paretian conception was not fruitful because of its complexity (a judgement shared with many Italian economists, Pantaleoni for example), therefore Cournot's route seemed to be the only viable research direction: it is a further approximation, which permits consideration of the increasing returns case.

Robbins' attack of the representative firm, 1928

'The Marshallian conception of a Representative Firm has always been a somewhat unsubstantial notion' (Robbins 1928:387). So opened in the September 1928 issue of the *Economic Journal* an article written by a young exponent of the London School of Economics, Lionel Robbins, devoted to the criticism of Marshall's representative firm (see also Silberling (1924), who in United States had already criticised the concept as misleading and superfluous). This article, although it contained a superficial interpretation of Marshall's notion, was important from an historical point of view in the cracking of Marshallianism and because the Marshallians in their defence of Marshall considered essentially Sraffa's and Robbins' papers. It is also interesting to note that Keynes was 'in sympathy' with this paper: in a letter to Robbins of 14 March 1928, in which he comunicates to Robbins the decision to accept his paper for publication in the *Economic Journal,* he writes:

> It is a very interesting piece of work, which much wanted doing, and for my part I am in symphaty with it. I should like to do away with the representative firm altogether, and I believe you are right in arguing that it really serves no useful purpose.
>
> (*Keynes Papers,* EJ/1/3)[20]

In his article Robbins examined the places in which Marshall used the concept, from which his most relevant results were:

- The representative firm is essentially a long-period conception with neither statistical significance nor practical usefulness.
- The representative firm is not a necessary tool: 'There is no more need for us to assume a representative firm or representative producer, than there is for us to assume a representative piece of land, a representative machine, or a representative worker. All that is necessary for equilibrium to prevail is that each factor shall get at least as much in one line of production as it could get in any other' (Robbins 1928:393).

- The representative firm is inessential to the hypothesis of stationariness as well as to the hypothesis of static equilibrium (which Marshall rejected because of 'his curious predilection for biological analogies' or 'for fear of becoming unintelligible to business men and economic historians' (ibid.: 395)). This is true both in the case of general equilibrium and of partial equilibrium.
- The representative firm is not necessary also in the case of diminishing costs under competitive conditions. He questioned the existence of external economies' referring to the criticisms of Young and Knight (ibid.: 398–9).
- Finally, the representative firm is a very poor tool for examining the problems of change and development. In a note, referring to the contemporary research programme of Allyn Young,[21] at that time at the London School of Economics, he said that:

> In a world in which growth in the economic system proceeds just as much by way of differentiation and subdivision as by the expansion and development of particular economic units, the idea of a representative unit which preserves its essential identity while undergoing progressive expansion is apt to be very misleading.... In such a case to continue to speak of *the* representative firm of the industry...is to suggest a state of affairs having no counterpart in reality... It is no accident, I suggest, that in *Industry and Trade* where problems of this sort are dealt with, the use of the Representative Firm is even more nebulous and half-hearted than in the *Principles*.
>
> (Robbins 1928:402–3 note)

The Marshallian defence: Pigou, Robertson and Shove, 1927–1930

Pigou's definite statement of the theory of competitive supply, 1927–8

A few months after the publication of Sraffa's article, in the June 1927 issue of the *Economic Journal,* Pigou replied, saying that he had considered it a 'very interesting paper'. He discussed 'analytically' the relation between the quantity of output and the costs of production of particular commodities in the long period, that is, in Marshall's language, he was concerned with the normal relation between output and cost and 'exclusively with variations in aggregate cost associated with and due to variations in the scale of output' (Pigou 1927:189) of the particular commodity–no inventions or other relevant changes are taken into account. Moreover he assumed that the relative value of the factors of production remains constant: this assumption is due to the fact that 'when changes in the relative value of factors of production are liable to occur in consequence of changes in the scale of production of an industry, it is not possible to assign a clear meaning to costs,

and therefore, is not possible to construct a costs function' (ibid.: 192), with the consequence that Pigou was forced to confine his study 'to commodities which individually employ so small a proportion of each of the several factors of production that no practicable changes in the scale of their output could sensibly affect the relative values of these factors' (ibid.). His study concluded that:

- with the class of commodity under review 'it is impossible for production anywhere to take place under conditions of increasing costs': 'In this matter my conclusion agrees with that by Professor Sraffa in his recent article' (ibid.: 193).
- On the contrary, Pigou maintaned that production could take place under conditions of decreasing costs, in opposition to Sraffa's judgement that it was impossible or extremely unlikely. He accepted that under competitive conditions internal economies cannot be considered, but he asked 'whether he [Sraffa] is right in denying that external economies special to particular industries may be looked for in a measure adequate to establish conditions of decreasing costs' (ibid.: 195). According to Sraffa' it was highly improbable that a small increase in the scale of output of a single industry will lead to a growth of external economies sufficient appreciably to affect costs in that industry. This is apparently true, Pigou said, but really illusory, in fact:

> Nobody, of course, imagines that a small addition to the scale of output will lead to more than a small increase of external economies. What signifies, however, is not the absolute size of this increase. It is the ratio between this increase, expressed as a proportion of previously existing costs, and the increase of output, expressed as a portion of previously existing output; and there is no reason why the ratio between two quantities which are both of the second order should not itself be of the first order.
>
> (Pigou 1927:195)

So Pigou refused Sraffa's criticism to external economies from the analytical point of view; but, he said 'to determine the actual content of any part of the cost function for any commodity would necessitate a very difficult combination of statistical research and intelligent guess-work' (ibid.: 196). The definitive statement of Pigou's theory of competitive supply came one year later in his June 1928 article in the *Economic Journal,* substantially incorporated in the third edition of the *Economic of Welfare*. In the paper Pigou concluded his study of the relation 'between variations in normal supply price and variations in quantity of output' (Pigou 1928:238) under 'other things being equal' and competitive conditions–that is the output of the individual firms are small relative to the output of the whole industry and free entry–in a rigorous and highly abstract manner in order to give precise

form to Marshall's discussion of internal and external economies. To do this he considered it necessary to renounce Marshall's representative firm and its complexity:

> Most industries are made up of a number of firms, of which at any moment some are expanding, while others are declining. Marshall… likens them to trees in a forest. Thus, even when the conditions of demand are constant and the output of an industry as a whole is correspondingly constant, the output of many individual firms will not be constant. The industry as a whole will be in a state of equilibrium; the tendencies to expand and contract on the part of the individual firms will cancel out; but it is certain that many individual firms will not themselves be in equilibrium and possible that none will be…. This is evidently a state of things the direct study of which would be highly complicated. Fortunately, however, there is a way round.
>
> (Pigou 1928:239)

The device introduced in order to reduce the analytical complexity is the concept of 'equilibrium firm', of which he studied the conditions of equilibrium:

> There can exist some one firm, which, whenever the industry as a whole is in equilibrium, in the sense that it is producing a regular output y in response to a normal supply price p, will itself also individually be in equilibrium with a regular output x.
>
> (ibid.: 240)

The equilibrium firm cannot have unexhausted internal economies, and the possibility that supply price might fall as industry output increases can be attributed only to external economies which lower the average cost curve of the equilibrium firm as industry output increases. Pigou used in this article the concept of external-internal economies–so defined by Robertson (1930)–whose nature is such that they emerge as a result of those increases in the size of the individual firm which are directly caused by increases in the size of the industry as a whole, a device of reconciling the contradiction between increasing returns and competition:

> The increased specialisation of its component firms made possible by an enlargement in an industry as a whole often involves a large reduction in costs…. An increase in the scale to which an industry is producing frequently alters–in general diminishes–the average and (marginal) costs of the equilibrium firm contained in it, whether or not it also alters its size. There is then no difficulty in seeing that the law of decreasing supply

> price, as conceived when correction has been made for transfer elements in rates of price change, is not merely possible, but is likely to be followed in practice by many manufacturing industries.
>
> (ibid.: 252)

Robertson (1930) recognised as ingenious Pigou's concept of externalinternal economies, but 'that it is not the line of approach which springs most naturally either out of Marshall's suggestions or out of the observed facts' (Robertson 1930:86–7) because, although 'it is doubtless true, as Marshall and Pigou observe, that the growth of an industry and the growth of its constituent firms frequently proceed more or less *pari passu*' (ibid.: 87), the bulk of the observed internal economies of large-scale production cannot have 'the derivative nature' of Pigouvian supposition. The reconciliation between increasing returns and competition (by endowing each firm with a rising marginal cost curve while allowing firm's marginal and average cost curves to shift as industry output changed), represented a view which became normally accepted after Viner (1931).[22] However, a crucial implication of the Pigou-Viner approach, which Viner (but not Pigou) recognised clearly, is the absence of realism and empirical relevance of their analysis:

> No attempt is made here at realistic description of the actual types of relationship between costs and supply, and purpose is the more modest one of presenting the formal types of relationship which can be conceived to exist under certain simplifying assumptions.
>
> (Viner 1931:23)

From Sraffa's point of view, of course, this defence was worthless, the break between theory and its practical relevance not being reassembled.[23]

Robertson defence and the 'destructive' criticism of Sraffa, 1930

Robertson (1930) tried another defence he hoped able to preserve the 'nature' of Marshall's approach. In a Symposium organised by Keynes and published on the *Economic Journal,* Robertson, in his paper, maintained against Robbins that the representative firm is 'a fruitful and indeed an indispensable instrument in the construction of a theory of value' (Robertson 1930:87). The question is whether the fact 'the scramble by individual firms, regardless of the actions of their neighbours, to reap the direct advantages of large-scale organisation and plant' (ibid.) in the process towards equilibrium, 'can or cannot be played upon by the mind with any success without abandoning the theory of competition' (ibid.). Robertson thought that Marshall 'while not rejecting Professor's Pigou external economies and Mr. Sraffa's monopoly theory as auxiliary weapons, held that it could' (ibid.). The metaphor of the

trees of the forest, Robertson continued, meant 'to assist the reader in making a more violent effort of the imagination than most of those who have quoted it have realised' (ibid.). The 'not easy' task of showing that '[competitive] equilibrium might be legitimately conceived with the representative firm working under conditions of decreasing cost, with price equal to the average costs of that firm, and with the industry as a whole obeying the law of increasing returns' (ibid.: 88) is pursued introducing the metaphor of Messrs Smith and Robinson's firm: it is a representative firm at a certain moment–with all the properties belonging to the Representative Firm but not the 'quality of being able to expand output indefinitely at a lower cost per unit' (ibid.), a quality which does not belong 'to no firm whose name is to be found in the directory' (ibid.). As a consequence 'the fact that "*the* Representative Firm" is to be conceived of as working under conditions of decreasing cost proves not incompatible with the fact that Messrs. Smith and Robinson will never obtain a monopoly of the whole trade' (ibid.: 89). Robertson used also another metaphor, comparing Messrs Smith and Robinson to 'a collection of water-drops at this moment forming part of a wave, and sharing all the obvious physical properties of the wave…but not its continuity of existence with the wave of five minutes later' (ibid.: 88). This position–Robertson recognised this point–'cannot be cleared up mathematically' (ibid.: 89), but he considered it the best way at that time available to throw light 'on the turmoil of what happens in real life' (ibid.).

Sraffa's 'criticism' of Robertson's paper was, according to Keynes and most commentators, destructive.[24] He commented as follows:

- 'if there is no equilibrium, it is not denied that internal economies may be the main force in operation…; it is only denied that *in a state of equilibrium* they can be…. Now, if Mr. Robertson thinks that internal economies are "the main factor" in the "progress towards equilibrium", how can he at the same time hold that they go on acting undisturbed beyond that point?' (Sraffa 1930a:90);
- as regards to the metaphors used by Robertson, 'I cannot see how it helps to reconcile the contradiction' (ibid.: 91):

> The argument…remains the same: so do the objections. When individual forms retained their identity throughout the discussion, the question which Mr. Robertson had to answer was; 'If firms could increase their output and thereby reduce their costs, why didn't they increase it before the expansion of the industry?' Now that firms lose their identity, the question to be answered is: "If the new firms can turn out a larger output at a lower cost that the old firms, why didn't they come into existence before? Why in the new, and not in the old position of equilibrium?'
>
> (Sraffa 1930a:91–2)

- 'We seem to agree that [Marshall's] theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconcile it with the facts it sets out to explain. Mr. Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that, in the circumstances, I think it is Marshall's theory that should be discarded'.

  (ibid.: 93)

In the light of Sraffa's criticism, Robertson's paper appears to be a perfect example of the contradiction between the will of examining a dynamic question and the actual use of static tools to tract it.

Shove's contribution, 1930

In his paper presented at the *Symposium,* Gerald F.Shove, a brilliant member of the Cambridge school (see Collard 1990; Harcourt 1991), wanted to present elements in order to construct a theory of competitive equilibrium able to take into account the demands made by Robertson without using the notion of representative firm. He wrote that:

> Equilibrium for the industry as a whole does not imply that all (or indeed any) of the individual firms are in equilibrium: every one may be either expanding or contracting, provided that the rates at which the output of the growing firms is expanding and that of the declining firms contracting are such as to leave the aggregate output unchanged.... What is necessary for equilibrium is that a general expansion or contraction in the scale of the business unit should not be profitable.
>
> (Shove 1930:96)

He considered first the question: how can the existence of internal economies be reconciled with competitive equilibrium?

> The internal economies here in question must obviously be internal economies of individual expansion, i.e. improvements in its internal organisation which a firm would obtain if it had a larger share in a constant aggregate output.... The answer which most readily occurs to the mind is that these internal economies of individual expansion are offset by equivalent diseconomies.... The obstacles which check the growth of a firm's share in a trade...are the increases in the cost of transport and of marketing..., which a firm is liable to encounter as it advances further into its competitors' territory or markets.
>
> (ibid.: 105)

Shove continues: 'This simple reflection at once enables us to reconcile competitive equilibrium with increasing returns or diminishing supply price arising from internal economies alone', without referring to external economies, a 'secondary line od escape' (ibid.: 107). This solution, Shove recognised, may meet two objections from Sraffa and Robertson:

- 'Sraffa will say, perhaps, that the equilibrium reached under these conditions is not competitive but monopolistic' (ibid.: 108–9);
- 'Robertson may complain that… I am evading his real difficulty–which only arises when the economies of individual expansion pre-dominate over its diseconomies' (ibid.: 109).

Shove answered Sraffa's possible objection by saying that, although behind it there may be a real issue–the question whether in the given situation value approximates to cost of production or departs from it widely–'we are not concerned with that problem here' (ibid.). On the contrary, 'we have simply to enquire whether, and if so why, substantial economies of mass-production are consistent with the survival of a large number of competing firms' (ibid.). To answer Robertson's question, he maintains that 'the solution of this problem turns, as Marshall saw, on the element of time' (ibid.): 'If a firm could enlarge its output to any required size… instantaneously…then indeed the predominance of internal economies would, on our present hypothesis, be incompatible with competitive equilibrium; but since it cannot, the two conditions can be reconciled' (ibid.: 110). In other terms, Shove stressed the fact that the firm's costs are not a function of only two variables (firm's and industry's output), as in Pigou (1928), but the function of three variables (firm's and industry's output and *time*): it is the Marshallian life cycle of the firms which contributes in an essential way to explain to co-existence of a large number of firms. What Shove proposed was an analysis of dynamic equilibrium, in a Marshallian mood, but discarding the representative firm. As Newman (1960) wrote:

> He saw very clearly…that equilibrium of the Marshallian type implied that the size distribution of output among firms should be constant and, further, that increases in the industry's output might lead to 'quite a different distribution of business between the size-groups'.
>
> (Newman 1960:598)

This statement was made again twenty-five years later by Wolfe (1954): in his article, inspired by Robertson and Shove's contributions,[25] he suggested that the representative firm should be treated as an 'abbreviated notation' for the distribution of firms by size, thus making the result of a process of chance. This concept of equilibrium can be represented mathematically, by employing, as Newman and Wolfe (1961) do, the

technique of non-homogeneous Markov chains. The rehabilitation of Marshall's life cycle theory by Negishi (1989) can be also considered a variation of the same theme of Newman and Wolfe. In any case, at that time, Shove's contribution remained isolated, as no one followed it up.

Young, 1928, and Schumpeter, 1928: the return to the classics' dynamics

Dynamics was at the heart of Young's (1928) article. Allyn Young was a fundamental figure in the theoretical reflection of the period 1910–28. As Schumpeter (1954) rightly wrote: 'he was among the first to understand the stage of transition that economic analysis entered upon after 1900' (Schumpeter 1954:875–6, note). Actually, as far as we are concerned, Young may be considered one of the economists who started the criticism of Marshall, with his critical review of Pigou's *Wealth and Welfare,* and a sort of critical conscience behind some of major theoretical works of that time, from Knight to Chamberlin. In the academic teaching years spent at Harvard between 1920 and 1927, Allyn A.Young developed an increasing disaffection and scepticism with regards to the model of static equilibrium and its capability in terms of description and prediction (see Blitch 1983a and b). In a letter of December 1922 to Knight, Young wrote: 'I have yet to see that the method of general equilibrium gives us anything at all that gets us anywhere' (9 December 1922, cited in Blitch 1983a:363). The solution of the problem of increasing returns and competition was not, according to Young, the theory of monopoly, but a revision of the external economies concept after abandoning the static method. In the autumn 1928 he wrote to Knight:

> The static view does not interest me very much, because, if it is rigorously adhered to, almost everything worth saying about it can be put onto in a very few pages. We have to depart from it somehow. The only question is just how.
>
> (cited in Blitch 1983a:364)

He could express his position in his speech in September 1928 as the president of Section F of the British Association at the University of Glasgow, three months after it was published in the *Economic Journal* with the title Increasing returns and Economic Progress'.[26] In it external economies were considered as the prime source of increasing returns and economic progress. The question of increasing returns is considered as a part of a theory of the industrial growth, in the perspective of Book IV of the *Principles*. The starting point of the article is the statement that the partial equilibrium apparatus which economists have built up for dealing with the range of questions raised by the phenomena of increasing returns permits examination only of some aspects of it. Certain aspects of those processes are obscured in the partial

analysis: in fact, Young said against Knight, the internal economies of some firms do not account for all the external economies. If, as with traditional apparatus, we assume a condition of comparative stability looking at the internal economies of a particular firm, those changes escape another order which occurs: the appearance of new products and new industries, the assumption of new tasks by the firms, that is the change which is not only quantitative but also qualitative:

> No analysis of the forces making for economic equilibrium, forces which we might say are tangential at any moment of time, will serve to illuminate this field, for movements away from equilibrium, departures from previous trends, are characteristic of it.
>
> (Young 1928:528)

Another view, 'simpler and more inclusive', had to be assumed: 'such as some of the older economists took when they contrasted the increasing returns which they thought were characteristic of the manufacturing industry taken as a whole with the diminishing returns which they thought were dominant in agriculture' (ibid.); in particular, Adam Smith's theorem that the division of labour depends upon the extent of the market. The subjects of Young's paper were two related aspects of the division of labour: the growth of indirect methods of production and the division of labour among industries, which he discussed by developing ideas already present in Book IV of the *Principles*. Young stressed that the size of the market is the most important single factor in determining the effectiveness of the industry and defines in a classical vein the market as 'an aggregate of productive activities, tied together by trade' (ibid.: 533) between which 'must be some sort of balance'. Moreover, Young emphasised that:

> the counter forces which are continually defeating the forces which make for economic equilibrium are more pervasive and more deeply rooted in the constitution of the modern economic system than we commonly realise.... Every important advance in the organisation of production...alters the conditions of industrial activity and initiates responses elsewhere in the industrial structure which in turn have a further unsettling effect. Thus change becomes progressive and propagates itself in a cumulative way.
>
> (ibid.: 533)

We have to stress a strong analogy between Young's analysis and Schumpeter's (1912 and 1928). In his 1928 paper, Schumpeter, after defining as 'inadequate, or even misleading' the traditional view of industrial progress inside an essentially static structure, maintains that change is the fundamental characteristic of capitalist economic progress. Consequently, expansion

cannot adequately be dealt with by static analysis at all: 'Expansion… is itself the result of a more fundamental economic force, which accounts both for expansion and the string of consequences emanating from it' (Schumpeter 1928:376). As it is well known, according to Schumpeter, a fundamental force is innovation, a specific function of the entrepreneur: this statement was the starting point of the contemporary theory of innovation and structural change. Also Young's view will be theoretically fertile: it is traceable, among others, in Stigler (1951), Downie (1958), Richardson (1975), and partly in Roemer (1986 and 1987).

The reading of Young and Shove's papers reveals in them several Marshall's ideas, However, we should not forget that the theoretical framework (and its epistemological nature) which forms the basis of these papers is different from Marshall's: in fact the clear separation between statics and dynamics seems to be the implicit assumption of our authors.

4 Concluding remarks

In Book IV of the *Principles* Marshall offered a dynamic view of the competitive process and the growth of the firm strictly connected with the classical view of Smith and Marx. Using the Smithian concept of division of labour as the starting point of his analysis, Marshall presented a model in which the division of labour produces increasing returns and tends to increase the scale of production and the opportunities for further division of labour; the exploitation of internal economies makes it possible for a firm to increase its size but this tendency does not transform the competitive system into a monopolistic one because the possibility of exploiting increasing return is limited by opposing tendencies: the life cycle of the firm and the difficulties of marketing, while external economies play the role of facilitating the general diffusion of the economies of production. Marshall's draft model is one of dynamic monopolistic competition–that is, the monopoly positions of firms can be only temporary because of the dynamic nature of the capitalistic system which does not allow the process to reach an end point–which makes Cournot's dilemma on increasing returns and competition unimportant. Marshall felt the need–a need of an epistemological nature; abstract economics without the study of facts is a 'calamitous notion' according to Marshall–of taking into account those dynamic components into the theory of value exposed in Book V, a theory which was developed essentially on the basis of marginal concepts. Therefore, an original mixture of classic and marginal motives in a fundamentally marginal framework emerged from Marshall's attempt. In it, in order to consider Cournot's dilemma, he created the concept of the representative firm. The analytical result, however, was not only unsatisfying but also inadequate to solve his problem of 'realism' of the theory, and was so resulted theoretically weak. If it is undoubted that there is an epistemological difference between Marshall and the economists who discussed

and criticised his theory in the 1920s, it is also certain that their criticism was permitted by the fragile analytical translation of Marshall's methodological requirement. The scarce solidity of the foundations of Marshall's theory of value was early recognised: at first, as Sraffa stressed in his 1926 article, qualifications, restrictions and exceptions were 'scattered about in footnotes and articles and carefully segregated from one another', then, progressively, mainly in the 1920s, the criticism emerged openly and widely. He emphasised two types of limit in Marshall's theory of value: of relevance and of analysis. Regarding the first, it was the supply curve based upon the laws of increasing and diminishing returns that sat in the dock. Under conditions of stable partial equilibrium, it was noted, the shape of the supply curve cannot be decreasing, unless it is introduced by the hypothesis of external economies in a precise sense–external to the firm, but internal to the industry–in order to guarantee the *ceteris paribus* condition. Moreover, under those conditions of partial equilibrium the shape of the supply curve can be increasing only if the totality of the factor of production is used in order to produce the commodity under examination. Last, but not least, some writers deny the theoretical validity of the long-period descending curve, holding that such a curve can only represent an irreversible, historical process, therefore that it is a descriptive curve. Regarding the second type of limit, the relevance of the Marshall construction, it was noted that the representative firm was not a necessary tool for the determination of equilibrium and, above all, it was a poor tool in terms of examining the problem of change and development. In fact in conditions of stationary state *and* partial equilibrium the broad forces of increasing returns–by their nature inter-industrial–have a very limited action, as well as the major limitation to the un-exhausted growth of the firm, the mortality of entrepreneurial ability, because under those assumptions the available resources do not change. Finally, the representative firm describes the characteristic of the equilibrium situation, but not the path to equilibrium and whether the process converges towards equilibrium.

To overcome the difficulties Marshall faced and to make the theoretical difficulties clear it seemed necessary, as Schumpeter first, and Knight then, saw, to separate static from dynamics. But this solution was unacceptable to the Marshallians, as it can be seen if we consider Pigou's work of reconstruction and defence of the tradition. Pigou, since the years of *Wealth and Welfare* and until to the end of the 1920s, tried to remedy the breaches opened in Marshall's theory. He accepted to play in the same field as that of the critiques, that is he assumed the autonomy of abstract reasoning and the necessity of absolute rigour of the theory of value–implicitly abandoning Marshall's epistemology–so trying to defend Marshall's construction without any reference to the element of time, as he himself wrote. He undoubtedly offered a logically consistent solution inside the framework of marginal analysis, but a weak solution because: (a) he pretended to preserve, in part at least, some of Marshall's realism; (b) at the same time, he made assumptions,

necessary from an analytical point of view, the non-realism of which was revealed first by Young back in 1913. Pigou's work fluctuated between the rigour requirement and the realism requirement, searching without success for a bridge between them.

We have to stress that the non-realism of the hypothesis is not a serious limit in Schumpeter's and Knight's perspective, which separate statics and dynamics. From that point of view the non-realism of the assumption, conceived as a necessity dictated by the requirements of rigour and statics, has a specific task: 'There is nothing unduly abstract in considering the phenomena incident to the running of economic life under given conditions taken by themselves. On the contrary it means giving this class of problems the treatment they require' (1928:368). It may be also a simple pedagogic instrument, as Viner (1931) reduced the theory of firm and industry. According to these authors, dynamics is the field in which changes and instability, the phenomena which characterised capitalism, have to be considered: 'they cannot be adequately dealt with by static analysis at all', Schumpeter said, and similar statements were made by Young, Knight and Robbins. In this sense, Schumpeter and Young, the authors who followed the research path of dynamics, recognised and accepted the practical irrelevance of the theory of value as inevitable. Moreover, they considered it wrong to examine discontinuous change in a static framework, because this implies considering economic life as in itself essentially passive, while according to them it is essentially dynamic and unstable. On the contrary, we have to say that this 'degrading' of static and stationary analysis, which makes the theory of value more or less a pedagogic instrument, is an undesired result in Pigou's perspective, unacceptable in the Marshall-Pigou research programme–a situation of no way out for the Marshallians, with the partial exception of Shove's 1930 article which represented a constructive but neglected contribution.

What was the role of Sraffa's article in the controversy on costs? Sraffa touched on a sore point of Marshallism–the problem of the co-existence of logical consistency and practical relevance of Marshall's and Pigou's theory of value–with an extraordinary acuteness. He adopted 'qualifications, restrictions and exceptions' previously expressed on Marshall's theory of value and fused them in a really anti-neoclassical criticism. Sraffa shared with Knight and Schumpeter a strong stress on the requirement of rigour–and he considered it useful in order to understand Marshall's difficulties, the reference to the typically Marshallian mixture of static and dynamic components which those authors attack–but he could not share the conclusive judgement of practical irrelevance of the theory of value in general. Sraffa did not accept 'the fact that this theory…has lost much of its direct bearing upon practical politics, and particularly in regard to doctrines of social changes, which had formerly been conferred upon Ricardo and afterwards by Marx, and in opposition to them by the bourgeois economists' (1928:535), deprecated (quoting from Keynes) the

fact that 'it has been transformed more and more into "an apparatus of the mind, a technique of thinking" which does not furnish any "settled conclusions immediately applicable to policy"' and that 'it is essentially a pedagogic instrument' with the purpose of training the mind, 'hardly apt to excite the passions of men' (Sraffa 1926a:535–6). The necessary theoretical rigour, for Sraffa, did not mean a sentence of practical irrelevance of the theory at all, but simply it demonstrated the irrelevance of Marshall's theory of value based on 'the assumption that the essential causes determining the price of particular commodities may be simplified and grouped together so as to be represented by a pair of intersecting curves of collective demand and supply' (ibid.: 535): this irrelevance was due to the theoretically weak foundations of that portion of the modern theory of value represented by the curve of supply, a weakness which he recognised in the heterogeneousness of the laws of increasing and decreasing returns on which it is based and in the limits of their use in the partial equilibrium structure. In 1930, in his comments of Robertson's paper, the practical irrelevance was explicitly considered a result of Marshall's neoclassical programme and for this reason Marshall's theory was to be discarded: 'if Mr. Robertson regards them [the assumptions implicit in Marshall's theory] as extremely unreal, I sympathise with him', Sraffa wrote. He did not want to 'discard the facts', rather Marshall's theory, inadequate to keep together theory and practical relevance. Hence, according to Sraffa, the analytical problem of the theory of value is not separated by that of its practical relevance. In 1925–6, from the analytical point of view, he considered the two paths of research which at that time seemed possible: that of general economic equilibrium, discarded apparently because too complex, and that of the theory of monopoly (the Cournotian path), which seemed to him practicable and fertile, even in the context of the partial analysis, a path then followed by most Marshallians in the 1930s. But this second best solution was early considered unsatisfying by Sraffa probably for theoretical and analytical reasons as well as from the point of view of practical relevance. In this sense, the 1925–6 articles, from which the *differentia specifica* of Sraffa's contribution clearly seems to result, represent an intermediate step of a programme of research of which the successive step was not clear at that time, but would become clear a couple of years later, when he had laid down a draft of the opening propositions of *Production of Commodities by Means of Commodities*.

Notes

1 I would like to thank the librarians and the staff of the following institutions: Wren Library, Trinity College, Cambridge; Modern Archives, King's College, Cambridge; Marshall library, Cambridge. Special thanks to the Provost and Scholars of King's College, Cambridge, for permission to quote from the unpublished writings of J.M.Keynes.

2 Stigler (1957) wrote: '[Marshall's] treatment of competition was much closer to Adam Smith's than to that of his contemporaries. Indeed, Marshall's exposition was almost as informal and unsystematic as Smith's in this area…. Soon he yielded a bit to the trend towards refinement of the concept. Beginning with the third (1895) edition, he explicitly introduced the horizontal demand curve for the individual form as the normal case and gave it the same mathematical formulation as did Cournot. But these were patchwork revisions, and they were not carried over into the many passages where looser concept of competition had been employed' (Stigler 1957:251–3). Recently Loasby (1989) maintained that 'much of what is in Marshall is far more clearly revealed if we approach him from Adam Smith rather than from modern microeconomics' (Loasby 1989:48). See also Marchionatti (1992:576–80).

3 As many authors have recognised (Maxwell 1958; Guillebaud, see introduction to Marshall 1961) while the changes in the different editions are important, 'the fact remains that Marshall's scheme of thought was virtually complete by 1890, which, in turn, implies that his views on competitive business were largely determined by the impressions he had gathered of British industry in the 1870s and 1880s during his visits to firms' (Maxwell 1958:675). Becattini (1975) showed that the fundamental ideas were, *in nuce* at least, already in *The Economics of Industry* (1879). See also Hart (1966).

4 In the process of growth of firms and industries the external economies also act as a contrasting force to monopolisation: they make the diffusion of technological change and knowledge easier (see Jenner 1964; Marchionatti 1992).

5 As Robbins (1928) said, 'if we like we may regard it as an average firm, but we must regard it as *an average which would only emerge arithmetically under conditions when all present tendencies to change had reached a state of equilibrium*' (Robbins 1928:390).

6 On Marshall's method see Becattini (1961) and (1990).

7 It is fair to say that Marshall was dissatisfied with his solution of Cournot's dilemma throught the representative firm device (see Appendix H).

8 '[Marshall's] marvelous comprehension both of purely analytic and of "realistic" aspects resulted in an exposition that seemed to leave many loose ends about and certaily left plenty of problems for his successors…. He insisted on including internal and external economies in his industrial supply schedule–I suppose in order to make these more realistic' (1954:1047).

9 Pigou's *Wealth and Welfare,* out of which the successive *Economics of Welfare* grew, was a major work on welfare economics at that time. As Myint (1965 [1948]) wrote: 'Prof. Pigou's *Economics of Welfare* occupies a unique position in the history of economic thought It is the culmination of the great neo-classical tradition; and yet at the same time it marks a departure from it. For in his attempt to systematise his predecessors' concrete ad hoc approach to welfare economic problems Prof. Pigou has arrived at a concept of the general optimum; and this concept of the optimum, represents a significant intermingling of the concrete particular approach of the English economists and the formal general approach of the continental economists' (Myint 1965 [1948]:173).

Pigou's books are constructed round the concept of the economic welfare of the community and the size and distribution of its national dividend, a concept which represented, according to Pigou, the central contribution of Marshall's *Principles* (see Pigou 1907). One of the questions raised by Marshall in the *Principles* was the problem of the possibility and significance of divergences between the private and social product: in Book V, chapter XIII, Marshall analysed the relationship of competition to optimum economic organisation and found that the competitive

results can depart from the criteria of optimum because of external economies and diseconomies. Pigou elaborated the importance of this source of disharmonies in *Wealth and Welfare* and then in *The Economics of Welfare* and explored the possibility of using taxes and subsidies to regulate the outputs.

10 Young's review contained also a famous criticism. Pigou had argued that in competitive industries subject to increasing costs marginal social cost exceeded marginal private cost in equilibrium, while the opposite was true in industries subject to decreasing costs: to avoid the misallocation of resource and permit the equality of the two marginal costs Pigou advocated a tax on the output of increasing cost industries and a subsidy to the others. But, Young emphasised, in increasing cost industries the rise of costs depends on the increase in the prices of specialised factors as output rises and so represents just a transfer of purchasing power from those industries using the specialised inputs to the owners of those factors.

11 Moore's 'Paradoxes of Competition' (1906) is considered 'the first article on the formal definition of competition' (Stigler 1957; see also Schumpeter 1954). In it Moore complained of the 'bewildering vagueness of a fundamental term', that of competition, and asked: 'In what respect is the idea of competition changed when the modifiers "perfect", "unlimited", "indefinite", "free", "pure", are added? If by these additions there is a change in the term, then, in cases in which the state of industry admits only of competition what is the nature of the limitation of the applicability of propositions deduced under the hypothesis of perfect competition? The almost invariable answer to this last question is that the imperfection of competition is simply a form of friction, producing, for the most part, a negligible variation from the standards that prevail in a régime of perfect competition' (Moore 1906:211).

Moore went on to present with some precision the conditions of perfect competition, emphasising the requirement of large numbers of competitors, and that 'the term competition undergoes a change of meaning accordingly as competition is between many or a few competitors'. He pointed out, following Cournot, the impossibility of increasing returns under conditions of competitive equilibrium.

12 A colleague of Knight at Chicago, Jacob Viner, also contributed to developing a rigorous model of perfect competition along similar lines: his work was largely the refinement of diagrammatic tools for elucidating and extending 'the traditional Marshallian pattern of assumptions' with respect to variations of cost conditions of the firm. But he published his results later (1931).

13 The reference is to J.B.Clark (1899).

14 These ideas were not completely new in the US at that time. Already Moore had noted 'actual industry–which, to a large extent, is in a state intermediate between perfect monopoly and perfect competition' (Moore 1906:215). Then J.M.Clark (1923) emphasised that in the contemporary American economic world the majority of markets lay in the intermediate zone between theoretical competition and theoretical monopoly. Viner (1921) anticipated some of the essential ideas underlying the theory of imperfect competion. A few years after in 1927, Chamberlin, in his dissertation at Harvard (under the supervision of Young), laid the foundation of *The Theory of Monopolistic Competition*.

15 In a letter to Sraffa, 25 January 1927, Keynes wrote: 'Dear Sraffa, your article in the December Journal has been very much liked over here. Everyone I have spoken to agrees that it puts you in the front rank of the younger economists. Pigou is extremely interested, and has been looking up your Italian article. You may be interested to know that he feels he must, in the light of it, reconsider his whole position' (*Keynes Papers,* L/S/25, Kings College, Cambridge). The impact of Sraffa's lessons at the University of Cambridge in the academic years 1928–9 to

1950–1 on the young generation of Cambridge economists was also relevant (see *Robinson Papers,* CEP I:vii).

16 Pareto, although sharing some of Marshall's methodological conceptions against Walras (see Zanni 1991–2; Marchionatti and Gambino 1997), was a strong critic of his theory of economic equilibrium: 'Marshall has not yet succeeded in understanding economic equilibrium'.

17 Sraffa was trained in the Torino University's environment where Marshallian teachings were widely diffused (see Faucci 1986; Gallegati 1990). Marshall theory was introduced in Italy after 1889 through Pantaleoni's work and 'thanks to Pantaleoni, Marshall became the most authoritative and popular representative of the new school [of economics] in Italy' (Gallegati 1990:136), the diffusion of which reached its peak before the beginning of the 1920s. Two distinct centres of influence that housed the major exponents of Marshallian economics has been identified in Rome (with Pantaleoni and Barone, supporters of the fertility of the methodology of partial equilibrium for the applied research and of the compatibility of the systems of Walras-Pareto and Marshall) and in Milan and Turin, near the Laboratorio di economia politica, a post-graduate centre, established in 1893 by Salvatore Cognetti de' Martiis (with Jannaccone, Graziadei and Einaudi). As Barone wrote (1894), summarising the thought of many economists simpathising with Marshall, it was necessary 'to make up by one's work for that lack of precision or clearness of the great masters'.

18 A major topic of his 1925 article is the fact that increasing and decreasing returns originated in different parts of the classical theoretical apparatus: 'The theory of decreasing productivity was always dealt with by classical writers in relation to the rent of land, and was therefore included, according to the traditional division of economics, in the theory of distribution. Increasing returns on the other hand were discussed in relation to the division of labour, that is in the analysis of production' (1986:16).

It is recalled at the beginning of the 1926 article: 'The law of diminishing returns has long been associated mainly with the problem of rent' (1926a:536); the law of increasing returns 'was regarded merely as an important aspect of the division of labour, and thus rather as a result of general economic progress than an increase in the scale of production' (ibid.: 537). 'The result was that in the original laws of returns the general idea of a functional connection between cost and quantity produced was not given a conspicuous place' (ibid.).

19 In the 1925 article as well as in a note in the 1926 article, Sraffa stressed the fact that in the neoclassical doctrine constant costs are considered extremely improbable because they can only result from the accidental balancing of the two opposite tendencies to decrease and increase costs. But if this *vice* to conceive a constant as the result of the compensation of two equal and opposite variable is abandoned, 'the absence of causes which tend to cause the cost either to increase or diminish may appear to be the most obvious and plausible way from which constant costs can arise' (Sraffa 1926a:541, note).

20 A couple of months before, in a letter to Pigou of 2 January 1928, Keynes maintained that 'the representative firm was a conception which was–deliberately–of too vague a character to support the precise mathematical superstructure' (*Keynes Papers,* EJ/1/3/3).

21 In his lecture notes at the London School of Economics in 1927–9 Young commented that Robbins was 'overcritical', for he did not appreciate the representative firm as an expository device, the vehicle through which external economies affect supply (see Blitch 1990; Kaldor 1990).

22 The firm's expansion in Pigou (1928) is offset by an exactly equal contraction on the part of the other firms in the industry, in Viner (1931) the expansion in the

industry takes place as a result of an increase in the number of firms. These are artificial assumptions the introduction of which is not necessary, as shown by Chipman in his famous 1970 article. The purpose of Chipman's paper is to formulate a general equilibrium model of perfect competition in which firms operate under increasing, constant or decreasing returns to scale. The crucial concept introduced by Chipman is that of *parametric external economies of scale:* 'each entrepreneur is assumed to believe that his firm is operating under constant returns to scale, and any departures from this assumed output-factor relationship are interpreted by him as brought about by a perturbation in his unit-homogeneous production function, even if such departures are caused in part by changes in his own level of output. Such shifts are, in turn, assumed to be governed by the level of output in the industry' (Chipman 1960:349). The concept is illustrated by Chipman in terms of Smith's pin factory: 'If a particular firm expands, some of the work can be divided and specialities will develop. Such specialised labor becomes available, at least part-time, to other firms in the industry. However, only a substantial expansion in the industry will provide enough openings for a pool of labour to develop with a specialised skill, and the contribution of a single firm to this process will be so imperceptible that it will be neglected by the entrepreneur. The change in the character of the labor force will be regarded as exogenous by all firms, even though each firm necessarily contributes to the process' (ibid.). These economies are conceived to be completely external in the minds of the individual entrepreneurs and fully reversible.

23 Keynes too was very criticed of Pigou's 1928 paper, as it appears in two letters sent to Pigou of, respectively, 2 and 10 January 1928. In the first letter Keynes writes: 'My point is…that the bulk of the article does not seem really to require any reference to a representative firm. Do you prove anything whatever beyond the two propositions following: (i) where there are firms in the industry which have not yet reached the point at which internal economies in the strict sense have ceased to apply, there can be no position of equilibirum with a multiplicity of firms, (ii) On the other hand, where the economies are external, so that firms other than new firms have reached a size which cannot be increased with advantage, there is no difficulty about equilibrium. Are not these conclusions both familiar and obvious, and does the elaborate argumentation add anything?' In the second letter Keynes writes: 'You seems to assume a world so static that it would be illegitimate to assume that an increase of demand modifies the rates of growth and of decay of individual firms. But it is not clear whether you are assuming a world so completely static as to be quite remote from anything in experience, or whether you are still able to preserve some faint immage of the actual world. My criticism amounts…to alleging that you spend all your time and trouble on what is easy and obvious, namely the algebra, and hardly any of it to making quite clear to the reader the precise character of the abstract world in which you are moving and its relationship to the real world' (*Keynes Papers,* EJ/1/3). At that time Keynes thought that the problem of considering increasing and decreasing returns could be solved only along the Cournot path pointed out by Sraffa; as he writes in a letter to Roy Harrod, 1 August 1928: 'I am still an adherent of the theory put forward by Sraffa in his Journal article to the effect that observed results could only be explained by assuming that each producer has within certain limitations his own private and local market' (*Keynes Papers,* EJ/1/3).

24 It is interesting to note that Sraffa was not fully satisfied with his criticism and probably he preferred not to publish it. Sending it to Keynes on 28 January 1930, he wrote: 'Dear Maynard, here is my criticism of Dennis, with reply and rejoinder. But please do read it and, if possible, destroy it. As you will see it is a)

silly b) rude to Dennis c) badly written. I didn't mean it to be any of these things, but good intentions will not help me' (letter kept in *Keynes Papers,* EJ/1/3).

25 Wolfe considered himself strongly influenced by Robertson on the representative firm, as appears in two letters he wrote to the English economist on 1 September 1954 and 25 October 1957 (letters kept in *Robertson Papers,* Trinity College, Cambridge).

26 It seems that this article was influenced by Sraffa's (see Gregory 1929). Also Charles Blitch maintained that 'Young's paper was written in partial reply' to the idea that 'in order to deal with increasing returns it was necessary to turn to monopoly theory' (unpublished letter to Nerio Naldi of the 28 May 1997; see also Blitch 1983b; I thank Nerio Naldi for his permission to quote this letter).

4 Sraffa and Cambridge economics, 1928–1931

Maria Cristina Marcuzzo[1]

Foreword

In the introduction to the first volume of her *Collected Economic Papers,* published in 1951, Joan Robinson in an illuminating line described Sraffa's teaching as 'penetrating our insularity' (Robinson 1951:vii). This sentence could have been a title for this chapter, which is mainly concerned with Sraffa's impact on Cambridge economics in the late 1920s and early 1930s. The assessment has been facilitated by the new catalogues of Keynes, Kahn and Joan Robinson papers,[2] the recently granted access to Sraffa's papers, and the availability of Austin Robinson's papers, the cataloguing of which is under way.[3]

In looking into this material and reviewing the new evidence, I hope to contribute to a better understanding of the issues involved and to trace out the development of new ideas more accurately. The scope of this chapter is, however, rather limited. I shall examine two episodes which are extensively discussed in the literature and for which I have found new evidence: (a) the developments in value theory, which went under the name of the imperfect competition revolution; (b) the 'arguing' about the *Treatise,* which paved the way to the Keynesian revolution.

The time span under consideration is also very short: from the autumn of 1928 (when Sraffa gave his first set of lectures) to the autumn of 1931–when, as result of many discussions (chiefly in the 'Circus'), Keynes decided that he had to postpone his own lectures, feeling that a 'theoretical clean up' was needed before he could 're-lecture stuff which is available in print'.[4] In the same year Sraffa resigned from his lectureship as from 30 September 1931.[5]

Sraffa's *Lecture Notes*

Sraffa's lectures on *Advanced Theory of Value*–after being postponed for a year at Sraffa's request–were listed in the *Cambridge Reporter* for the Michaelmas and Lent Terms of the academic year 1928–9, on Tuesdays and Thursdays, at 12 noon at King's. These lectures were given again, with very few

excisions and additions to the material covered, in the Michaelmas and Lent Terms of the academic year 1929–30, and in the Lent Term 1931 (Sraffa was on leave of absence in the Michaelmas Term 1930).

Among Sraffa's papers there is a set of *Lecture Notes on Advanced Theory of Value,* consisting of about 220 handwritten pages, of which roughly two thirds correspond to material covered in the Michaelmas Terms on theories of production and distribution, the remaining third–covered in Lent Terms–dealing with the theory of demand and forms of competition. In his *Lecture Notes* Sraffa drew on his previously published articles in the *Annali di Economia* (Sraffa 1925a) and the *Economic Journal* (Sraffa 1926a), leaving indications of the pages of the relevant passages.

In the *Lecture Notes* the focus on the theory of value historically considered is meant to show how the notion of cost of production was transformed from the classical school to the marginal school, leading–as the result mainly of Marshall's work–to unification with utility and the statement of a symmetry between cost and utility. For such a unification to be possible–Sraffa argues–the notion of cost of production had to undergo a series of changes which made it unrecognisable in terms of the meaning given to it by the classics, but comparable with utility. 'It is only when cost is conceived as a quantity of utility'–Sraffa wrote–'that is to say of negative utility, that it can be brought together with marginal utility in a single theory of value' (*SP* D 2/4 3(18)).

Comparison between the notion of cost in Petty and in the Physiocrats, on the one hand, and in Marshall, on the other, shows that, while for the former authors cost is mainly food for the worker, for the latter it is the sum of 'efforts and sacrifices', in abstinence or waiting and in labour required. These two notions of cost reflect different conceptions of what economics is about (classical economists were mainly concerned with *measures,* marginalist authors were mainly concerned with *motives*) and gave rise to two theories of distribution. Thus, Sraffa wrote:

> For Marshall, wages, interest and profits, are simply shares in the product; they are co-ordinate quantities, that can be regarded as acting upon the value of the product in the same way. Both, are the inducement required to call forth certain sacrifices, which are equally necessary for production, and they are also the reward of those sacrifices…. It is not necessary for the actual goods which compose real wages and profits to be in existence at the beginning of the process of production–the hope, or the promise of these goods is equally effective as an inducement. They operate on production only by being expected, but that comes into existence only when production is finished, as shares in the product.
>
> Petty and all the classics, on the contrary, take the opposite view. They don't regard at all wages as an inducement; they regard them as a necessary means of enabling the worker to perform his work.
>
> (*SP* D 2/4 3(22–23))

These two conceptions of wages and profits–in one, profits are surplus of the product and wages are necessaries, in the other, both are shares in the product–descend from the two definitions of costs given by A.Smith, as 'amount of labour, of a toil and trouble required for producing a commodity, *or* as a stock of material goods, which are used mainly for supporting that labour' (*SP* D 2/4 3(35)). From these two representations of cost of production two schools of thought developed: one, heralded by Say, which saw cost as the sum of the prices of productive services and the other, championed by Ricardo, which reduced cost to labour and excluded rent from cost, the treatment of capital being more uncertain.

Sraffa illustrates this transition from the notion of cost as stock of material goods to that of an amount of human sacrifice with a thorough analysis of Ricardo's theory and the change brought about by Senior, J. Stuart Mill and Cairnes, on the one hand, and of the Austrian School (Menger, Wieser, Böhm Bawerk) on the other. This narrative ends with a description of the opportunity cost theory mainly in the works of Davenport and Wicksteed.

The conclusion which this analysis leads to is that the interdependence of cost and quantity produced is:

> quite a modern idea. All the classical economists ignore it altogether so much that it cannot even be said that they assume constant costs to operate throughout, as their argument implies, since they do not take the question into consideration at all.
>
> (*SP* D 2/4 3 (79))

To make cost dependent on quantity, the laws of increasing and decreasing returns were used, transforming the meaning they had in classical political economy. Thus, in the marginalist approach:

> diminishing returns in general arise from a change in the proportion in which the different factors are combined in an industry, independently of whether the magnitude of the total product increases or falls. Increasing returns, on the contrary, are in general connected with increases in the absolute size of the industry, and have only a remote relation with the proportion in which the factors are employed: the essential thing is that the amount of factors used per unit of the product should decrease…
>
> (*SP* D 2/4 3 (88))

Sraffa then examines the assumptions underlying diminishing returns, according to the marginalist approach, namely:

> (1) that the entrepreneur is governed in his decision by the principles of substitution; (2) that there is a certain degree of *variety* and *independence*

> between the units under consideration: i.e., which compose the variable factor, or between the units which compose the constant factor, or between the methods by which the two factors can be combined (that is to say, the ways in which the variable factors can be utilised).
>
> (*SP* D2/4 3(103))

Sraffa then gives examples of diminishing returns in agriculture and discusses Wicksteed's distinction between two types of ordering of marginal units of a factor, one based on what is said to be 'an arbitrary arrangement of units of factors in a descending order of effectiveness' and the other on a 'causal connection between the number of units and the effectiveness of the marginal unit' (*SP* D 2/4 3(109)). Sraffa argues that there is no such difference, 'that, in fact, one is quite arbitrary as the other, because in both cases the decrease in the marginal product is due to the action of the producer, directed to obtain in each case the maximum product' (*SP* D 2/4 3(112)).

Sraffa then goes on to discuss the role of diminishing returns in the construction of the supply curve for the industry. He shows that, since they arise from a factor which is constant for the industry, but not for the individual firm, the supply curve should be made up not of the whole of the curve of the individual firm, but only of the quantity and cost that correspond to the optimum size of the firm.

Turning now to increasing returns, Sraffa explains why–at the individual firm level–they are incompatible with perfect competition, since they would lead to monopoly. Therefore, increasing returns for an industry can arise only because there are supposed to be external economies: 'the result being that, if a single firm expands its output, its costs rise; but if all the firms expand at the same time, the costs for each of them fall' (*SP* D 2/4 3(130)). It follows that while it is assumed that a close interdependence between the costs of any firm and the quantity produced by other firms in the same industry exists, it is also assumed that there is independence between these costs and the quantity produced by firms in other industries. Sraffa argues that this type of external economy is very hard to find. What is more likely to occur–as classical economists would have it–is that, as a result of general improvements, all industries will be affected. It follows that external economies cannot be considered in the supply curve of a commodity because prices of products of other industries are affected, therefore altering the demand curve for that commodity, and thus violating the condition of independence of supply and demand for a commodity in any given industry. Thus–Sraffa concludes–constant costs are the general rule. This topic brings to a close the lectures to be given in the Michaelmas Terms.

In the introductory remarks to the set of lectures to be given during the Lent Terms, Sraffa declares that he will not follow a 'logical scheme', but simply deal with miscellaneous topics: the assumptions underlying demand

curves, the theory of general equilibrium, and 'special' cases of value, such as monopoly and international trade.

After reviewing the substance of the arguments presented in the Michaelmas Terms, Sraffa first discusses imperfect competition and monopoly, arguing that monopoly should be regarded 'not as the opposite extreme of competition, which either is or is not: but as "a quantity", a substance as it were, which may be present to a greater or smaller extent' (*SP* D 2/4 13(3)). However, the degree of monopoly is definite only if demand is a constant elasticity curve, but in general elasticity is defined at a point and it is different at different points. Sraffa then comments:

> In these cases I am doubtful as to which is the characteristic point (where the two curves cross, as in competition, or where monopolist fixes prices, or an intermediate: the second *must* have elasticity *less* than unity, in general, if there are costs, *much* less). Probably, the best definition of the strength of a monopolist, is the elasticity of a constant elasticity curve which can be superimposed upon the demand curve (in the 'relevant' part) and most nearly fits it.
>
> (*SP* D 2/4 13(4))

What is meant by the strength of a monopolist is not the magnitude of his gains, but his power as a seller. Monopoly is so defined as to include the cases in which, although there are many producers of a commodity, due to 'the lack of indifference of the consumers as to the firm from which they are going to buy, the conditions of competition break down' (*SP* D 2/4 13(5)). Sraffa then explains that the difference between the usual case of monopoly and the ones considered by him

> lies chiefly in the nature of the substitutes, or rival commodities–i. e. whether the money which is not spent when price raised goes to *many* different commodities or to *one* substitute.
>
> When we are considering an individual producer this does not matter: it is indifferent to him what they do with their money, since they do not buy his goods.
>
> But when we want to consider the industry as a whole the two cases are very different. (It is obvious that to do this we cannot compound several demand curves–they refer to different things.)
>
> (ibid.)

There is, therefore, great interdependence between producers, because when one increases his price, the demand schedule of the others goes up and they also increase their prices. Consequently, the first producer will increase prices again and so on; the limit to this series of price increases is the loss of customers to other firms in the same industry or to firms in other industries. Under these conditions the market price cannot be determined, since there

will not be one single market price, but a series according to individual circumstances of firms. However, Sraffa argues:

> if we assume (and preferences can be expressed as proportion of price) that the private markets are very similar, we can see what the market price will be (I assume entry of new firms is forbidden).
>
> When individual firms raise price part of the customers are sent from one to the other–part are lost to industry. But no more are lost than if it were a single monopolist: it is *these* that set the limit to general rise. Consequently, the price will be fixed as by a *monopolistic association,* however small the lack of indifference.
>
> Of course this is only true for *short periods* and for longer, only insofar as expenses of *establishing trade connections* and build up a private market make it improfitable [sic] for newcomers.
>
> (*SP* D2/4 13(8))

Should individual increasing returns prevail, however, when the demand schedule is raised following an increase in price by other firms, it may pay that individual producer to reduce his price, and this will probably lead to the establishment of a single monopoly. In so far as this happens equilibrium is indeterminate.

The foregoing analysis shows the dependence of the shape of the demand curve upon the prices of other commodities, and the necessary condition for its validity, that is that prices of substitutes remain fixed.

The final set of lectures deals with the general method of approach of the general equilibrium theory in comparison to the partial equilibrium approach. Its main conclusion is stated as follows:

> As regards the equations of general equilibrium, their chief importance is as an attempt to prove that no vicious circle is involved in determining prices by supply and demand…a sufficient number of conditions may be found to determine all the prices and quantity exchanged simultaneously.
>
> (*SP* D 2/4 28)

Although in the *Lecture Notes* there are many references both to the 1925 and 1926 articles, their scope is much wider. This is clearly the result of the extensive work in which Sraffa has been engaged since the summer of 1927,[6] on what would eventually become the core out of which *Production of Commodities by Means of Commodities* grew. The development and transformation of the notion of cost of production from the classical school to the marginal school is the new element in the evolution of Sraffa's thought. The novelty appears to be the discovery that there are two notions of cost–one concerned with *necessaries* and the other concerned with *motives*–which gave rise to two theories of distribution and two conceptions of wages and profits, one as

surplus of the product over necessaries and the other as *shares* in the product. As a consequence, the whole attribution to the classical economists of the assumption of constant costs was discarded. In the 1925 article the idea is still attributed to Ricardo and Mill (Sraffa 1925a:316n), but the point is not reiterated in the 1926 article.[7]

However, in the 1926 article it is still maintained that '[i]n normal cases the cost of production of commodities produced competitively…must be regarded as constant in respect of small variations in the quantity produced' (Sraffa 1926a:540–1). In the *Lecture Notes* the argument is reiterated. The assumption about constant costs, however, turned out to be a crucial point in the difficulty encountered by Sraffa in presenting his own research project, since we are told in the *Preface* to *Production of Commodities by Means of Commodities* that when, in 1928, 'Keynes read a draft of [its] opening propositions…he recommended that, if constant returns were *not* to be assumed, an emphatic warning to that effect should be given' (Sraffa 1960a:vi). Now a further piece of evidence is provided by a letter from Pigou of January 1928, in which he wrote to Sraffa: 'Your equations seem to me capable of being subsumed as a special case of the general analysis. You in effect are simply supposing that each of the three (or n) commodities is being produced under conditions of constant returns' (*SP* C239 1).

The theory of imperfect markets

The excitement aroused by Sraffa's lectures is well described by Austin Robinson:

> When Joan Robinson and I came back from India and settled down again in Cambridge at the beginning of 1929, the most vigorous arguments of our younger friends were primarily concerned with Piero Sraffa's lectures and derived *more remotely* from his *Economic Journal* article in 1926.
>
> (E.A.G.Robinson 1977:26; italics added)

However, Keynes made the best prediction in writing to Lydia on 28 November 1927: 'On Saturday I had a long talk with Sraffa about his work. It is very interesting and *original*–but I wonder if his class will understand it when he lectures' (*JMK* PP/45/190/3/268).

Among Sraffa's class there were two outstanding pupils who were later to epitomise much of what is understood by Cambridge Economics, R.F.Kahn and J.V.Robinson. While I was not able to find direct evidence of Joan Robinson's attendance of Sraffa's lectures, apart from a letter to Kahn where she mentioned it,[8] we have records of Kahn's attendance both in his papers and in the Sraffa papers.[9]

Here I shall be concerned with a point raised in Kahn's Fellowship dissertation on *The Economics of the Short Period*–written between October 1928

and December 1929–and conceived under Sraffa's influence,[10] where, according to Kahn, the 'exposure of a serious error in Sraffa's exposition' (in the 1926 article) can be found. In that article Sraffa writes (Sraffa 1926a:549) that 'for an industry consisting of firms which are all similar and similarly situated the final position of equilibrium is the same as would be arrived at if the whole industry were controlled by a single monopolist' (Kahn 1989:94).[11] According to Kahn, the implication of Sraffa's point is that, under conditions of uniformity among firms, provided that the market is slightly imperfect, the magnitude of the imperfection is irrelevant to the equilibrium price.[12]

Kahn's challenge to Sraffa's conclusion is based on his analysis of the individual demand curve facing each seller. This curve, according to Kahn, indicates:

> what he imagines to be the relation between his price and his output, and the position of equilibrium depends on the slopes of these individual demand curves. These in their turn depend on the particular assumptions that are in the minds of the individuals when they draw up their demand curves.
>
> (Kahn 1989:98)

The assumptions–'that are in the mind of the business man when he maximises his profits' (Kahn 1989:100)–can be reduced to three cases; when he assumes that if it altered its price, (a) the *prices* of all other firms remain constant; (b) the *outputs* of all the other firms remain constant; (c) the other firms will alter *both* their prices and their output. Kahn proves that, in all three cases, the aggregate demand curve of an industry in the hands of a single monopolist is steeper than the demand curve facing each firms (identical and similarly situated) in a oligopolistic industry. It therefore follows that, contrary to Sraffa's assertion, 'under conditions of polypoly the equilibrium price is less than under conditions of monopoly' (Kahn 1989:117).

In the dissertation, Kahn had declared in a footnote that 'Professor Sraffa has admitted, subject to a possible reservation, the force of my objection to his argument' (Kahn 1989:95). Moreover, in the 1989 preface he added: 'An unpublished letter from Sraffa to Keynes (King's College, Cambridge, Library) is of interest' (Kahn 1989:xvn). Unfortunately, I have found no evidence of this. Rather, in Sraffa's papers, I found a note added to the *Lecture Notes,* and clearly written after Sraffa read Kahn's dissertation, in which Sraffa says:

> To say that in imperfect competition price is always less than in monopoly, it means to fall into the same error as above, which is based on assumption that problem is independent of the relation between individual and collective elasticity of D[emand]…. The point is that I assume a slight,

> but finite, degree of imperfection (elasticity of D[emand] *not* infinite). But in this case, with the rise in prices, the elasticity decreases all the time, without limit. (This argument would be conclusive if the final equilibrium were reached when *all* imperfection has vanished: but in fact it is reached long before that happens. Imperfection disappears for infinite price, whereas equilibrium is reached at finite price.)
>
> (*SP* D 2/4 10 (verso))

The tentative reconstruction I offer of the point at stake between Sraffa and Kahn is the following. Kahn based his analysis on conjectural demand curves whose slopes embody various assumptions about the behaviour of other firms within the industry. A change in price by any one firm does not leave the slope of the demand curves of all other firms unchanged because the reactions of competitors are taken into account. In general, when there is only one producer (as in monopoly), its demand curve is steeper than when there are many producers (as in oligopoly), because in the latter case firms are aware of the behaviour of others firms since there are alternative sources of supply for that commodity within the industry. Since equilibrium price, for given supply curves, is determined by the slope of the demand curve, it follows that in monopoly it is higher than in oligopoly.

On the contrary, Sraffa's argument is based on the degree of consumer preferences as shown by the elasticity of demand. Following an increase in price by one firm, demand curves facing all firms are raised. Since prices of substitutes go up, each buyer is willing to pay a higher price for the product of the firm from which he prefers to buy (Sraffa 1926a:547). The limit to the price increase is given by the loss of customers to the market, not to the individual firm, since customers will return to the firm they prefer when the other firms have also raised their price. He writes: 'The question seems to be whether the number of customers a firm loses when it alone raises the price is equal to the number it loses when *all* firms have raised it by that amount (*SP* D 2/4 10). Thus for Sraffa, unlike Kahn, 'for an industry consisting of firms which are all similar and similarly situated' there is no reason why the price corresponding to the Marshall's 'maximum monopoly revenue'[13] would be different in monopoly and in oligopoly.

Demand curves

The impact of Sraffa's challenge against the Marshallian approach was reinforced in the Symposium, held in the *Economic Journal,* in March 1930. Among the Sraffa papers, there is an invitation card to the Political Economy Club,[14] signed by its Secretary, Richard Kahn, announcing for 24 February 1930, "G.F.Shove, D.H.Robertson and P.Sraffa on 'Increasing Returns and the Representative Firm: A Symposium'" (*PS* D/3/7 13). This was perceived as quite an occasion as Keynes explained to Lydia in a letter of 24 February

1930: 'Tonight Dennis and Gerald and Piero are going to dispute together at my Economic Club and a large company will come to hear them. I shall need all my tea-cups and more than all my chairs' (*JMK* PP/45/190/4/207).

A new piece of evidence can now be added to this matter, in the form of a copy of letter written (but perhaps not sent) by Sraffa to Shove, dated 26 February 1930 on the question of including marketing expenses in the cost of producing a commodity. Sraffa wrote:

> My point, which I did not succeed in explaining properly in Monday's [Political Economy Club] discussion, is this: The cost of producing an additional unit is a definite sum of money, which depends only upon the quantity produced. But the cost of marketing is different: the 'cost of marketing 100 pairs of boots' is indeterminate until we know at what price the boots have to be sold. You can always find a sufficiently low price at which you can sell your 100 p[air] of boots *without incurring* any marketing expenses; and, on the other hand, if you spend a sufficiently large sum in advert, etc., you can sell your boots at any desired price, however high. Therefore, when you speak of the cost (including marketing expenses) of putting an additional unit on your competitor's market, I do not know what you mean until you tell me at what price it has to be sold.
>
> (*SP* D 3/7 8)

Moreover, in a handwritten note, Sraffa made his point even clearer:

> The chief objection to this point is that S [Shove] regards marketing expenses as part of cost of production]: he overlooks that they are directed to *affect* the demand curve, and therefore there is no demand curve which can be used with a supply curve that includes them–they are not independent.
>
> (*SP* D 3/7 23)

The issue of the marketing expenses had already been raised by Kahn during one of Sraffa's lectures (see *PS* D 2/4 13(2)). In his answer Sraffa pointed to two cases, one represented by a horizontal demand curve and the other by a down-sloping demand curve. The first case is when all the expenses necessary to raise the demand curve to market level are included in the supply price; in this case the two curves are not independent. The second case

> represents the demand curve as it is at the *current* amount of advertisement;… The supply curve represents *only* factory expenses of production and does not change with advertisement. The price is fixed on monopoly principles, so as to maximise monopoly revenue. For each total amount spent in advertisement there is a different demand curve (to be

> coupled always with the same supply curve) and therefore a different monopoly revenue.
>
> (*SP* D 2/4 13 (2))

Once again, Sraffa's point seems not to have been taken since, in his dissertation, Kahn wrote:

> Selling and advertising expenses are to be regarded as completely determined, being unambiguously dependent on the output. I understand from Professor Sraffa that when these expenses are *de facto,* if not *de jure,* a necessary adjunct to the process of production, both qualitatively and quantitatively, he would not regard them as marketing expenses at all. We are entitled therefore, on our special assumption [when a producer desires to increase his output he reduces his price rather than increases his advertising expenses], to disregard his objection that the inclusion of *marketing* expenses in cost of production renders the expression 'cost of production' 'dependent upon elements quite extraneous to the conditions under which the production of a given undertaking takes place' [Sraffa 1926a:544], And at the same time, of course, it is possible to regard the individual demand curve as a definite independent entity, since we get around Professor Sraffa's plea that changes in marketing expenses should be conceived as shifting the demand curve' [Sraffa 1926a:543].
>
> (Kahn 1989:89–90)

Among Kahn's papers an extended criticism of this passage by Sraffa is kept, probably to an early draft of the dissertation:

> When we say 'cost of production' we mean 'necessary cost'. And necessary cost implies a reference to a condition to be fulfilled, i.e. 'x costs are necessary *if* an amount y of the article has to be produced'. We do not always repeat this condition since it is common to all costs of production properly so called. But it is not common to marketing expenses. These are only necessary 'if a given amount of product has to be produced and *sold.*' Besides, a reference to the *price* at which it must be sold is required, since a firm could sell practically any amount, without any marketing costs, at a price sufficiently near to zero: just as it could *produce* any amount, without any marketing costs, if it hoards the product. Nothing is said about this price in §8 [Kahn 1989:89–90]: and therefore marketing expenses are *not* unambiguously dependent upon output. (*Any* definition that can be given seems unacceptable. The one relevant to the problem, i.e. such a price that covers all kind of costs and gives the maximum profit in general enables us to draw, not a curve, but a point–the maximum to be sought: in solving the problem we start from a single datum–and it is the solution itself!).
>
> (*RFK* 3/13/153)

Once again–following Marshall and Shove's[15] teaching–Kahn was adhering to a notion of the individual demand curve 'as a definite independent entity'. On the contrary, Sraffa's effort was to show that in general it was not, and in most cases that a given quantity (a point) rather than a schedule relating hypothetical or conjectural quantities to price was all which was needed for the problem to be solved.

The issue of how to draw demand curves was heavily debated between Joan Robinson and Richard Kahn when *The Economics of Imperfect Competition* was being written. In a letter to Kahn of 10 November 1930–i.e. soon after the work on the book had begun[16]–Joan Robinson wrote:

> I am prepared to believe that stuff now without your geometrical proof, though I think it should be done as a work of art. But it knocks a hole in Piero's stuff about monopoly analysis, as far as I can see. (v. p. 547 EJ, Dec. 1926 bottom of the page). When the demand curve for Rowntree is raised by the fact of Cadbury having raised his price, Piero says that Rowntree would only lower his price if the demand for his chocolate is very elastic and his private supply curve sharply decreasing. According to you it would lower it in any case. How's that?
>
> (*JVR* vii/228/1/3)

An entire chapter of *The Economics of Imperfect Competition* was devoted to analysis of the assumptions relating to the elasticity and slope of demand curve in affecting price in monopoly conditions (Robinson 1969 [1933]: 60–82). Just before the book was published she summarised aptly what was going on, in writing to Kahn, on 18 January 1933: 'Piero has sent back the proof of Book III [of *The Economics of Imperfect Competition*] which I sent him. He can't swallow the modern demand curve, but otherwise makes no big point–some useful minor ones' (*RFK* 13/90/1/58).

By that time Sraffa must have given up any hope that he could persuade the 'younger generation' of Cambridge economists to move away from the Marshallian demand and supply curves. I shall now turn to the other and certainly more important 'revolution' going on in Cambridge in the early 1930s where, perhaps, Sraffa was more hopeful of exercising some influence.

Arguing the *Treatise*

In the Michaelmas Term of 1929 Keynes was lecturing from the proofs of his *Treatise on Money,* but the book was published only in October 1930. He had been busy revising and rewriting it, under the stimulus and criticisms coming from different quarters, which did not stop after publication. Hawtrey, Hayek and Robertson on the one hand, and the members of the 'Circus' on the other, kept the argument about its validity and implications going.

As we know, the 'Circus' was the *Treatise* informal discussion group that met between late 1930 and the spring of 1931, including Richard Kahn,

James Meade, Piero Sraffa, Joan and Austin Robinson and also some of the most brilliant economics students of the recent generation. Unfortunately, scant written material has survived to document the group's activities, later reconstruction being based on the individual and collective recollections of the participants, and including a number of contrasts in the interpretation of how things really went.[17]

Very little is known about the role of Sraffa in the 'Circus', but for an account by Joan Robinson, almost fifty years later, according to which: (a) the 'Circus' was 'first proposed by Piero Sraffa' and (b) that '[Sraffa] was secretly sceptical of the new ideas' (Robinson 1978:xii). Of course, we also have the exchange between Sraffa and Keynes published in the *Collected Writings* in the form of a paper written by Sraffa, dated 9 May 1931, and of a letter by Keynes, dated 15 May 1931 (Keynes 1973a:207–11).

New evidence has been found of Sraffa's involvement in this discussion and more work is needed to understand the nature of his contribution.[18] On this occasion I shall discuss one issue related to the 'Fundamental Equations', namely the asserted 'independence' of the determination of the price of consumption goods and investment goods, as contained in a paper of 15 April 1931 (*SP* D1/81).

It will be recalled that in the *Treatise* the price level of consumption goods is set as equal to the sum of two terms, the first being the cost of production, while the second is given by the difference between the current cost of production of investment goods (I') and saving (S), defined as the difference between monetary incomes and expenditure on consumer goods. This term is positive, nil or negative according to whether the cost of the new investment is greater than, equal to or less than the current saving. The difference constitutes what Keynes calls the extra-profits (if positive) or losses (if negative). When the difference is nil the production decisions taken by entrepreneurs on quantities of consumption and investment goods correspond to (are compatible with) the decisions taken by the public as a whole to allocate their incomes between consumption goods and savings.

On the other hand, the price level of investment goods–by which Keynes means both capital goods and securities–is determined jointly by the decisions taken by the public on how to allocate their savings between bank deposits and securities, and by the decisions of the banking system on whether or not to create new deposits with the purchase or sale of securities. The price of the securities, and thus of the investment goods produced, is given by the match between the demand for securities by the public and the supply of them by the bank system as a whole. Again in this case, a positive difference between the value of the new investment goods *(I)* and their cost of production *(I')* means extra profits for the producers of investment goods, or losses should the difference prove negative.

The equilibrium condition of the overall system (i.e. when the extra profits in both sectors are zero) is given by the equality of the value of investment to saving. Thus, we have:

$$\begin{aligned} Q_1 &= I' - S \\ Q_2 &= I - I' \\ Q &= Q_1 + Q_2 \\ &= I - S \end{aligned}$$

where Q_1=extra-profits in the consumption goods sector; Q_2=extra-profits in the investment goods sector.

Total profits *(Q)* are the equilibrating mechanism, whose effects on the system depend on how profits are spent. In the 'widow's cruse' example (Keynes 1930:125), if entrepreneurs spend their extra-profits on consumption goods, the positive gap between the cost of investment goods and saving widens: the price of consumption continues to increase, and so do profits. (The opposite applies in the case of losses.)

The 'Fundamental Equations' apparatus was the object of criticism from the outset Hawtrey, Robertson, Pigou and Kahn objected to some of Keynes' definitions and conclusions. In particular, one issue dominated, namely the asserted 'independence' of the forces underlying determination of the two price levels. This issue was linked to two points which, according to Joan Robinson's later recollection, came to the fore during the "Circus": the 'widow's cruse fallacy' and 'the-buckets-in-the-well fallacy'. The exposure of the widow's cruse fallacy was that an increase in the expenditure on consumption goods, in particular when there is unemployment, is likely to increase output rather than prices. The buckets-in-the-well fallacy–(Keynes 1973a:223)–was the criticism of the contention made by Robertson according to which, as saving increases, more money would be channelled into the Stock Exchange via an increase in the demand for securities. If, then, there is an excess of saving over investment, the price level of consumption goods declines and the price of investment goods rises, moving therefore in the opposite direction, the two prices behaving, as it were, like 'buckets-in-a-well'. The argument of the *Treatise* implied, on the contrary, that the movements of the two price levels was usually in the same direction, but in general they were independent.

In early April 1931, in one of his many attempts that year,[19] Kahn sought to persuade Keynes that variations in the price level of consumption goods *(P)* and investment goods *(P')* contrary to what Keynes stated in the *Treatise* (Keynes 1930:123) are not independent, and that Keynes' statement, therefore, had little ground to stand on in the face of the criticisms raised by Robertson and Pigou.[20]

Kahn argued his case imagining–as he wrote in a letter to Keynes on 17 April 1931–drawing 'a cordon' (Keynes 1973a:206) to separate the sector producing consumption goods from the rest of the economy and doing the same for the sector producing investment goods. The value of monetary expenditure channelled into the consumption goods sector is equal to the value of the monetary expenditure channelled into the rest of the economy by the consumption goods sector. Similarly, the value of monetary

expenditure going into the investment goods sector equals the value of the monetary expenditure flowing from the investment goods sector into the rest of the economy.

Let us suppose, Kahn continues, that there is a fall in savings equal to *a*. This means a rise, equal to *a,* in the monetary expenditure in the consumption goods sector and a corresponding fall in the monetary expenditure in the investment sector. The expenditure on investment goods by the producers of consumption goods increases by *a,* while the inflow into the investment sector remains unchanged (the extra monetary expenditure coming in from the consumption goods sector exactly offsets the initial fall in saving). In principle, there is no reason why the price of investment goods should change. However, if the price of the investment goods were to increase, the expenditure on consumption goods would further increase by a corresponding amount (say *b*), so that total expenditure on consumption goods would increase by *a*+*b*. Given that an increase (or decrease) in the expenditure by one sector always implies an increase (or decrease) in the demand for the goods produced in the other sector, the two price levels are always connected. Thus, Kahn concluded: 'It *is* quite possible for *one* price level *or* the other to remain unchanged, but not for both' (Keynes 1973a:207). In other words, given the price level of one sector, the price level of the other sector was also determined.

Kahn's argument here was the logical consequence of the reasoning underlying the multiplier principle,[21] where the focus is not on how profits are spent, but how expenditure in one sector affects expenditure in the other sector.[22]

Sraffa's paper of 15 April 1931 was conceived as a criticism of an earlier note by Kahn of 5 April 1931 (Keynes 1973a:203–6), in which Kahn exposed the fallacy of the independence of the two price levels by adhering more closely to the terminology of the *Treatise,* rather than that of the multiplier article. Sraffa's paper provides us with his criticism both of the 'widow's cruse' and the asserted independence between the price of investment goods and consumption goods, which unlike Kahn's argument, is based on consideration of production.

As we saw in the *Treatise,* the profits in the consumption sector, Q_1, arise when there is an excess of expenditure over costs, meaning that more consumption goods are demanded than produced. This implies that fewer investment goods are demanded than have been produced, giving rise to an amount of losses (for the producers of those goods) equal (but of opposite sign) to the profits made by the entrepreneurs in the consumption goods sector. Thus, entrepreneurs in the investment sector, to make up for the losses–measured by the (negative) difference between the value of the new investment goods and current savings–can either sell securities or reduce their bank deposits. Keynes writes: 'The bank deposits thus released and the securities thus sold are available from, and are exactly equal to, the excess of current savings over the value of new investment' (Keynes 1930: 131).

Savings are again brought in line with the value of new investment goods, with no necessary change in the price of investment goods, for an assumed behaviour of the banking system.

In his paper, Sraffa commented extensively on the implicit assumption made in the *Treatise* according to which 'the process of making profits and spending them (an infinite number of steps) takes no time to happen', stressing the point that 'profits made by the sale of given goods cannot be spent in purchasing the *same* goods' (*SP* D1/81).[23]

Sraffa's argument is that the income generated by the production of goods represented by the cost of production cannot determine the price at which those goods will be sold. Expenditure takes place after earnings are paid out, and therefore the decision on how to allocate consumption and savings out of a given income occurs *after* consumption and investment goods are produced. Profits or losses made on goods produced and sold can only be influential on the output of next period. The role of profits therefore is to influence what the level of output will be in the next period, rather than the level of prices in the current period.

As is well known, criticism from the 'Circus'–although the matter is far from being settled in the literature–prompted Keynes to move on to a different track from the 'Fundamental Equations' and to bring in changes of output. It was during the summer term months of 1931 that, according to Kahn (1984:171), 'the great change occurred'. In fact, he postponed the lectures he was to have held in the 1931 Michaelmas Term to April-May 1932, feeling that a 'theoretical clean up' was needed. In late 1931 in a draft,[24] he was able to present the 'vital generalization' of the argument presented in the *Treatise,* which runs as follows:

> increases and decreases in the volume of output and employment depend upon the changes in disbursement relative to earnings (which is the alternative mode of expression I now offer to the reader) or in investment relatively to savings (which is the mode of expression I employed in my *Treatise on Money*).
>
> (Keynes 1973a:380)

In the spring of 1932 Kahn, Austin and Joan Robinson signed a Manifesto, presenting the 'alternative' (as Keynes put it) or 'complementary' (as Robinson had it in her correspondence with Keynes) of the 'proof, presented by Keynes in his lectures, that the variation on investment had the same sign as the variation in output. 'The problem seems to us'–they wrote–'to be susceptible to treatment by method of Supply and Demand' (Keynes 1979:44). During 1932, mainly under Kahn's influence, Keynes reshaped his 'new argument' in terms of supply and demand: increase in output and employment were made to depend on 'change in demand as a whole relatively to supply as a whole, due to deficient disbursement' (Keynes 1979:53).

Unlike the *Treatise,* the 'mainspring of change' (Keynes 1930:126) was *expected* rather than *realised* profits. In fact, in the fragment of what, according to Moggridge was probably Chapter 6 of the first 1933 table of contents, the level of employment is made dependent on *prospective* rather than *actual* magnitude:

> …we are basing our conclusions about employment on the proper criterion, namely whether it is expected to *pay* a firm in possession of capital equipment to spend money on incurring variable costs; i.e. whether the result of spending money on employment and of selling the output is expected to result in a larger net sum of money at the end of the accounting period than if the money had been retained.'
>
> (Keynes 1973a:66)

By 1932 the direction of Cambridge economics as far as its most important development is concerned had taken a turn in which the Marshallian apparatus was adapted rather than discarded. Unlike the *Treatise,* the *General Theory* gave a prominent place to aggregate demand and aggregate supply, although the implications of Keynes' theory were dramatically opposed to the vision of the economic system inherited from Marshall.

Concluding remarks

The new evidence coming from the Archives gives further support to our perception that Sraffa was deeply influential in the debates with the younger and older generation of Cambridge economists.[25] In this chapter I have argued that, although he was praised and relied upon, the impact of his criticism of the Marshallian theory and of his attempts to gain acceptance for an alternative approach were surprisingly ineffectual. Rather, his suggestions gave rise to developments which took a direction quite different from the approach which inspired them. Sraffa remained an isolated intellectual figure, feared and admired, rather than actually understood. This is perhaps another example–it is tempting to conclude–of the impossibility of 'penetrating' the insularity of an established body of economic doctrine.

Notes

1 Earlier versions of this paper were presented at the Conference on 'Sraffa and Modern Economics', Rome, October 1998 and at the session 'Sraffa's Centenary', ASSA Conference, New York, January 1999. I wish to thank my discussants M.Dardi and D.A.Moggridge, without implicating them, for helpful comments and suggestions. I am grateful to the Provost and Fellows of King's College, Cambridge, for permission to quote from unpublished letters by J.M. Keynes and J.V.Robinson, and P.A.Garegnani for permission to quote from unpublished manuscripts by P.Sraffa.

2 References are given as *JMK, RFK, JVR,* respectively, according to the classification in their respective catalogues, King's College, Modern Archives, Cambridge.

3 For Sraffa papers *(SP)* references are given according to the classification in the catalogue in Trinity College Library, Cambridge; for Austin Robinson papers *(EAGR),* since the catalogue is not yet available, references are given to the box where the document is kept in Marshall Library, Cambridge.

4 Letter to Austin Robinson of 28 September, 1931 (*EAGR,* box 9).

5 In Michaelmas Term 1931 Joan Robinson gave her first course of lectures on 'Pure Theory of Monopoly'.

6 Garegnani locates in the winter 1927–8: 'an initial (and decisive) turning point …which led to an examination of the classical economists with consequent abandonment of the Marshallian interpretation of them that had been behind the articles of 1925–26' (Garegnani 1998a:152). Also De Vivo (1998:6) argues that 'while preparing his *Lectures* he must have (re)-read Marx and the Classical economists'.

7 However, in the letter sent by Sraffa to Keynes on 6 June 1926, he again referred to constant costs as 'Ricardo's assumption' (Roncaglia 1978:12).

8 'I owe in fact far more to Piero's lectures and private conversations than I owe to any of Gerald [Shove] outside his published works', letter of 7 April 1933 (*RFK* 13/90/1). We have also E.A.G.Robinson's account (Robinson 1994:7): 'Joan had got to know [R.F.Kahn] as a fellow participant in Piero's Sraffa's very unorthodox lecture course.'

9 We have the notes taken by Kahn and his essays written for the course (*RFK*/3/3/359–384) and the answers given by Sraffa to a question raised by Kahn in one of the lectures (*SP* D 2/4 13 (2)).

10 In the 1929 preface to the dissertation, Kahn wrote that 'Chapter 7 [Imperfection of the Market] derives its inspiration from an article by Professor Sraffa' (Kahn 1989:vii); the point is reiterated in the 1989 preface (Kahn 1989:xv).

11 The same argument was reiterated in the *Lecture Notes.*

12 Sraffa, however, adds: 'In itself, this case is of no importance, because it is extremely unlikely that such uniformity would actually be found; but it is representative of a tendency, which prevails even in actual cases where the conditions of the various undertakings differ among each other, whereby the cumulative action of slight obstacles to competition produces on prices effects which approximate to those of monopoly' (Sraffa 1926a:549).

13 'If $y=f_1(x)$, $y=f_2(x)$ be the equations to the demand and supply curves respec tively, the amount of production which affords the maximum monopoly revenue is found by making $\{xf_1(x) - xf_2(x)\}$ a maximum; that is, it is the roots of the equation $\frac{d}{dx}\{xf_1(x) - xf_2(x)\} = 0$' (Marshall 1961 [1890]:704).

14 The Political Economy Club met on alternate Mondays during term time. It was started in 1912 and lasted until 1937, when Keynes became ill. In October 1927 R.F.Kahn became a member and later Secretary. Meetings started at 8.30. There was a large kettle and cakes. A bowl was handed out from which numbers had to be drawn. People were expected to rise and talk according to the number drawn (from conversation with Kahn, autumn 1986). The Club was revived after the war and continued to meet until the 1980s (I am grateful to D.Moggridge for pointing this out to me).

15 In the 1929 preface, Kahn wrote: 'It is difficult…to make sufficient acknowledgement of the advantage that I have derived through having been taught by Mr Shove. Much of what I now believe to be my own must in reality belong to him' (Kahn 1989:ix).

16 Joan Robinson started writing the 'nightmare'–as she nicknamed *The Economics of Imperfect Competition*–probably in the summer 1930, in close consultation with

Kahn. However, she read Kahn's dissertation only in January 1933, when her book was at proof stage (Marcuzzo 1996).

17 For an assessment, see Moggridge (1992).

18 Among Sraffa's papers the documents numbered from D1/70 to D1/77 and D1/81 to D1/82 are all related to the *Treatise on Money*.

19 See the letters of 5 April 1931, 17 April 1931, 7 May 1931, 15 August 1931 (Keynes 1973a:203–6; 206–7; 212–13, 218–19).

20 Eventually Keynes had to acknowledge the point and revised his formulation (Keynes 1973a:225–6).

21 The article on the multiplier was conceived probably in the summer of 1930 but rewritten between late 1930 and the spring of 1931.

22 For a detailed reconstruction of Kahn's role, see Marcuzzo (1998).

23 Sraffa adds: 'As this point is overlooked in several arguments of the *Treatise,* it will probably have to be raised often in our discussions' (*SP* D 1/8 2).

24 According to Moggridge's dating, this is the 'earliest' of the fragments of the period of writing during 1931–2.

25 Skidelsky (1992:289): 'It was Sraffa who gave Cambridge economics its shot in the arm in the late 1920s.'

5 On some controversial aspects of Sraffa's theoretical system in the second half of the 1920s

Duccio Cavalieri[1]

Hermeneutic questions to be addressed

In spite of the very extensive literature which has accumulated over time concerning Piero Sraffa's theoretical system, several significant issues pertaining to the interpretation of the work of this distinguished economist are still unresolved. In this article we will address some points relating to the period 1925–30, in which Sraffa set out his criticism of Marshall's theory of value and began to shape the basic theses that would be put forward later in his 1960 book. It is a five-year period which opened with his famous 1925 essay on the relations between cost and quantity produced; continued with the 1926 article in the *Economic Journal,* on the law of returns, and closed with two short papers in the same review, contributing to the debate promoted by Keynes in 1930 on increasing returns and the *representative* firm.

Four historiographic and analytical questions regarding this period seem to be in need of further clarification. First of all, one may ask why Sraffa chose to level his criticism against Marshall's theory of value,[2] in which marginalist analytical tools were applied for the first time to a classical conceptual structure,[3] rather than launching a direct attack on Jevons, Menger and Walras, who had tried to reduce the whole of economic science to the working out of the implications of the marginalist postulate.

If we may assume that even during that early phase Sraffa's main objective was to criticise the subjectivist approach to the theory of competitive values and to revive the classical doctrine based on the real cost of production, relieved from the labour theory of value, then we may wonder why he chose to contrast his ideas with the views held by an eminent scholar whose theoretical position was nearly midway between classicism and pure marginalism. The opinions expressed by those who defended a thesis diametrically opposed to his own might have been a more fitting butt of his polemics.

We will attempt to outline an answer of this issue in the first part of this study, by concentrating not so much on the supposed reasons of academic convenience that have all too often been invoked, but rather on

methodological and analytical arguments, clearly expressed in Sraffa's 1925 article. Basically, they consist in the idea that Marshall had built a theory of value based on two assumptions that overtly contrasted with the classical vision of price formation,[4] and had made an improper use of Ricardo's thought on that matter, exploiting it for his own aims.[5]

A second aspect of Sraffa's theoretical path which needs to be further studied is the question as to what led him, in the short period of time running between his 1925 and 1926 articles, to change his approach to the problem of price formation and to express the hope that the theory of value would free itself from the hypothesis of perfect competition and move in the opposite direction of monopoly. Up to that moment, his strategy had merely aimed at opposing Marshall's assumption of a supply curve of the competitive firm showing variable unit costs.

We know that in June 1926 Sraffa had already reached this conviction. At that time, he wrote to Keynes that he felt the theory of prices could not restrict itself to studying a first approximation assumption, such as Ricardo's hypothesis of constant returns, which he thought to be 'the best available for a simple theory of competition'. His idea was that an approach to the problem based on imperfect competition was analytically preferable, for practical reasons, to that of a general economic equilibrium ('Pareto's point of view'). 'I am now trying to express in a simple form'–he wrote to Keynes–'how equilibrium can be achieved in such conditions, which I deem to be fairly good approximations to certain aspects of reality'.[6]

Given these premises, one might have expected Sraffa to set out decisively on the road of imperfect competition. Or, failing that, to try to adapt Pareto's point of view to an analytical context of imperfect competition, thus anticipating the later developments by Negishi, Benassy, Laffont and other theorists who supported the idea of a general equilibrium of imperfect competition.

Things moved, however, in quite a different direction. Sraffa soon abandoned the road of imperfect competition–between 1927 and 1928–and with it the study of semi-monopolies, or polypoly. This was a form of market he considered to be predominant in the real world. He believed it would represent the most appropriate way to build a theory of value that would not be limited to the particular case of constant returns, where price determination could be separated from quantity determination. The reasons he abandoned this road have not yet been wholly clarified. They will be dealt with as a third specific point of investigation in the course of the present work.

Once again, this choice by Sraffa lent itself to a methodological explanation, pointing out the analytical need to introduce into the theory of determination of competitive price a subjective demand curve, faced by the firm–an assumption in conflict with the tenets of the objective theory of value that Sraffa intended to defend. On the other hand, the determination of the equilibrium of an imperfectly competitive firm could not fail to resort to

Marshall's method of partial equilibria, the use of which Sraffa deemed to be legitimate only within the framework of a first approximation analysis. This silent choice which Sraffa made was, however, unlikely to be easily comprehended, for it implied the abandonment of a research line that had appeared promising to many scholars who had discussed his 1926 article.

It was probably at this point, in the winter of 1927–28, that Sraffa started to think about a multisector linear model of production of commodities by means of commodities, capable of highlighting the structural interdependence among the various industries. Sraffa himself recalled that in 1928 he had discussed the main lines of such a project with Keynes (who had suggested, should Sraffa's analysis of the problem not presuppose constant returns to scale, to inform the readers of this fact).

Searching for the reasons underlying this final methodological choice made by Sraffa represents a fourth specific problem considered in this investigation.

Sraffa's theory of prices of production did not include any functional relation between costs and quantity produced, a circumstance testifying a basic continuity in the author's theoretical vision. It started out from a very restrictive premise: that the quantities of commodities produced in the system were given, so that any influence of demand on prices was to be excluded a priori (except for that part of demand which could be directly linked to the technical requirements of reproduction).

The heuristic limits which characterised such a theoretical framework could hardly pass unnoticed. If Sraffa's aim was to repropose the Ricardian explanation of prices of production, exclusively focused on the supply side–albeit in a new analytical form that could relieve it of the useless weight of the labour theory of value–then the theoretical model that he had selected for this purpose was inevitably doomed to appear unconvincing, since it was based on assumptions that excluded any different analytic perspective from the very start.

Thus the question arises as to what induced Sraffa to make such a choice. The answer is simple: the idea of isolating the production system at a given point of its evolution made it possible to focus attention upon certain properties which Sraffa thought to be essential for any economic system–those independent of variations in the volume of production and in the proportions among the 'factors' utilised. If the quantity of commodities produced, the technology of the system and one of the two main distribution variables were known, all relative prices of production could be simultaneously determined, together with the other distribution variable.

In order to abstract from the demand side of the problem while considering the extent to which the relative prices of commodities would change when social distribution of income changes, it would have been necessary to assume, against all logic, that changes in the distribution pattern and in relative prices did not induce variations in the volume and composition of expenditure and in production techniques.

It is well-known that Sraffa did not pursue his research on the problem of value and distribution beyond this initial stage. To promote a return to the classical approach to the problem, which he felt had been 'submerged and forgotten' by Marshall and the marginalists,[7] it was natural to delineate an appropriate analytical context, by formulating a set of suitable assumptions, provided that they were regarded as destined to be removed at a later stage. Unfortunately, however, this later stage never occurred in Sraffa's analysis of the problem.

The methodological issue and the reasons for a sudden abandonment

In the mid-1920s, Sraffa focused his attention on the so-called laws of returns, which had been called into question a few years earlier by Clapham for their lack of empirical content.[8] In 1925, Sraffa decided to examine the validity of such a critique from a different perspective, being that of economic logic. He believed that both the hypothesis of increasing productivity and that of diminishing productivity, which he deemed to be of heterogeneous nature,[9] required the presence of conditions contrasting with Marshall's logic of partial equilibria.

Some time later, in the preface to his 1960 book, Sraffa recalled that this idea had led him 'in 1925 to try to demonstrate that only the case of constant returns [was] generally compatible with the premises of economic theory' (p. vi). That is to say, with Marshall's assumption of perfect competition.

Only one year later, upon Keynes' request, Sraffa summarised the contents of his 1925 essay for English readers. In the second part of his 1926 article, however, he suggested a different theoretical framework: one that would associate, as in a monopoly, a diminishing demand curve faced by the individual firm with curves of average and marginal cost characterised by various possible shapes.[10] This analytical approach involved an implicit resumption of Marshall's pattern of price determination, whose basic premises (the partial equilibrium method and the law of non-proportional costs) were satisfied.

In that article, Sraffa clarified the changes he believed should be introduced into Marshall's framework in order to make it more consistent and extend the scope of its validity.[11] Thus the content of that article did not represent a simple extrapolation of the line adopted earlier by Sraffa. On the contrary, it was objectively at variance with his earlier stance.[12]

The possibility that this theoretical position might give rise to misunderstandings was perhaps initially underestimated by Sraffa. Only later would he realise the extent of this risk. This awareness came gradually, presumably between 1928 and 1929, when Sraffa was preparing his Cambridge lectures on the advanced theory of value[13] as part of a course that would cover the entire historical development of the subject, from Petty and the Physiocrats to Marshall and Pareto.

It was at that time that Sraffa resolved: (i) to abandon Marshall's method of partial equilibria once and for all;[14] (ii) to relinquish the idea of further pursuing his study of the hypothesis of imperfect competition; and (iii) to change direction in his research and move towards the construction of a more general theory of prices.

This reading of the evolution of Sraffa's thought seems to be widely shared by its interpreters. The prevailing idea is that, shortly after publication of his 1926 article, Sraffa changed his mind about the usefulness of developing the Marshallian approach to the theory of the firm in terms of imperfect competition,[15] and concluded that such a route was impracticable for analytical reasons and the whole theory of price should be rebuilt on a different basis.[16]

In support of this interpretation, it has often been recalled that during the 1930 debate with Robertson and Shove on the subject of increasing returns and the 'representative firm' (two of the so-called 'empty boxes' of the Marshallian theory), Sraffa made no mention of any imperfect competitive solution. On that occasion, he argued that Marshall's partial equilibrium analysis was of very restricted validity and should be replaced by a different analytical approach, capable of leading to simultaneous determination of prices in all industries.[17]

The theoretical model that Sraffa had in mind at that time was probably similar to the Walrasian system of general competitive equilibrium, which Sraffa considered an analytical construction of remarkable interest, although too abstract and complex to be of any practical use (see Sraffa 1926a:540–1). However, it diverged from the Walrasian model on a very important point: the formal symmetry of the roles attributed by Walras to demand and supply. In Sraffa's long-term conception of price determination only the role of supply was relevant.

From this point of view, Sraffa's picture of the problem was not too distant from Marshall's vision of the process of determination of normal long-term prices. Marshall held that one could describe such prices as governed by cost of production, but with a significant proviso: that 'he does not claim scientific accuracy for the wording of his doctrine, and explains the influence of demand in its right place' (Marshall 1961 [1890]: 291).

Sraffa raised three main objections to Marshall's method.[18] The first concerned Marshall's definition of individual industries as the exclusive consumers of a given production factor or as the exclusive producers of a given commodity.[19] Sraffa maintained that this reasoning could affect the type of returns to scale, inasmuch as increasing returns tend to be all the more probable–and diminishing returns less and less probable–the broader is the definition of industry.

According to Sraffa, only in two exceptional cases could Marshall's approach possibly be reconciled with a supply curve with variable costs: first, for increasing returns, in the case of economies of scale which were external

to each individual firm, but internal to one individual industry undergoing expansion (an improbable category of economies, analysed by Marshall and Pigou); second, for diminishing returns, in the case studied by Barone concerning an industry utilising the whole quantity of a given production factor, regardless of whether its total product increased or diminished. The scope encompassed by Marshall's approach was therefore rather limited.

This methodological attitude was linked with the criterion Sraffa had adopted to determine whether or not it was licit to resort to Marshall's *ceteris paribus* hypothesis, typical of the partial equilibrium analysis. According to this criterion, changes in the quantity of commodities produced in a single industry could be overlooked only if they had not generated variable returns to scale with direct effects on the technical coefficients of other industries, since this would have necessarily resulted in price and income distribution changes (see Sraffa 1925a:326–7). It was a flexible criterion, which made it possible to assume a restricted margin of interdependence among industries, and did not require, or prevent, the assumption of constant costs.

The second objection raised by Sraffa concerned the assumptions implicit in Marshall's analysis of partial equilibrium, because of its recourse to the *ceteris paribus* clause. They implied that production costs and the level and composition of the social product could be determined independently by output prices, and that the supply curve of each commodity was unrelated either to those of other commodities or to the demand curve. Sraffa held that, in general, such assumptions could not be satisfied, because any small change in the quantity of commodities produced by a given industry could affect the production costs of other industries. One should rather recognise the interdependence relations of a technological nature which linked different industries on the supply side.

The third objection that Sraffa raised against Marshall's theory of value was that, in a free competition context, an increase in production costs due to the presence of a limiting factor would be borne by each of the industries that made use of such a factor; but not necessarily by each firm, since each individual producer could increase or reduce the quantity of the scarce factor used, without substantially affecting its price,[20] a state of things which is clearly inconceivable for all producers as a whole.

This line of reasoning prevented Sraffa from conceiving the aggregate supply curve of an industry over a short period as a horizontal sum of the individual supply curves, as Marshall had done. On the other hand, it became even more difficult to accept the assumption of constant prices for all other commodities–a typical assumption of a partial equilibrium analysis–as some of these commodities were likely to require precisely the use of that scarce and irreplaceable factor whose price was susceptible to increase.

As far as the case of diminishing costs due to external economies was concerned, a case logically admissible within Marshall's theory, Sraffa

considered it to be a 'purely hypothetical and unreal construction' which would lead to the same result. He thought that diminishing costs could not be presupposed in the construction of an industry supply curve, on account of the difficulty in summing individual cost curves whose shapes would change with variation in the quantities produced at the industry level.[21]

The real reasons for Sraffa's criticism of Marshall

A number of Sraffa's interpreters have wondered what led him, in 1925–6, to take Marshall's version of the neoclassical theory of value–or one of its current *vulgata*–as the main butt of his criticism. That is to say, to refute precisely the version of the dominant theory that aimed to mediate between the classical and the marginalist viewpoint,[22] rather than other more canonical versions of that doctrine.

Some of the answers that have been put forward to this question in the literature are rather perplexing. It has been suggested that Sraffa may have criticised Marshall's doctrine either because at that time Marshall was very popular in Italy, where he was considered the most important representative of neoclassical economics;[23] or out of misconceived career motivations (curiously described as reasons of 'academic policy').[24] It has also been claimed that the idea of demonstrating that Marshall had not succeeded in abandoning Ricardo's theoretical model may have appeared rather attractive to a young critic of marginalism, impatient to get ahead and be noticed and understandably sensitive to the assessment of his work within the academic world.[25]

Yet Marshall's theoretical vision was no longer predominant in Italy in the mid-1920s. As Sraffa himself recalled, reservations, restrictions and exceptions to Marshall's theory had long been present in notes and articles, even though an overt criticism of the theoretical construction of the English economist was carefully avoided.

With remarkable modesty, Sraffa presented his 1925 essay as a mere attempt to coordinate the pre-existing critical material. It is therefore difficult to imagine that he chose Marshall's theory of competitive price as the main target of his criticism merely because he thought it would represent the ideal subject for a scientific scoop.

Other interpreters have preferred to adopt an approach that does not cast doubt on the young Sraffa's intellectual honesty, pointing instead out purely analytical reasons. Thus some have appealed to the clear advantage, recognised by Sraffa, of addressing the research to a solution of partial equilibrium of imperfect competition, rather than moving towards a far more complex solution of general economic equilibrium.[26] Others have spoken of the possibility of thereby avoiding an unnatural mix of objective cost functions and subjective demand functions.[27]

In my opinion, Sraffa had two specific reasons for criticising Marshall. The first is that he believed Marshall to have made improper and historically

distorted use of Ricardo's thought on the issue of value.[28] From this point of view, I think Sraffa's criticism of Marshall for not assuming constant returns in perfect competition implicitly tended to defend Ricardo, giving Marshall a dose of his own medicine.

Marshall had indeed offered a somewhat simplified interpretation of Ricardo's theory of value, presenting it as an explanation of price based on real production cost, under constant returns to scale.[29] Sraffa intended to show that once Marshall's theory was reformulated in a consistent manner, it implied a constant average cost, just as did Ricardo's theory.

So far, on Sraffa's part, there was neither an explicit acceptance of Marshall's approach to the theory of competitive price, nor a definitive rejection of it. Sraffa's behaviour in the mid-1920s revealed his concern to leave a door open to opportunity, which might allow Marshall's approach to be revived, once it had been clarified that his theory could not be interpreted as a completion of Ricardo's approach.[30] Only later, during the 1930 debate on increasing returns, did Sraffa make a more decisive rejection of Marshall's method of partial equilibrium analysis and of his theory of competitive price.

On the issue of returns to scale, both Marshall and Sraffa failed to interpret Ricardo correctly, as the latter had spoken of freely reproducible commodities, not of commodities produced at constant costs.[31]

I think Sraffa felt Marshall had sought to deprive Ricardo's theory of value of its most typical feature, namely the fact that in the long period it focused only on supply, and to encompass it within a more general analytical context that would also cover shorter periods of time, in which price determination would involve an equilibrium between demand and supply.

Sraffa saw Marshall as the author of the erroneous doctrine which stated the 'fundamental symmetry of the general relations in which demand and supply stand with respect to value'. A doctrine

> conditioned by the non proportionality of total production cost to the quantity produced: if the production cost of each unit of the commodities considered did not change with variation in the quantity produced, the symmetry would be interrupted, the price would be exclusively determined by production costs and the demand could not affect it at all.
> (Sraffa 1925a:320)

Basically, then, Sraffa considered Marshall's theory of competitive price as an unjustified and insidious attempt at reformulating Ricardo's doctrine in the neoclassical language of market equilibrium.[32] In his opinion, Marshall had purposely and surreptitiously overturned the main theoretical results which Ricardo had achieved.[33] I am thus advancing here a crucial historiographic hypothesis on which my interpretation of Sraffa's theoretical work in the mid-1920s will rest or fall.

The two articles of 1925 and 1926, and some of their interpretations

Some commentators have maintained that Sraffa's 1925 and 1926 articles are very similar to one another ('twin papers'), suggesting that the second is little more than a synthesis of the first. In my opinion, on the contrary, they display a number of significant differences.

In his 1925 article, and in preparatory notes discussed with his friend Raffaele Mattioli, Sraffa examined some formal contradictions in Marshall's analysis of the equilibrium of the firm and industry under perfect competition. In essence, he there explained three things:

- why he felt he could not accept Marshall's statement that there was a 'fundamental symmetry of the general relations in which demand and supply stand with respect to value' (Sraffa 1926a:535) in the theory of competitive price;[34]
- why he regarded as inadequate an analysis of the theory of the firm carried out along the lines of partial equilibrium and grounded on that symmetry premise;
- why he intended to support validity of the classical approach to the theory of long-term competitive prices, an approach which denied that symmetry.

Sraffa's main thesis was that, in the study of relations between cost and quantity produced of an individual commodity, one was faced with a basic methodological alternative. Either one could abandon the assumption of perfect competition, implying production at constant costs–something to be considered as an exception from an empirical point of view–and replace it by another, less restrictive hypothesis, or else he should give up Marshall's method of partial equilibria, which allowed 'only a first approximation to reality' and did not seem to be capable of reconciling the need for logic consistency with the requirements of realism.

In Sraffa's opinion, the supply curve of a firm and the corresponding demand curve were not independent from one another (even in the absence of advertising and selling expenses, which he did not include among production costs). The same reason that led Marshall to attribute to the firm's supply curve an increasing upper portion–that is, the probable rise in the rental price of certain production factors as a consequence of the expanding volume of production–likewise caused Sraffa to believe that if a similar phenomenon had also occurred in other industries, the demand would have been affected in such a way as to make it impossible to determine the equilibrium of the competitive firm, due to the temporary shift in the two curves of demand and supply.

Significantly, the 1925 article concluded with a criticism of Marshall's method of partial equilibrium for not allowing the interdependencies to be taken into account. The method, Sraffa contended, could be applied only to

small variations in the quantity produced. That is, only to changes that did not substantially modify the general framework of the analysis. Large changes would have resulted in considerable variations in the prices of factors used in other industries as well, a circumstance that would necessarily have required the abandonment of Marshall's partial equilibrium analysis.

In the 1925 article Sraffa contested Marshall's attempt at building a supply curve of industry that would coordinate several different tendencies of factor productivity under a single 'law of non-proportional costs'. He examined a series of analytical problems affecting Marshall's analysis of partial equilibrium; showed that, from a logical point of view, the hypothesis of equilibrium of the perfectly competitive firm was indissolubly linked to production at constant costs;[35] and pointed out that the shape of Marshall's supply curve implied that the increase in aggregate demand concerned only one commodity, with no external effects on more than one industry.

No new theory of price, however, was contained in Sraffa's 1925 essay. It simply proposed a return to the idea of a supply curve at constant costs. From this point of view, the 1926 article–which Sraffa considered to be a sequel, rather than a mere summary, of the previous one[36]–was certainly more significant. Not only did it suggest abandoning the usual assumption of perfect competition in favour of a revival of the less constraining classical assumption of free competition,[37] but it also proposed to apply to the latter analytical context the formal apparatus of monopoly theory. It thus introduced an important innovation, which was to lead to a radical review of the concept of industry, laying the basis for its subsequent identification with the market of the products of each individual enterprise.

Sraffa's proposal was to build a new theory of price for substantially similar, but not identical, commodities. The hypothesis was that such commodities were offered at variable unit costs by quite a number of firms competing with each other in price and product quality. Each firm was assumed to be facing a diminishing demand curve, more elastic than in a monopoly. A circumstance which would limit the expansion of supply by each individual producer.

The innovative nature of this proposal was immediately grasped in Cambridge, not only by the two editors of the *Economic Journal,* Keynes and Edgeworth, but also by Pigou and by Kahn and Joan Robinson, two exponents of the younger generation of Keynesian scholars, still linked to Marshall's approach to price theory.

Later on, however, some of the Italian interpreters of Sraffa cast doubts on the real importance of his 1926 article. Following Schumpeter, they judged it to be of inferior quality as compared to his preceding essay[38] and described it as a deviation from Sraffa's previous line of reasoning.[39]

The most scathing judgements on the article considered its first part as a mere reformulation–the 'English version', requested by Keynes–of the previous essay, and its second part as a temporary abandonment of the line of

thought envisioned in 1925. This supposed change of direction was ascribed to the desire to show the logical possibility of a competitive equilibrium of the firm compatible with the assumption of increasing returns to scale.

The deviation was regarded as unacceptable for several reasons: first of all, because it tended to carry out 'a theoretical operation that was totally contained within Marshall's vision of the industry equilibrium of a single product' (Talamo 1976:63); second, because it seemed to make concessions to demand and consumer preferences (see Graziani 1986:191); and third, because it infringed the rules of the game by introducing hints of daily experience and businessmen's opinions into a theoretical framework.[40]

It should be noticed that all these interpreters had some sympathy for Sraffa's work as a whole, but it was precisely for this reason that they felt it to be their duty to go beyond, or even against, the statements expressed in his 1926 article. They were probably convinced that by so doing they were working in Sraffa's own interest, raising the overall coherence of his scientific programme, which, in their view, did not imply a theory of price where demand played a significant role.

These developments resulted in a curious *conventio ad excludendum,* which led many Sraffian scholars to ignore his 1926 article, as if it had never been written or consisted merely of an English-language summary of the previous essay. Moreover, there has been a strong tendency to trace a substantial line of continuity between the theoretical approach adopted in Sraffa's 1925 article and that contained in his 1960 book.

Sraffa's second article was thus passed over almost in silence. The spotlights remained focused on his first essay, even though Sraffa himself made it clear that it no longer adequately expressed his thought (to the point of preventing its publication in an English version).

The outcome of this process has been a systematic misunderstanding of the meaning of the 1926 article, which was ultimately considered by these interpreters of Sraffa's thought as a bungled attempt, which ended up in a blind alley, to move away from his previous theoretical line.

Samuelson's frontal attack and its effects

One of the main conclusions drawn by Sraffa in the two articles we have just examined is that the production cost of the commodities offered by a perfectly competitive firm must generally be seen as constant with respect to small changes in the quantity produced, 'as we are not entitled to take into consideration the causes which may make it rise or fall' (Sraffa 1926:541).

This conclusion was opposed by Samuelson, who held it to be an ideological statement, not deducible from Sraffa's analysis and flawed by a simple, but fatal error: the failure to recognise that the condition of static equilibrium of a perfectly competitive firm requires an increasing supply

curve, so as to preclude indefinite expansion of the business production scale.[41]

So far, the arguments produced in defence of Sraffa against this frontal attack on his theoretical construction have not been fully convincing. The main point, raised by Garegnani, was that the rule recalled by Samuelson would be valid only under conditions of general economic equilibrium, where the production-possibility frontier for an industry, in the presence of limiting factors (such as land), would assume a concave shape, expressing increasing unit costs. Whereas Sraffa had objected to the Marshallian use of such supply curves in a more restrictive partial equilibrium context–namely in a model with a single homogeneous primary factor, labour, implying constant unit costs and a linear production-possibility frontier. In the presence of scarce land, however, or dishomogeneous labour, Marshallian rising supply curves would be justified.

The logic on which this line of defence was founded was rather weak, for it referred to Sraffa's treatment of the matter as if his assumption of constant unit costs were only a first approximation hypothesis, used for analytical convenience and destined to be subsequently abandoned.

Sraffa's intention was to show not the lack of empirical content in Marshall's theoretical construction (as Clapham had tried to do), but rather the restricted nature of Marshall's predicative field, which implied a constant supply curve for the industry, logically incompatible with a symmetrical theory of value. Once this result had been achieved, there was no further motive for Sraffa to proceed along this line of reasoning, instead of recognising the existence of a more general functional relationship between cost and quantity produced. Indeed, that is what he ultimately did.[42]

Sraffa believed that Marshall's method could coherently account only for two special and highly unlikely cases of variable costs by a competitive firm. Therefore he thought that, *as a first approximation,* one could assume as normal the case of a supply curve at constant costs:

> The low probability of the hypotheses that give rise to each of the tendencies to cost variability seems to suggest that the absence of both is to be considered much more general–given the conditions of partial equilibria–than the presence of only one of them. Thus the most appropriate approach is to regard as normal the case of constant costs, rather than that of increasing or diminishing costs.
>
> (Sraffa 1925a:316)

But Sraffa knew very well that, in a further approximation to reality, it would become 'necessary to extend the field of investigation so as to examine the conditions of simultaneous equilibrium in numerous industries' (Sraffa 1925a:541)–which he did in his 1960 book–and to take into account the circumstances which could result in external economies (as he had done in his 1925 essay).

As a matter of fact, in the closing sentence of his 1925 essay, Sraffa had stated that from the point of view of the equilibrium of a single industry, 'which is only a first approximation to reality, it must be admitted that the commodities, in general, are produced under constant cost conditions'. Later, however, he came back to the same point and recalled that in 1925 he had intended to demonstrate that, in general, only the case of constant costs could be considered as logically compatible with the assumption of perfect competition, without mentioning that it was a first approximation hypothesis.[43]

Thus Samuelson's criticism–his ironic reference to a 'one-leg theory of price' lacking the demand side–has a sound basis as far as this aspect is concerned. In attempting to re-launch the classical theory of prices of production, Sraffa actually restricted his attention to a special case of the theory of value,[44] a case which he legitimately dealt with as a first approximation to the problem. He then neglected to remove this initial assumption, and seemingly went on in the construction of his theory of prices almost forgetting its obtrusive presence.

A second line of defence of Sraffa's work sought to provide a more specific textual basis for opposition to Samuelson's attack, by recalling that Sraffa's aim in 1925–6 was not to deny any role of demand in the determination of competitive values, but rather to single out, among Marshall's supply functions, those that were endowed with a rigorous foundation and those which were not.[45] It was also maintained that Samuelson's interpretation of Sraffa's critique of Marshallian supply functions did not give a correct account of the issue, failing to recognise that Sraffa had ultimately abandoned the Marshallian partial equilibrium approach in favour of a more general analysis of simultaneous determination of long-term competitive prices.

Samuelson was definitely wrong when he contended that Sraffa ignored the fact that a competitive equilibrium of the firm requires an increasing supply curve. This issue is grounded on a substantial misunderstanding of the contents of Sraffa's 1925 article–a paper which had not yet been translated into English and did not make for easy reading.

In his 1925 essay Sraffa had indeed warned his readers against the risk of making precisely the 'fatal mistake' that Samuelson later attributed to him:

> In the perfectly possible case that the individual marginal cost were constant for some or even for all the quantities of product, in the part concerning such quantities the marginal cost curve would correspond to the average cost curve; and within these limits the equilibrium would be indeterminate, given the definition of competition that we have followed so far…. Under such circumstances, if the unit cost curve is constant for a given tract, equilibrium will be achieved at the point corresponding to the

> maximum quantity which can be produced at that cost; *and it will no longer be admissible to claim that the curve is at constant costs throughout, as this would lead to the monopoly of the firm considered.*
>
> (Sraffa 1925a:311; italics added)

Sraffa never held that constant cost cases exhaust the categories of admissible competitive prices, as claimed by Samuelson (1987). Indeed, he argued exactly the opposite: that two cases of variable unit costs, both of them consistent with the assumption of perfect competition, were theoretically conceivable.

Real, presumed and missing influences on Sraffa's work

After his 1926 article, where imperfect competition was envisioned, Sraffa began to consider the idea that in order to re-propose the 'the old and now obsolete theory' of price based on the real production cost–which he still regarded as the best available–he should follow a different path, that based on the analysis of multisector linear models of production. This type of analytical approach was later to become known as the neo-Ricardian approach. It drew on a solid theoretical background: that of the Russian-German school of mathematical economics.

The main representatives of that school–Dmitriev, Tugan-Baranowsky, Bortkiewicz, Struve, Charasoff–had attempted to explain the process of determination of the normal profit rate by combining Ricardo's analysis of value and distribution with the Walrasian theoretical system and with a revised version of Marxian reproduction schemes, criticised by Böhm-Bawerk.

Those scholars had undeniably achieved some important theoretical results. Dmitriev had shown that, given the technical conditions of production and the real wage rate, the relative prices of commodities could be determined by two distinct analytical methods, together with the uniform profit rate, which depended exclusively on the technical conditions of production of the vertically integrated wage goods subsystem. For an economy where each commodity was produced by a separate industry, he had identified the inverse functional relation between wages and profits, which plays a fundamental role in Sraffa's solution to the problem of choice of techniques.

Bortkiewicz, in turn, had formally solved the Marxian problem of transformation of values into prices, both by using for this purpose Dmitriev's equations of reduction of prices to dated quantities of labour and by resetting Marx's extended reproduction schemes correctly, so as to determine relative prices and the profit rate simultaneously. Charasoff had then generalised to n commodities Bortkiewicz's solution to the problem.[46]

The names of these scholars are not recalled among Sraffa's bibliographical sources, where only Marx and a few classical authors are

mentioned. Unfortunately, little is known about the range of works read by Sraffa in the 1920s and early 1930s. We are therefore confronted with a delicate historiographic question: whether the young Sraffa, who had an interest in Marxism and could read German, was at that time already acquainted with the 1905 critical essay by Tugan-Baranowsky on the theoretical foundations of Marxism and with two famous studies by Bortkiewicz on Marx's theoretical system, published in 1906–7, both of which contained extended references to Dmitriev's essay on Ricardo's theory of value and distribution and to the work of Tugan-Baranowsky.[47]

By replacing in Bortkiewicz's Marxian transformation model the quantities of labour with the corresponding quantities of commodities included in a physically specified subsistence real wage, we get the price model without labour coefficients described in the first two chapters of Sraffa's book.[48]

By the early 1940s, Sraffa was certainly familiar with these writings.[49] By that time various contributions had been published in English on the subject (by Sweezy, Dobb, Winternitz and May). It is not easy, however, to ascertain whether Sraffa was acquainted with these works of the Russian-German school when in 1928 he submitted a preliminary draft containing the central propositions of his theory of prices to Keynes. Anyway, in his analysis of the determination of prices of production, Sraffa went further than the neo-Ricardian economists of the Russian-German school, as he succeeded in demonstrating that relative prices of commodities can be determined without even passing through the intermediary of values.[50]

Let us now explore another significant issue: why Sraffa's long intellectual fellowship with Keynes did not exert any substantial influence on the theoretical work of the Italian economist. This lack of influence may indeed appear strange if one considers that Keynes and Sraffa were both deeply committed to a critique of the dominant neoclassical paradigm, and both took the relationship between special cases and the premises of economic theory as the butt of their criticism. Their critical targets thus coincided, but the levels of abstraction on which the two authors had chosen to operate were different.

Furthermore, Sraffa was mainly interested in a long-term issue, namely the relation between prices of production and the distribution of the social product. In his theoretical system, final demand played no role; neither did money.[51] Keynes, on the other hand, was pursuing a typical short-term issue, where the role of demand was most important: that of determining the overall level of utilisation of resources, when the production capacity of the economy was taken as given.

Sraffa's attention focused mainly on testing the internal consistency of the dominant theory. He criticised it on purely logical grounds.[52] Keynes, in contrast, put forward an external criticism, centred on the irrelevance of the basic neoclassical assumptions for a correct understanding and interpretation of reality.

Sraffa, as we know, was closely linked to the Ricardian-Marxian theoretical tradition. Keynes, who was a liberal and had a much more pragmatic attitude, found little he could share therein. His political project strove to achieve internal reform and improvement of the very capitalist system that Sraffa would perhaps have preferred to replace with a new social order. Yet, in spite of these divergences, scientific collaboration between the two scholars might have been attained. The introduction of the Keynesian principle of effective demand into a pattern of long-term analysis of the Ricardian type might have made it possible to establish an organic link between price theory and the Keynesian theory of income and employment.

The highest point of scientific relations between the two scholars was Sraffa's contribution to an ongoing debate between Keynes and Hayek on the theory of capital, in 1932. It played a decisive role not only in determining the outcome of that debate, but also in orienting Keynes' subsequent thought on the theory of investment.

Soon after this episode, Sraffa fell into a twenty-year period of silence, right up to 1951, when his introduction to Ricardo's *Principles* was finally published. During that long period of time, Sraffa had no teaching duties. His role as head of the Marshall Library and assistant director of research for economic studies must therefore have left him considerable time to devote to his studies. He was, however, engrossed in preparing the critical edition of Ricardo's works, which kept him engaged up to 1955.

At a certain point his undertaking began to seem never-ending, partly due to the difficulty of locating Ricardo's letters and partly to the task of writing the introductions. Throughout the last few years of that period Sraffa was also working on the subject of prices of production.[53] He was eventually almost overwhelmed by this combined effort. Maurice Dobb's contribution was of decisive importance for the accomplishment of the editorial work.[54]

If one were to reach the conclusion that the prolonged break in Sraffa's scientific production was due to his great commitment of time and effort to the famous critical edition of Ricardo's works and correspondence, one may perhaps wonder if Keynes had had a really good intuition when in 1930 he proposed to the Royal Economic Society to entrust Sraffa with a task which would divert his attention from more creative activities for well over a quarter of a century.

Some concluding remarks

Our re-reading of Sraffa's early theoretical works has focused on the examination of four controversial aspects: (i) the reasons underlying the criticisms in the 1925 article against Marshall's theory of value; (ii) his 1926 decision to suggest a different line of analysis, nearer to the case of monopoly; (iii) his early abandonment of the idea of constructing a theory of

imperfect or monopolistic competition; (iv) his subsequent decision to concentrate his attention on a simple model of production of commodities, suited for simultaneous determination of all relative prices.

The outcome of this study confirms the basic coherence of Sraffa's theoretical work, which aimed at reviving the classical explanation of long-term competitive prices based on real production costs, relieved from the useless burden of the labour theory of value and from the presence of neoclassical elements of distortion.

Within the seemingly linear trend of this theoretical itinerary, a single methodological turning point of a certain significance may be noted. It took place when Sraffa–soon after his 1926 article, which had opened the way to the theory of imperfect competition–unexpectedly refused to continue to move in that direction, realising that it would imply a return to Marshall's criticised analysis of partial equilibrium and his symmetric vision of price determination.

After that abandonment–perhaps a little premature, but due to comprehensible methodological reasons–Sraffa's interest in purely abstract theory addressed itself to the study of a circular process of production, in which the same commodities appeared as products and as means of production. Faithful to his plan of re-launching the project of an objective theory of value entirely grounded on the real cost of production, he gave up the idea of a joint determination of all prices and outputs and focused his attention on a much simpler problem which concerned the construction of a theory of relative prices when the instantaneous production configuration of the economy was assumed as given. By this assumption, any functional link between supply and demand was severed right from the beginning.

We do not know whether Sraffa was fully satisfied with that solution, or not, but he did not seem to regard it as sufficiently pervasive, as we may guess from the fact that in the preface of his 1960 book he mentioned his intention to carry more deeply and extensively the critical part of his research programme forward, or to delegate that task to 'someone younger and better equipped'. Provided–he added–that the foundation of his theory of prices would hold up.

Today, forty years on, there is evidence that the base laid by Sraffa has only partially resisted the test of time and experience. It has provided sufficient support for a critique of the most aggregate version of the neoclassical theory of value and distribution. But it has neither brought about a definitive abandonment of the general equilibrium versions of that theory, nor promoted a suitable revival of the classical approach to the problem.

As a matter of fact, Sraffa spent a great amount of his intellectual energy in the attempt to develop the theoretical implications of a first approximation assumption which could not be removed without seriously damaging his basic thesis. Ultimately, he ended up by postulating much of what he intended to prove.

His method of analysis–by distinct but coherent logical stages implying specific assumptions–involved the risk of misunderstandings. On the one hand, supporters of the theoretical approach of neoclassical synthesis sought to reabsorb his thought into the dominant tradition (as had already occurred with Keynes' theory) by looking at his theory of prices as a particular, and somewhat irrelevant, case of the more general Walrasian model. On the other hand, neo-Marxist scholars found difficulties in interpreting his attitude towards the labour theory of value, the origin of profit and the possibility of overcoming the basic contradiction between bourgeois and Marxist economics.

On the whole, there was a constant overburdening of Sraffa's line of reasoning with the idea that he was aiming at a global reconstruction of economic science. In my opinion, such an idea was fundamentally extraneous to Sraffa, who had purposely and unpretentiously limited his attention to a few theoretical cases (that he regarded as particularly suited for making 'intellectual experiments').

Notes

1 The author thanks Neri Salvadori, the discussant of this paper at the Turin meeting in October 1998, for some helpful comments.

2 In the spring of 1923, when he was in France, Sraffa read, or reread, Marshall's *Principles of Economics* and jotted down a few critical notes about some of its passages, as testified by a notebook bearing the date of April of that year, preserved together with all *Sraffa Papers* (henceforth *SP*) at the Wren Library of Trinity College, Cambridge (*SP*/D1/2). In November of the same year, Sraffa adopted Marshall's manual as a reference text for his course in political economy at the University of Perugia.

3 Marshall refused to regard his theory as a sort of compromise (see a letter to J.B. Clark dated 1908, in Pigou 1925:416–18). He maintained that it was a general theoretical construction, within which the two opposite theories of value based on real production cost (Ricardo) and utility (Jevons) could be encompassed as particular cases. In his view, each of these two theories was correct in what it stated, but incorrect in what it denied.

4 These were the hypotheses of variable unit production costs and of the fundamental symmetry in the general relations in which demand and supply stand with respect to price. See Sraffa (1925a:280).

5 Marshall interpreted Ricardo's theory of value as grounded on the assumption of constant unit costs, which excluded any role for demand.

6 See a letter written by Sraffa to Keynes, from Milan, dated 6 June 1926, kept in the *Keynes Papers,* partly reported by Roncaglia (1975:17–21).

7 Among the *Sraffa Papers,* there is a note remarking on the existence of a great lack of understanding between his contemporaries and classical economists, in spite of the simplicity and explicitness of the language the latter used (*SP* D3/12/4:14).

8 See Clapham (1922). The English historian held that some of Marshall's analytical categories were no more than 'empty economic boxes', useless for practical purposes. That article started a debate with Pigou.

9 Sraffa thought that the hypothesis of increasing returns could be explained by the technical division of labour within industry and regarded diminishing returns as related to the specific nature of agriculture (See note *SP* D1/43:33–34).

10 According to Sraffa, what really prevented a competitive firm from expanding production indefinitely was not the presence of increasing cost, but the limited extension of demand, expressed by a price curve with negative slope.
11 In his 1926 article, Sraffa himself underlined that this was the 'method indicated by Marshall to the manufactures designed for particular tastes' (Marshall 1961 [1890]: Book V, Chapt. 12, Par. 2), 'the very same as that followed in cases of ordinary monopoly' and did not make it possible to sum the particular curves of individual firms 'so as to form a single pair of collective demand and supply curves' (1926a:546).
12 It is interesting to notice that before the publication of his 1925 article, Sraffa had discussed with Maurice Dobb a note containing some of his ideas concerning imperfect competition, not included in that essay. They appeared in a re-elaborated form in the 1926 article, upon the recommendation of Dobb himself.
13 A set of 220 pages of Sraffa's lecture notes, which basically reproduce material taken from the 1925 and 1926 articles, is preserved in *SP*/D2/4.
14 Two years later, at the conclusion of the debate with Robertson on increasing returns, Sraffa asserted with emphasis that Marshall's theory of value 'should be discarded' (Sraffa 1930:93). But, significantly, he did not specify whether this should be done in favour of Cournot's point of view or that of Pareto.
15 As Sraffa himself recognised, Marshall can be legitimately regarded as a forerunner of the modern theory of imperfect competition. Schumpeter considered Marshall as the father of that theory.
16 See Panico (1991:560–61). Similar ideas were also expressed by Napoleoni (1964:175–6), andTalamo (1976:60, 65, 73–4, 84).
17 Sraffa did not make clear the reasons which led him to change his mind on the appropriateness of applying the static analysis of partial equilibrium to the theory of imperfect competition. One reason that most naturally comes to mind is the lack of plausibility of the assumption that a firm knows what kind of demand curve it is facing.
18 The scarcity of studies on Sraffa's method is surprising and contrasts with the abundance of bibliographic references on the methods of Keynes, Hayek and Schumpeter. For an interesting work on the subject, covering the 1920s, see Signorino (1998).
19 See Sraffa (1925a:320). Let us note that in his 1960 book Sraffa adopted precisely the definition of industry as the only producer of a given sort of commodity that he had previously criticised.
20 See Sraffa (1925a:287). Sraffa held that, assuming rational behaviour by the firm in the presence of a constant factor, returns could be increasing only if such a factor were indivisible.
21 See Sraffa (1925a:306). Among the *Sraffa Papers* there is a note by him (*SP* D1/32:21.2), probably dating from 1927, emphasising that the analytical tool of the collective supply curve could never be applied to a real industry.
22 Sraffa himself had noticed this aspect some months earlier in his obituary for Maffeo Pantaleoni (Sraffa 1924:648–53), where he maintained that in Pantaleoni's treatise of economics the theories of classical economists were harmonised with those of the marginalist economists, 'in line with Marshall's teachings'.
23 Marshall's theory, in spite of Pareto's opposition, had spread rapidly in Italy through the work of two groups of scholars, namely the Roman group that clustered around Pantaleoni, Barone and Ricci and the Turin group centring around Einaudi, Jannaccone, the Cognetti de' Martiis Laboratory of Political Economy and the review *La Riforma Sociale*.
24 Such reasons were hinted at by Talamo (1976:64), in a remarkable critical essay

on the interpretation of Sraffa's thought where a supposed 'necessity to enter into the Marshallian head of 1925 economists with clamour' was recalled.

25 See Becattini (1986:42–3). My interpretation is different, being based on the assertion that Sraffa had no qualms about using harsh words towards the academy, as he had openly spoken out against the techniques used to hush up the 'scandal' of raising doubts on the empirical foundations of Marshall's supply curve (Sraffa 1926a:536).

26 Sraffa (1926a:540). See, for instance, Panico (1991:560), who held that Sraffa had chosen the first solution for 'pragmatic reasons' of analytical nature.

27 See Roncaglia (1975:23), who, in this regard, mentioned a 'hybrid of irreconcilable objective and subjective elements'.

28 See Roncaglia, (1991:377), according to whom 'Sraffa's first critique concerns Marshall's distorted interpretation of Classical (particularly Ricardo's) analysis', and Groenewegen (1991:82), in whose opinion Marshall 'transformed these classical ideas into what he wanted them to be in order to heighten the degree of resemblance of his own notions'. A similar criticism had been levelled against Marshall by W.Cunningham and W.J.Ashley.

29 In an appendix to the *Principles,* Marshall had accused Ricardo of not having made the hypotheses explicit that had led him to treat the particular case of production with fixed technical coefficients as a sufficiently general case.

30 See, for instance, a note by Sraffa (*SP* D3/12/7:114), where he mentioned the possibility of freeing Marshall's theory from all subjective elements.

31 Garegnani has recently claimed that Sraffa's unpublished papers show a gradual evolution of his views on the classical economists, which probably led him, starting from 1927–8, to 'abandon the Marshallian interpretation of the classical economists, thus turning his back on the position that underlay his 1925–26 articles' (Garegnani 1998a:152). In my opinion, in the mid 1920s there was but one of the ideas contained in Marshall's interpretation of Ricardo that Sraffa shared–and it happened to be an erroneous idea, namely that Ricardo thought that most of the commodities exchanged daily on the market were produced at constant costs (see Sraffa 1925a:316).

32 Sraffa believed that Marshall had conducted his attempt rather insidiously, without declaring it explicitly, but claiming to be a follower of the classical tradition who was simply 'translating' Ricardo's thought into mathematical formulae.

33 In his 1925 essay, Sraffa had stated that Marshall had ingeniously concealed a radical change of approach that had come about in his thought in the 1880s, on the laws of non-proportional costs and the role of external economies. 'Those laws have been replaced and Marshall has been extremely clever in pushing this transformation through almost unnoticed' (Sraffa 1925a:306).

34 Sraffa held that Marshall's demand and supply curves originated from a false similarity with mechanics, a science where experiments can be repeated in substantially identical conditions (see *SP* D3/12/42).

35 'Either we take those variations [in costs and quantities] into consideration for all the industries of the group, and then we have to shift from the specific equilibrium of a certain commodity to general equilibrium, or we neglect those variations in all industries and then the commodity examined must be considered as produced at constant costs' (Sraffa 1925a:325).

36 See, to this regard, Sraffa's letter to Keynes dated 6 June 1926, already referred to.

37 Free competition does not imply any atomistic subdivision of demand and supply, or any perfect transparency of the market, but only free entrance, output homogeneity and a uniform profit rate in the long term.

38 See Roncaglia (1975:20n.); Becattini (1986:39); Maneschi (1986:11); etc.

39 See, for instance, Talamo (1976:63 and 65), who regarded the 1926 article 'not as a complete theoretical proposal suggesting an alternative to the dominant approach, but rather as a simple diversion down a side road', which led Sraffa 'into a dead end'.

40 This was deemed to be all the more serious because it occurred 'with a direct, …almost brutal, appeal to the concrete reality of the market and industry' (Becattini 1986:39). The conclusion was that 'only the 1925–26 article and a half had to be considered (ibid.: 40).

41 Samuelson (1987:458; 1990:269). This line of reasoning implied the idea that the difficulties found in Sraffa's theory did not merely consist in the fact that it only dealt with a single case, but in the specific nature of such a case, which by hypothesis excluded any possible effect of variations in demand on output levels and prices of production.

42 In his letter to Keynes dated 6 June 1926, Sraffa wrote: 'Although I believe that Ricardo's assumption [constant returns] is the best available for a simple theory of competition (*viz.* a first approximation), of course in reality the connection between cost and quantity produced is obvious.' However, he held that this connection was a modern idea, unknown to the classical economists, who assumed constant costs (see *SP* D2/4 3:79).

43 'The temptation to presuppose constant returns in economic theory is not entirely fanciful. It was experienced by the author himself when he started on these studies many years ago–and it led him in 1925 into an attempt to argue that *only the case of constant returns was generally consistent with the premises of economic theory*' (Sraffa 1960a:vi; italics added).

44 Just as the case had been sixty years earlier for Pigou, who, in a letter to Sraffa dated January 1928 (*SP* C 239), considered Sraffa's equations of production of commodities in constant cost conditions a 'special case' of the general theory.

45 See Panico (1991:556). This distinction is inferred from the fifth and last section of Sraffa's 1925 essay, where he stated that his aim was to identify the real situations that were logically compatible with Marshall's model of supply curve and those which were not. On that point, see also Sraffa (1926a:536).

46 Thirty years later, a second generation of the Russian-German mathematical school made wide use of circular models of production. It included Leontief and von Neumann, two authors who considerably influenced Sraffa's thought.

47 Dmitriev was known in the English-speaking world, because in 1931 Bortkiewicz had devoted an entry to him in the *Encyclopaedia of the Social Sciences,* published by Macmillan.

48 The only difference between the two approaches is that Bortkiewicz's system of equations classically assumes that wages are paid at the beginning of the production process, whereas Sraffa's system of equations is based on the opposite assumption that wages are paid *post factum.*

49 Among Sraffa's papers, there is a notebook (*SP* D1/91), started in 1943, with comments on some passages by Bortkiewicz and Dmitriev.

50 Sraffa's analysis yielded also other original results, such as the distinction between basics and non-basics, the 'auxiliary construction' of the standard commodity and the method of subsystems.

51 I dealt more extensively with these aspects in a previous paper (Cavalieri 1984).

52 In Sraffa's opinion, Marshall's theory of value could not be interpreted in such a way as to endow it with internal logical consistency and, at the same time, make it compatible with the events that it aimed to explain (see Sraffa 1930a:93).

53 There is however considerable evidence indicating that Sraffa did not resume his work on a neo-Ricardian theory of value until 1951.

54 It is well-known that in the last few years he was assisted by Maurice Dobb, whose contribution played a prominent role in finishing the work.

Comments

6 On Marshall's representative firm

A comment on Marchionatti

Tiziano Raffaelli

Marchionatti's chapter offers a perceptive survey of the Marshallian debates in the 1920s which helps to place Sraffa's articles in a context much broader than usual. In particular, the chapter convincingly stresses the relevance of American contributions, often largely ignored. Given the cluster of problems relevant to those debates, it is fair to notice that depth of historical analysis is not sacrificed to broadness of outlook.

The wide spectrum of the issues raised in the articles under review, issues not always convergent and dealt with–or ignored–from different perspectives, can be called to mind by simply mentioning some of the main ones: the theory and concept of competition, the relationship between industry and firm (distribution of production between firms of different size and age), normal profit as a component of price, the statics-dynamics and (sometimes identified with it, perhaps by Marchionatti himself) mechanics-biology relationships, the realism of economic theory and, if we take into account Young's contribution, economic progress itself.

Increasing and decreasing returns, the representative firm and Marshall's theory of value provided little more than the crossroads where different lines of thought met, often, as in Sraffa's case, to follow their own direction. Marchionatti's main achievement is to consider all the incoming branches of the crossroads. I will limit my comments to one of the traffic-lights, the representative firm, being conscious that the whole subject deserves closer attention by historians of economic ideas.

After more than 100 years (whatever date we take as its beginning), the ghost of the representative firm is still haunting at least some economists' minds, partially confirming Robbins's disheartened prediction that, 'as is the way with ghosts [the representative firm] bids fair to outlast many more virile creations' (Robbins 1928:287).

Yet, soon charged with 'vagueness' and 'inconsistency', the representative firm appeared to have been replaced satisfactorily by Pigou's invulnerable 'equilibrium firm'. In the long run, however, this proved to be a regressive shift in the Cambridge research programme because the new notion hid many more problems than it helped to solve. First, it assumed 'perfect competition' which,

as Marshall once wrote to the young Keynes, 'belongs to the mathematical world on the other side of the looking glass' (Raffaelli 1996:141). Second, it made internal economies disappear, except for those induced by increase in the size of the industry. Sraffa's articles had not directly attacked the representative firm, possibly also because of its anti-marginalist implications, but had already turned its meaning into a definite concept, close to Pigou's 'equilibrium firm'. Thus, Sraffa's view of the static equilibrium of perfect competition pursued Marshall into a Pigouvian corner, whence the only exit was through the external (to the firm)/internal (to the industry) economies that Marshall would probably have considered little more than scientific toys.

After Robertson's passionate but unfortunate defence, the 'representative firm' was cast by Shove into his melting pot, and its material used to construct a theory of the distribution of firms by size. In recent rehabilitations of the idea (Newman 1960; Newman and Wolfe 1961), in order to make it more precise, this 'stochastic' meaning of the term has come to the forefront again as a proxy for a theory of the structure of industry.

On the other hand, evolutionary economists have taken the representative firm as the clearest example of Marshall's 'shyness' in his attempt at biologising economics (Hodgson 1993b; Limoges and Ménard 1994). They charge the notion with being a retreat from the biological atmosphere of the fourth book towards the mechanical equilibrium of supply and demand that characterises the fifth.

Though from a different perspective, reminiscent of Young's 1928 article, Marchionatti also points out the tension in Marshall's writings between the meaning of 'representative' from a biological (growth path of firms) and a mechanical (fixed amount of production) point of view. The fifth book is seen as an 'unsuccessful attempt to maintain at least in part the dynamic character of industrial competition, as described in book IV, in a stationary context'. The 'representative firm' is held to be precisely the tool responsible for this failure and the Marshallian school (Robertson) affords 'a perfect example of the contradiction between the will of examining a dynamic question and the actual use of static tools to deal with it'.

Marchionatti clearly states that Marshall, unlike Schumpeter, Viner and Pigou, did not accept the complete separation between value theory–abstract and static–and the dynamics of the real world. He seems to regret that neither Marshall nor the Marshallians took the next step: to abandon static value theory and directly follow the lines laid down in book IV.

Nobody was more conscious than Marshall of the limitations of his (or indeed of anyone else's) theory of value. He hoped the representative firm could make it less abstract and unrealistic by introducing those movements that are compatible with the stationary state (trees of the forest metaphor). Reading the relevant passages, there is reason to believe that he also hoped that the representative firm could be susceptible of further developments and provide a link between statics and dynamics, mechanical and biological analogies. After

calling the representative firm for help in the effort to understand what 'normal supply' means (Marshall 1961:342), he takes the reader into a meandering metaphorical world were the simple alternative mechanical-biological looses much of its sharpness. At first he introduces the usual metaphor of the pendulum (Marshall 1961:345), but soon adds new levels of complexity: as a second approximation, the string hangs 'in the troubled waters of a mill-race', sometimes flowing freely and sometimes partially cut off. Then the reader meets a new disquieting entry: 'the person holding the string swings his hands with movements partly rhythmical and partly arbitrary' (Marshall 1961:346). These metaphors, that 'illustrate all the disturbances with which the economist and the merchant alike are forced to concern themselves', descend from a theory of the levels of complexity that cannot be fairly represented by any double-decker static-dynamic or mechanical-biological apparatus.

I agree with Marchionatti that Marshall's main problem was dynamic but I would add that this is the very reason why he refused a self-contained value theory (mechanical equilibrium) that would close any access to the main problem, dooming itself to hopeless irrelevance. In Marshall's eyes, the 'representative firm' was the device by which 'increasing returns'–due to both external and internal economies and typical of the real world–were not cut off from the first analytical level ('Distribution and exchange', and the preface to the fifth edition of the *Principles* state that 'dynamic' considerations can never be absent from economic analysis, which is a study of economic *forces,* even when it concentrates on 'stationary' conditions). This relatively simple device could be the first step towards further levels of analysis (this partly explains Marshall's ambiguities about 'biological' as well as 'mechanical' representativeness). However unsatisfactory the solution, its objectives and guidelines are worth consideration.

Let me support this claim with a philological-philosophical note on the term 'representative firm'. It is common to consider it synonymous with 'normal', 'average' or 'typical' firm, a practice induced by Marshall's own interlacement of the terms (Marshall 1961:317–18, 459). 'Typical' in particular looks quite appropriate, calling to mind Weber's epistemology, and the need to capture some 'historical' element in the concepts we employ (Marchionatti has recourse to it). Groenewegen implicitly leads to similar conclusions, when he perceptively suggests the influence of Le Play's 'unusual method of gathering facts' (Groenewegen 1995:168), thus making the relevance of the representative firm dependent on its aggregation of historical phenomena with no subordination to their statistical 'tractability'.

However, any reduction of the term 'representative' makes it lose some of its resonance. Marshall was always very conscious and attentive, sometimes even meticulous, in his linguistic usages, which reflect subtle semantic differences. The first point to be kept in mind is that the 'representative firm' was one of the terms–like supplementary cost, trade capital, internal and external economies–which Marshall introduced to capture and clarify some

mental processes which take place in the mind of both the economist and the businessman.[1]

The businessman, who has to decide whether to invest in a new factory or machinery, has to bridge the logico-categorial gap between firm and industry. In order to make head or tail he has to build up in his mind some image of the place he is going to fill in the economic world. This task is performed by something close to the idea of the representative firm.

On the other hand, the economist engaged in devising a theory of value, even in stationary conditions, must perform a similar mental operation (because the concept of 'marginal cost' has no meaning where increasing productivity prevails as it usually does). If these are the loose coordinates of the representative firm, the next question is: how can we reach the target? According to Marshall, by 'selecting' a firm with certain well-known properties (and selection is a mental process usually associated with 'typical' ideas). But he also writes by 'imagining' the representative firm (Marshall 1961:459), and this verb is not used by chance. Imagination, and its derivatives, are sparingly present in the *Principles,* though the methodological chapter of book I exhalts the role of scientific imagination:

> The economist needs the three great intellectual faculties, perception, imagination and reason: and most of all he needs imagination, to put him on the track of those causes of visible events which are remote or lie below the surface, and of those effects of visible causes which are remote or lie below the surface.
>
> (Marshall 1961:43)

In the philosophy he had carefully studied in the 1870s, Kant and Hamilton in particular, imagination was an important faculty, somehow intermediate between sensation and perception on the one side and intellect, conception, and reason on the other. For Kant, '*imagination (Einbildungskraft)* is the faculty of representing in intuition an object that is *not itself present*' (Kant 1929:165), also when the object is not present to the senses. And Hamilton, whose *Lectures on Logic and Metaphysic* Marshall annotated and often quoted in his early notebooks, devoted many pages to the issue stating that 'the Representative [Faculty] or Imagination proper…consists in the greater or less power of holding up an ideal object in the light of consciousness' (Hamilton 1870–4, vol. IV:130) and that 'a vigorous power of representation is as indispensable a condition of success in the abstract sciences, as in the poetical or plastic arts' (ibid. vol. II:265). (This quote reminds us almost literally of Robertson's 'violent effort of the imagination'; Robertson 1930:87.)

Of course, imagination had also its own risks, and Marshall was prudently sensible to the political side of a faculty by which 'we may construct an edifice of pure crystal' (Marshall 1961:782). In a simpler and more general way, Hamilton had asserted that imagination could be 'a source of error' (Hamilton 1870–4 vol. IV:131).

In the light of this philosophical background, the choice of the term 'representative' was no product of chance. Discussing the relationship between 'normal' and 'average', Marshall distinguishes the first, expressing a 'law of tendency', from the second, which is only the product of statistical calculations. While the first term is connected to 'law' and is the product of reasoning about empirical data, 'average' calls to mind the availability of statistical methods of fact gathering. Of course, average is a plurivocal term, as each phenomenon can be part of different series and therefore of different 'averages'. Thus, normal price is defined as a kind of average price, 'the average value which economic forces would bring about if the general conditions of life were stationary for a run of time long enough to enable them all to work out their full effect' (Marshall 1961:347). 'In a stationary state alone…, Marshall repeats, "average price" and "normal price" are convertible terms' (ibid.: 372). If the 'firm' he was looking for was a device that should help to grasp the concept of normal supply price in a stationary state, where average and normal are one and the same, what was the need of introducing a new term like 'representative'? Esoteric reasons for this linguistic choice are implicitly reaffirmed in his letter of 14 May 1920 to the Indian economist Sinha: 'I prefer my own definition of "Representative firm": but "average firm" may serve for rough uses' (Whitaker 1996, vol. III:377).

I think that precisely its openness, vagueness and irreducibility to lawful 'normality' or statistical 'average' that were later to disturb the economic profession were seen by Marshall as features apt to prevent any tight closure of the theory of value, even in a volume on economic foundations: 'A man is likely to be a better economist if he trusts to his common sense, and practical instincts, than if he professes to study the theory of value and is resolved to find it easy' (Marshall 1961:368).

In due time, the linguistic, epistemological and ontological simplification that emerged from the Marshallian debates of the 1920s provoked more casualties than the lonely sentinel of the representative firm.

Note

1 See Marshall (1961:377 and 460) for the businessman's recourse to the 'representative firm'. See also Becattini (1962) for a penetrating analysis of the subjective side of the issue.

7 Why did Sraffa lose interest in imperfect competition?

A comment on Marcuzzo

Marco Dardi[1]

In a much-quoted letter to Keynes of 1926 (partially reproduced in Roncaglia 1975), Sraffa contemplated three possible lines of development for his inquiry into the theory of value: an elaboration of the hypothesis of constant costs; a turning to general economic equilibrium; or an extension of the theory of monopoly to markets with many competing agents. None of these projects, as we know now, was to be actually carried out in the years that followed. Only the third one left a mark on Sraffa's published works (Sraffa 1926a): a few pages containing an outline of a theory of imperfect markets that greatly impressed Keynes and the younger Cambridge economists with whom Sraffa was in closest contact–Gerald Shove, Richard Kahn, and Joan Robinson. In the late 1920s, an intense exchange took place between the four of them on various aspects of imperfect competition. But no sooner had the subject gained momentum as a most promising area of research than Sraffa lost all interest in it, and detached himself from the discussions going on in Cambridge and elsewhere from 1930 on. In fact, looking back from the standpoint of Sraffa's later works, few subjects appear more remote than imperfect competition.

Was this, then, simply an occasional detour from Sraffa's deep-seated interests, perhaps a mere concession to the tastes of an English audience (Harcourt 1972:15), and all in all an instance of misdirection of intellectual resources? It would seem that the episode is not to be set aside so lightly. As has been argued recently (Mongiovi 1996:215–16), when Sraffa undertook his exploration into the field of imperfect competition he seriously believed that a generalised theory of monopoly might constitute a sound foundation for a theory of value in which increasing returns co-existed with a supply-and-demand framework. He set about a project which he deemed valid at that particular time, and it is still not clear what it was exactly that deflected him from pursuing it. Unpublished evidence unearthed by M.C.Marcuzzo in the personal archives of Sraffa, Kahn and J.Robinson throws light on two moments in the exchange between Sraffa and his Cambridge interlocutors which seem to provide at least a part of the answer. The first moment is when Sraffa, faced with the revelation that

an important claim in his 1926 article did not hang together, had to abandon a substantial part of his theoretical proposal. The second, when he raised a difficulty connected with the problem of marketing expenses which had apparently slipped past him at the time of the article, and which then, although leaving his interlocutors singularly unimpressed, seemed to him to constitute an unsolvable puzzle. Careful examination of these two episodes directs us towards a plausible explanation of Sraffa's disillusionment with his original idea of generalized monopoly as the link between supply-and-demand and increasing returns.

Marcuzzo interprets the discussion in the 1920s as a sign, among others, of the incomprehension which surrounded Sraffa in his early years in Cambridge, and finds support for a widely-held view according to which Cambridge was a sort of academic island resisting all attempts at penetration. Our reconstruction leads us to see the whole matter the other way round. Far from being frustrating, the discussion helped Sraffa to obtain a sense of the distance between his true interests and what most of the economists of the period were doing. The young economists in the group around him were much closer than Sraffa to mainstream research. They were right, therefore, in declining to follow him along a path that would, with time, turn out to be a very personal one. If the metaphor of an island is appropriate, it should actually be applied to Sraffa himself, totally absorbed in the task of clarifying the features of a project that was so singular that it would differentiate him not only from the nearest circle of Cambridge economists, but also from the whole theoretical mainstream of the mid-century.

As already mentioned, we are basing our interpretation on two salient points: the 'serious error' (Kahn 1989:xv) that Sraffa made in his 1926 article, and on which Kahn remarked critically in his Fellowship Dissertation of 1928–9; and the question of 'marketing costs', which was the object of an informal argument between Sraffa, Kahn and Shove around 1929–30.

As to the first point, it is natural for present-day readers–familiar with the findings of years of game-theoretic analysis of oligopolistic markets–to side with Kahn against Sraffa, although it would be fairer to talk of a non sequitur, rather than an error, on Sraffa's part. The latter's claim was to the effect that, whatever the degree of imperfection prevailing in a market, the equilibrium that will be established in the end will be the same as would be established if all the sources of supply (firms) were under the control of a single monopolist trying to maximise joint profits (Sraffa 1926a:549, and lecture notes quoted in Marcuzzo). It was easy play for Kahn to demonstrate that, on the contrary, if the sources of supply make their decisions independently of each other, equilibrium will be a function of the assumptions that each firm makes about its rivals' conduct. For a whole set of assumptions, situated between the two extremes of Cournot-type and Bertrand-type assumptions,[2] equilibrium prices will certainly be less than monopoly prices (Kahn 1989: ch. 7).

These propositions are familiar enough nowadays as to not require going through the demonstrations once more. Besides, Sraffa himself (as we are told by Kahn 1989:xv, 95) acknowledged the mistake, although with 'a possible reservation' (see below, endnote 5). The matter can, therefore, be considered definitely settled. It is more interesting to try to understand how it was that Sraffa, usually a very sound reasoner, was misled to such an extent. The outline of his reasoning in the article (Sraffa 1926a:547–9) is remarkable for the effort to characterise a state of equilibrium without sparing a word for the motivations of the agents on whose decisions depends whether a state of things is or is not an equilibrium. The weight of the whole demonstration is borne by the distinction between two types of 'margin'–the buyer at the margin of exit from the particular market of a single firm, and the buyer at the margin of exit from the general market for the product. Sraffa's reasoning proceeds informally, but rigorously, up to the point of demonstrating that a generalised process of revision of prices upwards in a market characterised by imperfect competition must by necessity be brought to an end. Every time that, after a firm has taken the initiative of raising its price, the price increase generalises to all its competitors, the firm recovers part of its own clientele, with the exclusion of those customers who have left the general market. As the buyers who remain in the market become fewer and fewer as prices go up, the recovered clientele shrinks at every round of price increases. Thus, sooner or later, the incentive to raise one's own price disappears, and the process comes to a halt. The non sequitur arises when Sraffa affirms that this stopping point is the same one at which a monopolist having total control of the offer would halt.[3] This is not in itself an error, as Kahn seemed to believe: nowadays, we know of plenty of ways to sustain a monopolistic outcome as a non-co-operative equilibrium in models of imperfectly competitive markets.[4] The point is that Sraffa's conclusion needs precise assumptions concerning the strategies that each entrepreneur expects from his rivals, and the latter from him. From what can be understood from a manuscript note quoted by Marcuzzo in her paper, as a result of Kahn's objections Sraffa realised that the problem lay in the relationship between the elasticity of the market demand and that of the particular demand of each firm–a relationship that depends in fact on the conjectures of each agent regarding the correlation between his own price and those of the others.[5]

We can better understand the sense of Sraffa's argument and the weight of the impact of Kahn's criticism if we consider that Sraffa's expressly declared objective (Sraffa 1926a:548) was to oppose the thesis of the indeterminacy of the equilibrium price in the case of monopolists producing rival commodities–a thesis supported by Edgeworth in a fundamental article first published in Italian in 1897 (Edgeworth 1897: an English translation had just appeared in *Papers Relating to Political Economy* in 1925). In this article, two sources of indeterminacy were considered: first, the non-existence of an equilibrium, as in the well-known case of duopoly with limited productive

capacity of the competing firms; second, and more importantly, the intertwining of conjectures in a case in which an equilibrium (which, nowadays, we could identify as a Nash equilibrium) is perfectly defined, but more or less sophisticated strategic reasoning leads the competitors away from it (Edgeworth 1897:27–31). Sraffa's attempt was to exclude in toto any form whatsoever of indeterminacy, demonstrating that, whatever the case, 'the equilibrium is in general determinate' (Sraffa 1926a:549), and that it is independent of the degree of imperfection and of the type of conjectures of the agents. In other words, Sraffa's profound objective was to rid the analysis of imperfect markets of all kinds of 'mental' determinants by trying to detach equilibrium from the way in which entrepreneurs see their mutual interaction, and from those aspects of the buyers' preferences that determine the degree of imperfection.

Had it worked, his demonstration would have produced a clear-cut result which we could term typically "Sraffian", in the light of the mature Sraffa of *Production of Commodities*. It does not matter how much imperfection there is, nor what market agents believe: equilibrium prices are either perfectly competitive or monopolistic. The former is true, if there is absolute indifference on the part of buyers; otherwise, the latter is true. This result would do away with all the complicated subjective processes which seem to get in the way of any attempt at explaining prices by taking market phenomena as the starting point. Our hypothesis, by way of conclusion, is the following: it is possible that the proof–clearly given by Kahn–that Edgeworth's argument is actually irrefutable, and that dealing with imperfect markets renders the mental determinants of equilibrium unavoidable, was one of the reasons for Sraffa's estrangement from the entire problem.

The second theme in the discussion, marketing costs, seems to resume in a different form one of the fundamental points of Sraffa's criticism of the Marshallian theory of value, the independence of supply and demand as agencies that determine equilibrium values (Sraffa 1926a:538–40). In the 1926 article there is a mention of the possibility–by adding marketing expenses to production costs–of dealing with a situation of imperfect competition with decreasing production costs as if it were a situation of perfect (price-taking) competition with increasing *total* costs (production plus marketing). While Sraffa considered the two representations 'from the point of view of formal correctness[...]equivalent' (ibid.: 544), he concluded however that the sole adequate with respect to the nature of the problem was the one that did not conceal the imperfection of the market from which the marketing expenses originate. The manuscript notes reported by Marcuzzo relative, first, to Kahn's Fellowship Dissertation, and then to the discussion with Shove at the time of the 1930 symposium on increasing returns, introduce a new consideration which had no place in the 1926 article, on which Sraffa then put great emphasis: the dependency of marketing costs on both the quantity that a firm plans to sell and also the price. The main

interest of these notes lies in the fact that, in raising the problem, Sraffa seems to have been disconcerted by it but, at the same time, to have had a perfectly clear notion of how to work it out. In fact, by following his suggestions, we can see at once that, by introducing price as a new argument into the cost function–so that costs now depend on the quantity which is produced and the price at which the producer intends to sell it–the problem of maximising the producer's profit is easily solved. Although we do not find any formal development of the solution in Sraffa's notes, the conclusions to be drawn are so obvious that they could not possibly have escaped him. Let us see briefly what they amount to.

Let $c(y)$ represent the cost of producing y, and $s(y, p)$ the cost of selling y at price p, the first derivatives c_y, s_y, s_p being all positive. For any given level s° of the selling costs the set of solutions to the equation $s(y,p) - s^\circ = 0$ defines a demand function in implicit form, with elasticity at any point given by

$$e\left(y, p, s^{\circ}\right) = \frac{ps_p}{ys_y}. \tag{1}$$

The monopolist's–or imperfect competitor's–optimal choice is given by the solution of the problem of maximising the profit function, $py-c(y)- s(y, p)$, with respect to the variables y and p. Under the (sufficient only) conditions that sp be non-decreasing with respect to p, and the sum $c_y + s_y$ non-decreasing with respect to y, we get the first order maximum conditions

$$y - s_p = 0 \tag{2}$$

$$p - c_y - s_y = 0. \tag{3}$$

The first condition requires that the quantity produced be equal to the cost of keeping one's clientele in the face of a marginal price increase; the second, that the price be equal to the marginal cost of producing and selling that quantity at that very price. By utilising [1] and [2], condition [3] can be transformed into the usual condition of maximum monopoly profit,

$$p - c_y = s_y = \frac{ps_p}{ye} = \frac{p}{e}, \tag{3'}$$

which now, of course, characterises the solution to the profit maximisation subproblem that corresponds to a given value of s.

The obviousness of this solution makes us understand why Sraffa's objection was not received as particularly disruptive. In fact, it did not find an echo in his interlocutors. Once he had taken note of Sraffa's point, Kahn got round it by dealing only with the subproblem of equilibrium obtained by

taking a given level of marketing costs as in [3′], i.e. without considering marketing costs as a strategic variable (Kahn 1989:89–90). In the text of the 1930 symposium published in the *Economic Journal* (Shove 1930), Shove did in fact treat them as a strategic variable, but without even mentioning the point raised by Sraffa. The question then is, what was so deeply disturbing for Sraffa in a problem which left the others unmoved? Why, while envisaging what the right solution should be, did he write in a manuscript note that '*any* definition that can be given seems unacceptable' (*RFK* 3/13/153, quoted in Marcuzzo's chapter, p. 91)?

Sraffa's rather cryptic remark on there being not a curve (of marketing costs?), but a point, as the solution to the problem (ibid.) does not seem to contribute to an explanation. As we have seen above, a function of marketing costs conforming to Sraffa's qualifications can be defined; and the fact that the solution boils down to a single point does not create any particular difficulty, as it is true in Sraffa's as well as in the traditional theory of monopoly. The only difference between the two theories is that, in the former, the solution is represented by a point (y, p, s) in a three-dimensional space, instead of by a point (y, p) in a bi-dimensional space. The revealing sentence seems to us to be a different one, taken from a manuscript note in which Sraffa points out that the aim of marketing expenses is that of making demand dependent on supply. 'Therefore there is no demand curve which can be used with a supply curve that includes [marketing expenses]–they are not independent' (*SP* D 3/7 8, quoted in Marcuzzo's chapter, p. 90).[6] Now, what can be wrong with supply being a determinant of demand? Trying to enter into Sraffa's frame of mind, a possible answer is the following: the fact that the cost of influencing demand cannot be obtained from the objective elements that constitute the technology of production, but from all those ephemeral, invisible, complex elements that determine consumer preferences among various sources of supply (Sraffa 1926a:544–5)–elements even more ephemeral than those that determine preferences among goods intended as homogeneous classes of products. From this comes the implication that, the moment we admit that it is supply that determines demand, we also admit that costs, and therefore prices, are determined by subjective elements of the sort most impervious to precise and rigorous analysis.

Our second hypothesis is that this aspect of imperfect competition made Sraffa understand that, by following the lead of marketing costs, it is not that the 'two blades of the scissors' that determine value give up their place to the sole technological and distributive blade represented by cost conditions. On the contrary, it is indeed the 'utilitarian' blade, in a version that is most difficult to deal with analytically, that wins space and importance compared to the other. By means of marketing expenses, utility enters into costs, so to speak. Sraffa, in his lectures on the theory of value of the late 1920s, had already dissociated himself from Marshall's attempts in the *Principles* to reduce costs to utility by means of the notion of 'real' costs as distinguished from

monetary costs of production (see Marcuzzo in this volume and also the passage from a letter of Sraffa to A.Asimakopulos of 1971, quoted by Salvadori 1998:257). He then came to realise that imperfect competition may bring about a similar outcome. Seen in this light, it is not surprising that a non-insurmountable difficulty had such an arresting effect on him.[7]

The two hypotheses that we have proposed concur in showing us Sraffa blocked by the discovery of the radically subjectivist outcome of his attempt at reconciling increasing returns with the theory of value by means of the generalisation of monopoly. In reality, the latter is a theoretical innovation which is so powerful that it can make increasing returns, submerged by the variety of subjective and strategic considerations that interfere with the relationship between technology and prices, move into second place. The recent historiography on Sraffa has brought into focus the reasons why he abstained as much as possible from taking into account in his theoretical work those forms of social interaction in which subjectivity is expressed most directly. It is not that he considered them irrelevant; on the contrary. According to all evidence, his idea was that they were too rich in complexity and had too deep roots in history for them to be reduced to simplistic and 'evanescent' abstractions, such as the marginalist economists' concept of utility.[8] From this comes the contrast noted by A.Sen among others (Klamer 1989:138) between the wealth of Sraffa's humanistic interests and the narrowness and mechanical appearance of his theory. Against the prevailing trend in economics which, from Marshall's famous manifesto *The Present Position, of Economics* (Marshall 1885), has always attempted to combine subjectivity and objectivity, internal motivations and what is externally visible and measurable, Sraffa posed as a separator: although he never put down in writing the reasons for this conviction of his, it is clear that for him theory ideally embraces what is visible and measurable, while the underlying human motivations belong to other domains–history, philosophy, any sort of knowledge that does not require the precision and exactitude of theoretical analysis.

This reconstruction of his passing interest in imperfect markets will hopefully add to the perspective of the experiences through which Sraffa arrived at bringing into focus this peculiarity of his. For some time, it seemed possible to him to deal with market interaction as something that produces results that are subjected to a logical necessity that derives essentially from the structure of technology, and only in very small part from subjective, utilitarian or other sorts of motivations. Through discussion with his fellow economists in Cambridge, he realised that this was not so, that in the field of market phenomena the relative weight of the subjective element is overwhelming. We can thus conclude that, if his interest in imperfect competition vanished, it was not because he discovered that the subject was uninteresting *per se,* but because he became convinced of its not being

amenable to that visible and measurable order which is the prerequisite of economic theory according to the only meaning that he believed in.

Notes

1 This is an extended version of a contribution originally presented in the form of comments on the paper by M.C.Marcuzzo included in this volume. All my references to unpublished materials are based on research conducted by Marcuzzo in various Cambridge archives. While thanking her for generously sharing with me part of her findings, I naturally exempt her from any responsibility for the usage that I have made of them.

2 Incidentally, Sraffa's reasoning seems to be implicitly based on the latter.

3 In the text published in 1926 the connection between the previous reasoning and the conclusion is left unexplained. In the 1929 lecture notes we find an attempt at explanation in the sentence 'No more [customers] are lost [by an individual firm to the market] than if it were a single monopolist' (*SP* D 2/4 13(4), quoted in Marcuzzo's chapter, p. 86). Did Sraffa perhaps believe that this was a way to demonstrate that an individual firm would not stop increasing its price any sooner than a monopolist would? The argument, however, is in any case insufficient.

4 As is well known, it is enough to treat interaction within an imperfect market as an infinitely repeated game, with strategies made conditional on information acquired in the course of the play. Also Chamberlin (1929; 1933, chapters III and V) was to support a theory similar to Sraffa's, with reference to small groups of competitors, with or without product differentiation. However, he based his theory on an entirely different reasoning, one which implied long-term rationality of the strategic behaviour of firms, quite in line with the spirit of the modern developments of industrial organisation theory.

5 Although it is not very clear, the passage seems to suggest that even if Sraffa admitted his own error, he did not consider Kahn's thesis to be irrefutable either. This might be the 'possible reservation' mentioned by Kahn (Kahn 1989:95). In fact, as mentioned above, Sraffa's conclusion is not wrong in itself, but different arguments from his would have to be offered in order to sustain it (see preceding note).

6 To avoid possible misunderstandings, let us recall that in the terminology of Marshall's *Principles,* to which Sraffa still seemed to cling in 1926, the term 'supply curve' is used, with reference indifferently to competition and monopoly, as a synonym for cost curve in Marshall's sense (the curve obtained by setting against each amount of supply the minimum price necessary for maintaining that amount on the market), and not–as in the modern sense–the function that associates the chosen quantity with each given price. It is for this reason that Marshall continues to speak of the 'supply curve' of the monopolist, while being perfectly clear that, given the demand function, the monopolist's choice is a point on the latter.

7 At this point, we should also consider that marketing costs propose once again all the difficulties connected with strategic interdependence which, as we saw in section 2, may have prevented Sraffa from pursuing his analysis of the equilibrium of imperfectly competitive markets. It is difficult to believe, in fact, that the function $s(y, p)$ of a firm is not influenced by the level of the marketing expenses of the others. But there is no evidence that Sraffa noted the problem in this connection.

8 See the remarkable passage from a 1927 manuscript quoted in Kurz (1998:35). See also Salvadori (1998, especially pp. 244–6, 257–8).

Part III

Continuity and change in Sraffa's thought

From the 1920s to
Production of Commodities by Means of Commodities

8 Continuity and change in Sraffa's thought

An archival excursus

Luigi L.Pasinetti[1]

Premise

The present chapter originates from curiosity stimulated by a letter that Piero Sraffa wrote to John Eatwell and Alessandro Roncaglia in 1974. Eatwell and Roncaglia had been working on an English translation of Piero Sraffa's 1925 *Annali* article (Sraffa 1925a). They had had endless sessions with him, discussing the details of the translation. When everything was finished and the article was ready for publication, Sraffa had second thoughts and wrote them a letter, in which he withdrew, at least temporarily, his permission to publish.

The letter (here reproduced as document 1 in the Appendix) implies that Sraffa's opinion had undergone some changes from the time he had written the *Annali* article. Presumably, he did not want to bring this issue into the open through any discussion that might arise from publication of his article in English. He thus preferred–as he wrote–to postpone publication until after his death.

But what kind of changes of opinion may have taken place? This is what triggered off my curiosity.

Evolution in Piero Sraffa's thought

The *Annali* article was published, in Italian, in 1925, as is well known. It contains the background analytical scheme which is behind Sraffa's more famous 1926 *Economic Journal* article (as he explicitly says in his opening sentences). Sraffa had never refused permission to publish translations of his articles. The *Annali* article itself had already been translated from Italian into French, German, Spanish, Japanese, Polish and had also been reprinted in Italian. Was an English translation to be considered so special?[2] And, more importantly, what was it that made Sraffa so sensitive?

To begin with, it seems necessary to establish some sense of proportion. In the history of economic thought changes of opinion have not been

uncommon. There have been famous, and in fact radical and striking, changes of opinion. We may think of the case of Keynes, who changed his mind, in the early 1930s, by repudiating his *Treatise on Money* (1930) and moving towards his 'revolutionary' *General Theory* (1936). One may also think of Kaldor, who, around 1940, repudiated his marginal theory writings and went through a 'conversion' to Keynesian economics. But there cannot evidently have been anything of this sort in Sraffa's case. I think everybody would agree that it would be unthinkable to look for radical changes of mind of this type in Sraffa's thought.

Yet, something must have taken place. Some hints may be found in the Preface to Sraffa's *Production of Commodities by Means of Commodities* (1960a), where he refers to the question of returns to scale. As is well known, Sraffa had claimed in 1925 that the only logically consistent hypothesis to make, in a theory of production, is that of constant returns to scale. But in his 1960 book he claims that his analysis does not imply any assumption on returns to scale. It would be difficult to class this as a radical change, especially if we consider that Sraffa himself, for the benefit of the reader, suggests that:

> If such a supposition [i.e. that of constant returns] is found helpful, there is no harm in the reader's adopting it as a temporary working hypothesis. In fact, however, no such assumption is made.
>
> (Sraffa 1960a:v)

Was the change all here? Or was there something else? In any case, something had happened. This should not be surprising: from 1925 to 1960 there elapsed thirty-five years! It remains to be established how much or in what sense Sraffa's thought had changed.

Everybody would accept it as normal that the thought of any active intellectual always undergoes some change, or, as one might say, some evolution, as time goes on, owing to the cumulation of intervening discussions and reflections. This must certainly have happened in the case of such a scholar as Sraffa. Hence, to envisage a sort of evolution in his thought appears quite reasonable; an evolution that may have been more rapid in certain periods than in others; sometime so rapid as to suggest a sort of turning point. But nothing, one can imagine, could be like a break of the sort experienced by Keynes or by Kaldor–to take the cases just mentioned–or even, to recall yet another famous case, of the kind that characterised the change that intervened from the *Tractatus* to the *Investigations* of Ludwig Wittgenstein (1922, 1945); a change attributed, incidentally, to Sraffa himself.

Thus, if we accept that some sort of 'evolution' must have taken place in Sraffa's thought, what remains to be investigated is how far, or to what extent, it went. This is the intriguing question.

A personal immersion in the Sraffa's Papers

I have locked myself up in the Trinity College Wren Library at Cambridge for the past fifteen days,[3] and I have tried avidly to read notes and scripts, and files and files of papers, which seemed to be relevant to the question stated above. I found that consultation of Sraffa's papers and manuscripts, as at one time I found my conversations with him, instantly bring into relief a personality of immense and disconcerting complexity. I obviously began with the year 1925 and I tried to concentrate on anything that might have appeared relevant in order to detect Sraffa's long journey to his book (Sraffa 1960a). Of course time has been too short, even by taking advantage of my previous consultations of Sraffa's papers (all after 1994, when Sraffa's archives were opened). Therefore the impressions which I shall try to express in this chapter cannot be anything but provisional.

The catalogue of Sraffa's papers at the Trinity College Wren Library is not perfect, yet is clear enough to give a helpful guide. Leaving aside the 'Personal papers' (classed as section A), those concerning his 'academic career' (section B), his 'Diaries' (section C), the 'Memoirs of colleagues' (section F), the 'Bibliographical material' (sections H and I), and finally the 'Miscellaneous material' (section J), it was natural for me to concentrate on the 'Correspondence' (section C) and on the 'Notes, lectures, publications' (section D). The correspondence is inevitably fragmentary and a bit disorderly, but is a mine of information, direct and indirect, and a potent stimulus for conjectures. The publications are well known. The unpublished lectures are many and varied, the most important of which being the sixteen *Lectures on the Advanced Theory of Value,* delivered for the first time in 1928 (Michaelmas Term), and then repeated, with amendments and additions, in the three subsequent years.

For the purpose that I had in mind it is, however, the *Notes* that revealed themselves to be relevant and interesting. I found them fascinating and disconcerting: an enormous number of various sheets, of any dimensions, backs of other documents, small books, block-notes, small and large, fragments of printed papers (newspapers or else), on which one finds notes, and notes, and corrections of notes, sometimes very brief, other times of the length of proper articles, on the most disparate and unexpected subjects. The language used is Italian, at the beginning, slipping then gradually into English in time; and in fact always being a mixture of the two, in different (and changing) proportions. There are quotations and here also French and German appear (copied in his clear hand-writing). There are comments, and there are a number, which seems never to end, of criticisms, counter-criticisms, reflections and second thoughts. Not all, but most of, these notes are *dated* by Piero Sraffa himself, with indication of day, month and year. (Many of those that are not dated are datable from the context.) A query immediately arises: for whom are these dates? Most probably for himself: in

order to remember the circumstances behind the notes when coming back to the problems concerned, especially after long interruptions. But really only for himself? The conjecture is difficult to repress–thinking of Sraffa as a historian, a careful philologist, a powerful and highly critical intellectual–that they could also have been put there for the benefit of those who might be interested in reading them in the future. If this were so, his purposes would appear to be, or to have become, really far-reaching.

Some remarks on the Sraffa archives subdivisions

It is important to recall that the classification of Sraffa's notes was made, by a professional catalogue expert, *after* Sraffa's death. It is by all means natural to take Sraffa's publications as the points of reference and of attraction of his notes. This has full justification when the notes are near, in time, to the corresponding actual publications. But when, between the notes and publication, there elapses a long period of time, such justification becomes weaker. In this perspective (if we exclude the early publications on monetary subjects), the notes in preparation of the articles of 1925 and 1926 and the notes in preparation of the (unpublished) 1928 Lectures can be singled out with sufficient clarity. Then, from 1928 onwards, all theory notes that do not refer to Ricardo's *Works and Correspondence* are classified as being 'in preparation of *Production of Commodities*'. This may not be entirely justified.

The period from 1928 to 1960 is a very long period indeed in any scholar's life–more than thirty years! A distinction of these notes from the previous ones is however clear enough. On the cover of more than one file Sraffa himself writes: 'notes after 1927'. And the catalogue makes a distinction between pre- and post-1928 notes. It seems clear that a distinction is drawn by Sraffa himself between the earlier notes, that were specifically aimed at imminent publications, and a more substantial, far-reaching, set of notes, aimed at a more considerable kind of work. Sraffa seems to have something definite, perhaps great, in mind. In normal circumstances, one might have expected from him the writing of a book. And in fact there is a note in his files that is headed 'Impostazione del libro'–an explicit statement of his intentions on how to write 'the book' (see document 2 in the Appendix).[4] But if this was so, the period of preparation of such a book kept on becoming longer and longer, while being characterised by various events, abrupt halts, new engagements, with long interruptions. It is reasonable to expect that, in this tortuous way, his original intentions may have been affected, and may have changed, to a certain extent.

Let me review, schematically, what is revealed by the grouping of the theory notes (i.e. those that do *not* refer to the edition of Ricardo's *Works*):

- there is first of all the period 1928–31, which obviously must have been a crucial period in framing Sraffa's aims and intentions;

- then there is a gap, that extends up to the beginning of the 1940s. This is the period in which Sraffa devotes himself, fully as it appears, to the edition of Ricardo's *Works;*
- the notes are resumed in 1941, all of a sudden and very intensely, then at a slower pace as the years proceed, up to 1945;
- there comes another interruption from 1946 to 1955. This is the period in which Sraffa is engaged with taking Ricardo's *Works* to actual publication. He is also victim of an unfortunate accident in a sadly famous holiday in Norway;
- finally, there is the period from 1955 to 1960, where one finds Sraffa's final efforts to gather at least part of his notes into a book, finished to all purposes in 1958, but published, amongst endless hesitations, at the end of May (Italian version, a week later, early June) 1960.

Overall, I found therefore three relevant, but separate, periods for my purposes, with three corresponding groups of notes: 1928–31, 1941–5, 1955–9. These three groups of notes are quite distinct, in terms of the subjects investigated. In the archives, they are *all* classified as 'notes in preparation of *Production of Commodities*'; simply because no publication took place, except at the end, in 1960. However, this way of considering Sraffa's notes, reflections and self-criticisms risks being misleading in many respects. Sraffa *did not know* in 1928 that, in 1960, he was going to publish a small book called *Production of Commodities by Means of Commodities*. He intended indeed to write a book, as pointed out above, but his intentions about the kind of publication(s) that would come may have been quite different, at the beginning, and they may have changed, or 'evolved', quite a lot from the early 1930s to the final year (1960).

Three streams of thought

On reading Sraffa's notes, one remains disconcerted and bewildered: I was for days and days. But when I went back and reflected, and looked over my notes, and tried to synthesise in my mind the hundreds of fragments of thoughts, criticisms, re-formulations, counter-thoughts, etc., forcing myself to take a detached overview, as from a bird's eye view on a high flight, I got the impression of at least three clearly distinguished, though intermingling, strands, from beginning to end, in Sraffa's remarkable set of notes. These three strands concern the development of three corresponding streams of thought.

First stream of thought. One thing that appears quite clearly from the notes since 1928, starting immediately after the publication of the 1925 and 1926 articles, and parallel to the revision of the 1928–31 lecture notes, is that Sraffa is convinced, since the beginning, that an aberrant distortion had taken place in economic theory in the second part of the nineteenth century. From 1870 onwards, dominant (marginalist) economics had caused a change in the

content of the whole subject, with respect to what it was previously. More precisely, Sraffa finds that, since 1870, economic theorists uses indeed the same vocabulary, the same language and terms of reference as before, but the underlying concepts have undergone a 'terrific' change. Sraffa shows astonishment: did not Smith and Ricardo on the one side and the marginalists and Marshall on the other speak the same English language? Why does no one realise that the actual content, the concepts behind the same words, have become entirely different and concern entirely different things? There is an 'abysmal gulf' (*SP* D 3/12/4, f. 14)[5] between the marginal economists' writings since 1870 and the economists of the beginning of the nineteenth century (see Appendix, document 3). The basic problem is not, or not only, a question of a different theory. We are not simply facing a question of 'marginal theory' versus 'classical theory', as one may be inclined to think. For Sraffa, marginal theory is an aberration. There exists, for him, a sensible economic theory and an aberrant economic theory. The change of name itself which took place, from Classical 'Political Economy' to Marshall's 'Economics', is there to 'mark the cleavage' and 'Marshall's attempt to bridge over the cleavage and establish a continuity in the tradition is futile and misguided' (*SP* D/12/4). In Sraffa's opinion, one must discard the aberrations and go back to an economic theory that is sensible, true and reasonable: the economic theory that existed before the 1870s. This first stream of thought in Sraffa's notes appears therefore as belonging to the history of economic thought.

Second stream of thought. From what has been said above, Sraffa appears to be convinced that it is a question of absolute priority and necessity to develop a ruthless critique of the aberrations brought into existence by marginal economic theory. The great majority of his notes and reflections and comments are in this direction. They form an impressive set of critical arguments; and, in this, Sraffa really reveals himself as an exceptional critical mind. The notes in the Archives provide a determined, reiterated, punctilious set of criticisms of the economic theory that has come into being since 1870. Within this critical stream of thought, one can find many substreams. Since the field is immense and the notes are numerous, I may mention at least four themes that frequently occur as specific targets of his poisonous arrows: (i) the marginal theory of production and distribution; (ii) the theory of value (which the marginalists call price theory); (iii) the theory of marginal utility; (iv) the theory of interest, when interest is presented as a reward for abstinence (his remarks on this subject are particularly sarcastic). This second stream of thought in Sraffa's notes is therefore aimed at a critique of dominant economic theory. It is by far the most extensive and prevailing stream of thought in Sraffa's notes, especially in the early periods.

Third stream of thought. A third strand of arguments unfolds as a logical consequence of the previous two. For Sraffa, it is absolutely necessary to return to the point where sensible economic theory stood, i.e. to the point

where its development was interrupted and distorted. It is necessary to return to the Political Economy of the Physiocrats, Smith, Ricardo and Marx. One must resume economic theory at the point where it was left. And one must proceed in two directions: (i) to cleanse it of all difficulties and incongruities that the Classical economists (and Marx) had not been able to overcome; and (ii) to go on and develop the relevant and true economic theory as this should have evolved, from 'Petty, Cantillon, the Physiocrats, Smith, Ricardo, Marx'. This natural and consistent flow of ideas had suddenly been interrupted and buried under the all-invading, submerging, overwhelming tidal wave of marginal economics. It should be rescued. This third stream of thought appears therefore, at last, as a constructive stream of thought.

An impossibly grand research programme

The three streams of thought sketched out above make up such a huge research programme as to frighten anybody who might think of carrying it out in isolation. Yet Piero Sraffa, at the beginning, seems to have aimed at doing precisely that.

One can see such programme as showing up at the time of his coming to Cambridge, and more clearly at the stage of the revision of his (unpublished) *Lectures on Advanced Theory of Value,* i.e. in the years 1928–31. But it must not have taken long for him to realise the sheer impossibility of bringing such an atrociously grand research programme into actual shape. The contrast between aims and realistic possibilities begins to emerge strikingly from his notes, while he is preparing the amendments to his *Lectures on Advanced Theory of Value*. These *Lectures* had all been handwritten in 1927. They were delivered in the three subsequent years, with changes and amendments, which one can find added, in his clear writing, on the manuscript, with obviously increasing dissatisfaction.

The sheer fact of being compelled to lecture stimulates Sraffa's mind to the limit of endurance. One can see from his critical notes that he goes in depth, he goes into analysis, he goes in extension. Never does one find him going toward a synthesis. Thus he writes notes which are essentially critical and provisional. Apparently, these notes are for himself, but perhaps he may have begun quite early to look ahead and hope that someone in the future might pick them up. (One could understand in this way also his care in marking them with a date.) Criticisms add themselves to criticisms and to the critique of criticisms.

It is a fact that, at a certain point, even delivering his *already written-up* lectures becomes for him an excruciating experience. It must indeed have become a hard task for him to save himself from frustration.

We can infer that Keynes' intuition was sharp enough to realise that Sraffa was in serious predicament, without perhaps understanding clearly the basic source and wide extent of his drama. In any case, Keynes is sufficiently

impressed to become convinced that in some way somebody or something should come to the rescue. Thus Keynes manages to convince Professor T.Gregory of the London School of Economics to withdraw from his already signed-up agreement with the Royal Economic Society to collect and edit the works and correspondence of David Ricardo. The contract is transferred from T.Gregory to Piero Sraffa. A real blessing. God knows what Sraffa would have done otherwise.

At that point, Piero Sraffa is relieved. He resigns his Cambridge lectureship so as to stop the nightmare of delivering lectures and he immerses himself, for the following thirty years, into his newly acquired task–a task which to external observers appears, from that point on, as his major concern. Behind the scenes, his principal grandiose research programme is temporarily put aside. Not entirely, of course. If nothing else, he catches the opportunity to clarify to himself, and to clear up, the incongruities in Classical economic thought. This merges well, after all, with the first part of what I have called, above, his 'constructive' strand of thought.

Sraffa becomes so aware of the relevance of Ricardo's works to his research programme, that when, in 1941, the bulk of Ricardo's writings have gone to the printer (to remain there for years, owing to his difficulties in writing the introductions and then owing to the discovery of new documents, as will be explained below), he goes back to his programme and begins to shape up a new phase which, from the notes, now appears as leading him to concentrate on the correct formulation, in terms of equations, of at least some of his 'Classical' propositions. This is quite evident in his 1941 notes, where one can see his earlier thoughts being resumed at the point where they had been left. In fact he had already tried to formulate his theory in terms of 'equations' as early as in 1928. He had even showed such equations to Keynes. This event is mentioned at many points in the drafts, and then, though in a slightly more diluted form, in the published preface to his book. But in the late 1920s he had barely been able to satisfactorily go beyond the 'equations without a surplus'. In 1941–4 he really makes a breakthrough. With the advice, not always followed and actually sometimes contradicted, of Besicovitch, he succeeds in formulating correctly the equations with a surplus and with labour explicitly introduced,[6] while discovering the notions of a maximum rate of profit independent of prices, of basics and non-basics, of the 'Standard system'. These results really represent a remarkable achievement. Obtained in isolation and silence, they will be included in the first part of his book twenty years later. But at the time they absorb all his efforts. There is very little else he can do on the rest of his original research programme. He goes back, now and then, to his previous notes, and adds some comments and further reflections. Not much more than that. As a consequence, the horizon of his research programme is drastically restricted. As he proceeds, he is excited by the remarkable properties he is discovering in the mathematical formulation of his equations. But this absorbs his time. He is compelled to postpone, or cut down, the other aspects.

Precisely at this point, another interruption comes in his way. Unexpected events, during the war, lead Sraffa to take advantage of an exciting discovery of a different sort. In July 1943, by chance, a locked metal box containing a considerable number Ricardo's papers, actually the whole series of his letters to James Mill and other manuscripts, is unexpectedly found at Raheny, Co. Dublin. As soon as Sraffa is informed and becomes aware of the discovery, he has no hesitations in deciding that he must re-think the whole layout of the plan of publication of Ricardo's *Works,* even though the volumes are already in print! Increasingly, especially from 1944, his concern is shifted away from his theory notes. Very rapidly, his energies are fully diverted to the task (including the excruciating experience of writing the 'Introductions', with the help of Maurice Dobb) required to re-structure, and then to carry Ricardo's volumes I to X to actual publication (1953–7). He could hardly have done otherwise, under the mounting pressure of the Royal Economic Society for the long overdue publication of a work that had been 'in print' for more than ten years. To this purpose, his energies are absorbed almost fully from 1945 up to 1955 (with the added misfortune of the time forcedly lost as a consequence of the already mentioned mountaineering accident in Norway).

When, in the end, all Ricardo's works are published (with the exception of the Indexes, which were to remain in the pipeline of publication until 1973), Sraffa finally does go back and resumes his theoretical work, as it was left in the 1940s. From 1955 to 1960, when nobody would have expected it, he succeeds in setting together enough propositions to be able to complete and, at long last, publish a book. We all know it well: a ninety-nine-page book, amazingly dense in concepts, terse and essential, extraordinarily compact and disconcertingly cryptic–*Production of Commodities by Means of Commodities*. Sraffa abstains from making any claim. He presents it as no more than 'a prelude to a critique of economic theory'.

What fraction of the original programme?

What fraction of the original programme has eventually come to fruition? This becomes an irrepressible question at this point. The richness of the existing manuscripts can give us at least some idea of the wide gap that has grown in time between the original intentions and what Piero Sraffa finally becomes convinced to publish.

First of all, one must record with sadness that Sraffa abandons the aim of publishing anything on the history of economic thought. This is by itself an extraordinary decision if we consider his original intentions. An idea of the width of the original purpose may be seen from a very clear and telling scheme (see document 4 in the Appendix) of how he sees the development of economic thought from Petty to Marshall. In the same folder, one finds a page–headed 'Principio'–giving his intended plan of exposition (document 5 in the Appendix).

The ten-year interruption that follows, due to his ground work for the edition of Ricardo's *Works and Correspondence,* induces him–as one may clearly perceive from the post-war notes–to a severe re-assessment. His original grand programme–left aside for ten years–undergoes a radical, down-to-earth reconsideration, presumably in view of a more realistic awareness of what could be done, given the effort and time absorbed by the setting together of a satisfactory formulation of his equations. In a note, which in the *Sraffa Papers* is among the post-1945 notes, we find a scheme headed '? Preface', where Sraffa gives an explicit account of the cuts he has decided to make with respect to the originally intended scheme (see document 6 in the Appendix). But the restructuring does not stop at this stage and goes on and on, as one may realise by comparing what is said in document 6 itself with what one finds in the final publication.[7] Quite surprisingly, in the end, nothing explicit remains on the history of economic thought. Only indirectly do we find brief (yet remarkable) pieces, pertaining to the history of economic thought, in the 11-volume edition of Ricardo's works. In *Production of Commodities,* all that one can find is a two-and-a-half page appendix called 'Appendix D–References to the Literature'. And that is really all. It seems incredible, if we think that these two-and-a-half pages are what is actually published on the history of economic thought by a person who is considered as one of the greatest scholars in the field.

The same process of a progressive restriction of the horizon also comes to affect the major stream of Sraffa's work: the one referring to the critique of current economic theory. It is indeed astonishing to realise that, in the end, no explicit critique of marginal economic theory remains (with the exception perhaps of very short bits here and there, such as the one on the average period of production), though the concern with this critique is clearly the major objective that Sraffa had in mind since the beginning. A hint is given in the opening sentences of the Preface to his book. He states:

> It is…a peculiar feature of the set of propositions now published that, although they do not enter into any discussion of the marginal theory of value and distribution, they have nevertheless been designed to serve as the basis for a critique of that theory. If the foundation holds, the critique may be attempted later, either by the writer or by someone younger and better equipped for the task.
>
> (Sraffa 1960a:vi)

Consistently, he sub-titles the book: *A Prelude to a Critique of Economic Theory*– an implicit confession of his awareness of remaining very far away from what his manuscripts reveal to be his original targets. At the same time, his last sentence just cited reveals the beginning of his opening up to the hope that some people of the younger generation may follow his lead and carry on his (originally conceived) task.

One must conclude that, as far as actual publication is concerned, what have been called above the first and the second streams of thought in Sraffa's original programme–really two major strands of thought in his notes–have, in the end, been abandoned.

It sounds paradoxical–if one thinks of Sraffa's well-known powerful, critical mind–that he should decide in the end to leave critique aside altogether and go straight on–and in an amazingly concise way–to what has been singled out above as the third stream in his thought: the constructive stream of thought. And it sounds almost unbelievable that, after reproaching Marx, in his earlier notes (see Appendix, document 2) for not having presented, first, a historical explanation, thus being the cause of his not being understood, he should do exactly the same. However, much worse, he not only drops his historical conception of the evolution of economic thought, he also leaves any critique aside altogether; and on the top of that uses an extraordinarily compact method of exposition, compressing his arguments to the limit of incomprehension. No wonder the result has been found puzzling, cryptic and by some people even obscure.

The state of Sraffian understanding has somewhat improved. Many economists of the younger generation have not disappointed his hopes. His constructive contributions to the analysis of the relations between value and income distribution, in a most general production economic system, have by now been perceived. His analytical results concerning the Standard system and the relations between prices and income distribution have been widely illustrated. Many of the proofs concerning the remarkable properties of his system of equations (such as uniqueness, non-negativity of solutions, joint production with fixed capital and land as special cases, etc.) have been reformulated with the help of powerful mathematical tools (such as Perron-Frobenius theorems). Moreover, his analysis of the switching of technique has been at the centre of a vast debate in capital theory. And his Introductions to Ricardo's *Works* have opened up the way to a clearer and deeper understanding of Classical economic theory.

But precisely because his analysis was not preceded by an exposition of his conception of the historical evolution of economic thought and by his critique of marginal economic theory, his constructive efforts are still far from being fully understood. Many economists, even among those basically sympathetic to his approach, remain in a state of dissatisfaction.

Most of all, the part of Sraffa's analysis that remains in the shadow is the one concerning the role of physical quantities, and the economic movements through time. One can understand quite well how prices and quantities are separated in Classical economic theory and how, consistently, they are in Sraffa's theoretical scheme. But Sraffa withdraws one step back. In his published 'propositions', the physical quantities are taken as given, so much so that some critics have misinterpreted his system as being only a half-system, referring to the price side and not to the quantity side of the economy.

How did he conceive the economic movements of physical quantities, i.e. the dynamics of an economic system? It is tempting to look at von Neumann's scheme, or at Leontief's dynamic model, to try to gather some clues. But, in spite of the analytical similarities with Sraffa's 'Standard system', von Neumann's approach appears inappropriate; and so does Leontief's. Sraffa does not even mention von Neumann's model, nor does he mention Leontief. Those who had the opportunity of putting questions to him on these similarities know Sraffa's negative responses. Von Neumann's and even more Leontief's approaches are quite alien to his conception of the movements of an economic system through time.

What really is then Piero Sraffa's conception? It is not easy to give a satisfactory answer to this question. In Sraffa's early notes one finds some hints at the problem of 'closing' the system, in terms of what wages and profits could buy. But these are passing remarks (or so they appear to me). My impression, of course provisional, is that, on this aspect, the (in many respects) enormous mass of Sraffa's notes are not sufficient to reveal any clear direction. Maybe he simply did not have time to apply his mind to these problems, or did not give them priority.

My personal feelings remain that the only direction consistent with his line of thought lies in a conception of economic movements in terms of structural dynamics. But, I must stress, these are personal feelings. The question remains wide open. I fear, it also remain beyond the reach of his manuscripts.

Concluding remarks

The present 'bird's eye view' exercise on Sraffa's manuscripts may well suffer from a somewhat hasty drive to arrive at least at some (provisional) conclusions. But it has been difficult for me not to be deeply impressed by the realisation of the drama that this remarkable man may have lived through, in isolation and silence.

No doubt an evolution in his attitudes, more than in his thoughts and convictions, did take place. He moved from an early volcanic eruption of never-ending criticisms of current economic theory, within a remarkably original conceptual framework concerning the historical development of economic thought–yet remarkably concealed even to his friends–to more mature reflections and a search for a distinction between those traditionally held propositions and conceptions that could directly be shown to be lacking logical foundations and those that should be criticised with great circumspection, given the widespread hostility towards Classical and Marxian views. Finally, he arrived at an extra-cautious attitude that led him to limit his published propositions to a concise, unassailable, analytical framework that could be used, without being accused of ideological prejudices, for a critique of marginal economic theory,…in the future.

The exercise I have carried out may not bring out many indisputable results. But there is one of them that I should like to stress, though we all knew it already, namely the importance of Piero Sraffa's manuscripts–not only for the historians of economic thought, but also for all those who are looking for a solid logical basis for a critique and reconstruction of economic theory.

There are many signs that Piero Sraffa was aware of the importance of his manuscripts. Already in his letter (1974) to John Eatwell and Alessandro Roncaglia he states that: 'As for any publication of my manuscripts after death, any decision will either be in my will or left to my literary executors' (see Appendix, document 1). However, there was nothing in his will on this issue. A literary executor was designated later, in an additional codicil, but without explicit instructions.

The only hint that has been found so far is an incomplete note on the back of a fragment of a calendar. It was noticed, with bewilderment, by Giancarlo De Vivo, while he was looking for something else, in a file containing correspondence with antiquarian book-dealers. It may have ended up there by mistake. The note is written in Italian, in pencil, and has all the appearance of a part of a draft of instructions to be given to the prospective editor(s) of his unpublished manuscripts. It is reproduced here as document 7 in the Appendix. It makes two important points. One is a repetition of the last sentence in the already mentioned Eatwell-Roncaglia letter: 'As far as the work of scholars that were to have access to my MSS, I am against the incomplete quotation of unpublished MSS.' The other point is more substantive: 'that possible introductions and notes to the publication of my MS should be limited to supply the factual elements necessary to the understanding of the said MS leaving aside as much as possible any comment or interpretations of ideas.' Given the vastness of Sraffa's notes, and their fragmentary character, one cannot but admire the far-sightedness and wisdom of these recommendations, even if they are not going to make it easier for the editors to follow them.

Appendix

Documents from Sraffa's unpublished Papers[8]

Document 1

Letter of Piero Sraffa to John Eatwell and Alessandro Roncaglia (kindly made available by Lord Eatwell)

Trinity College
20 Sept. 1974

Dear Eatwell and Roncaglia,

Thank you for your letter dated 8 August. It is most kind of you to take an interest in my old papers.

As regards the English translation of my article in *Annali* 1925, it seems to me impossible to present to a new public in one's lifetime an article without implying that one still agrees with all that it contains, or else pointing out which are the points or aspects on which he has changed his mind. I do not feel that I could do this. I would therefore not wish the article to be published again in my lifetime.

Concerning quotations from my letters or other MSS. I am opposed to quotation from, or incomplete publication of, unpublished manuscripts.

As for any publication of my manuscripts after death, any decision will either be in my will or left to my literary executors.

Yours
Piero Sraffa

Document 2

Sraffa Papers D3/12/11, f. 35 *(attributed dated November 1927)*

Impostazione del libro

L'unico sistema è di far la storia a ritroso e cioè: stato attuale dell'ec.; come vi si è giunti, mostrando la differenza e la superiorità delle vecchie teorie. Poi, esporre la teoria. Se si va in ordine cronol., Petty, Fisiocr., Ric., Marx, Jevons, Marsh., bisogna farlo precedere da uno statement della mia teoria per spiegare dove si "drive at": il che significa esporre prima *tutta* la teoria. E allora c'è il pericolo di finire come Marx, che ha pubblicato prima il Cap., e poi non è riuscito a finire l'Histoire des Doctr. E il peggio si è che non è riuscito a farsi capire, senza la spiegazione storica. Il mio scopo è: I esporre la storia, che è veramente l'essenziale II farmi capire: per il che si richiede che io vada dritto all'ignoto, da Marshall a Marx, dalla disutilità al costo materiale

Translation

Layout of the book

The only way is to make history in reverse that is: present state of ec.; how it has been reached, showing the difference and superiority of the old theories. Then, present the theory. If I go in chronol. order, Petty, Physiocr., Ric., Marx, Jevons, Marsh., it is necessary to make first a statement of my theory to explain where I 'drive at': which means to present first all the theory. And then there is the danger to end up like Marx, who published Cap. and then did not succeed to finish the Histoire des Doctr. And the worse is that he has not succeeded to make himself understood without historical explanation. My purpose is: to present history, which is really the essential thing. To make myself understood: for which what is required is that I go straight to the unknown, from Marshall to Marx, from disutility to material cost.

Document 3

Sraffa Papers D 3/12/4, f. 14 *(dated November 1927)*

…

It is terrific to contemplate the abysmal gulf of incomprehension that has opened itself between us and the classical economists. Only one century separates us from them: [then the following sentence, here reproduced in italics, is added as a footnote] *I say a century; but even ½ a century after, in 1870, they did not understand it. And during the preceding century an obscure process of 'disunderstanding' had been going on*. How can we imagine to understand the Greeks and the Romans? [then the following sentence, again here reproduced in italics, is added as a footnote] *Or rather, the extraordinary thing is that we do understand, since we find them perfect, Roman law and Greek philosophy*. The classical economists said things which were perfectly true, even according to our standards of truth: they expressed them very clearly, in terse and unambiguous language, as is proved by the fact that they perfectly understood each other. We don't understand a word of what they said: has their language been lost? Obviously not, as the English of Adam Smith is what people talk today in this country. What has happened then?

Document 4

Sraffa Papers D 3/12/4 f. 10 *(dated November 1927)*

History

Classical Political Economy (The age of Ricardo) or A.Smith?

From Petty to Ricardo – right conception, fundamental assumptions
Primitive, rudimentary technique

(A.Smith had strong 'vulgar' tendencies: he can truly be said to be the 'founder of modern economies'!)

Vulgar Political Economy (The age of Mill)

From Malthus to Stuart Mill –	All wrong here: they have the wrong conceptions of modern economics and the rudimentary technique of the classical
Period dominated by Mill: Marx stands here towering as the last of the classical amongst the vulgar, just as Smith stood isolated among the classicals, being the first of the vulgar.	

Economics (The age of Marshall)

Since Jevons & Co to Marshall –	highly refined technique, rotten conceptions and fundamental assumptions
But technique so highly perfected that sometimes compels them unconsciously to modify their conscious assumptions (justly contradicting themselves) and thus reaching partially true conclusions	

Note that at the end of the classics developed primitive socialism (Owen, Hodgskin) and *caused* vulgar P.E. At the end of vulgar period came Marx and *caused* economics.

Document 5

Sraffa Papers D 3/12/4 f 12 *(attributed date: November 1927)*

Principio

I shall begin by giving a short 'estratto' of what I believe is the essence of the classical theories of value, i.e. of those which include W.Petty, Cantillon, Physiocrats, A.Smith, Ricardo+Marx. This is not the theory of any one of them, but an extract of what I think is common to them. I state it of course, not in their own words, but in modern terminology, and it will be useful when we proceed to examine their theories to understand their *portata* from the point of view of our present inquiry. It will be a sort of "frame", a machine, into which to fit their own statements in a homogeneous pattern, so as to be able to find what is common in them, and what is the difference with the later theories.

Then I shall go over these theories very cursorily, dealing with them, not at all exhaustively, but examining only those points which are relevant to my present purpose. So, of the Physiocrats, I shall not talk of…the physiocratie, but only of one of its basic points.

Document 6

Sraffa Papers D 3/12/43 f. 4 *(attributed date: post-1945)*

? *Preface*

I intended at one time to add, to include in this work both an introduction which explained its relation to the work of earlier classical econ (writers), (some anticipation of this I have given in Secs…of the Introduction…) and a number of controversial notes on views held by modern economists. I have decided however to send it forth bare as it is and let it be judged on its own merits: if it is found of any interest there will be time to…there may be other opportunities of publishing those additions.

Slogans not used

The St. Syst provides tangible evidence of the rate of profits as a non-price phenomenon.

A Dividend could be declared before knowing what is the price of the company's product.

Document 7

Sraffa Papers H2/89, f. 56 *(date uncertain)*

che le eventuali introduzioni e note alla pubblicazione di miei MS dovrebbero essere limitate a fornire gli elementi di fatto necessari alla comprensione dei MS stessi lasciando da parte il più possibile commenti e interpretazioni di idee.

Per quanto riguarda lavoro di studiosi che avessero accesso ai miei MSS sono contrario alla citazione incompleta di MSS inediti.

Translation

that possible introductions and notes to the publication of my MS should be limited to supply the factual elements necessary to the understanding of the said MS leaving aside as much as possible any comment or interpretations of ideas.

As far as the work of scholars that were to have access to my MSS, I am against the incomplete quotation of unpublished MSS.

Notes

1 I am grateful to the Librarian and to the Master of Trinity College, Cambridge, for all the facilities of which I have been able to take advantage in my visits to Cambridge. I am also grateful for comments on earlier drafts to: Giancarlo de Vivo, John Eatwell, Geoffrey Harcourt. Financial support from the Italian CNR and University and Research Ministry (MURST 40 per cent) is gratefully acknowledged.

2 Only recently has an English translation of Sraffa's article finally appeared in print (in Pasinetti 1998). The translation is the one carried out and discussed with Sraffa in 1973–4 by Eatwell and Roncaglia.
3 This happened in the first half of September 1998.
4 I am grateful to Giancarlo de Vivo for leading me to this note.
5 I shall use the symbols *SP* to refer to excerpts from the *Sraffa Papers,* followed by the section (a capital letter) and the reference numbers.
6 De Vivo (1998) confirms this in his very interesting and detailed analysis of Sraffa's path to the final formulation of the equations of his book.
7 There is an interesting and witty letter from Raffaele Mattioli, dated 15 March 1955, that reveals that they had talked about the intended resumption of Sraffa's project, and of the drastic cuts that needed to be made. Mattioli writes (in Italian): '…I hope you have succeeded in the past 30 days to reduce to half a kilogram the twenty kilos of *paperasse*…and I hope you write the first rough draft of the "modest little book". Keep me informed…' (*SP* D3/11/83, f. 6).
8 I am grateful to Sraffa's literary executor, Pierangelo Garegnani; and also to John Eatwell and Alessandro Roncaglia for permission to publish the (so far) unpublished documents in this Appendix.

9 Some notes on the *Sraffa Papers*

Giancarlo de Vivo[1]

I should like to start these notes with a warning as to the necessity of using great caution in the interpretation of what is today to be found (or *not* to be found) at Trinity College, Cambridge, among Piero Sraffa's papers. There are several reasons for this. The first is that the papers have been ordered, and most probably weeded, several times. It is very clear that Sraffa himself must have ordered and thrown away some of the papers, as after all is only too natural to expect.[2] (For instance I can only think that the fact that virtually nothing survives of drafts of the introductions to the Ricardo volumes derives from a deliberate choice of Sraffa.[3]) It is also known that John Eatwell and Alessandro Roncaglia helped Sraffa to reorder his papers in the early 1970s, and later some work on the papers was also done by Sraffa with the help of Pierangelo Garegnani, and, towards the end of his life, with A.Campus. Moreover, the papers were removed after Sraffa's death, and although Garegnani tells us they were catalogued and numbered rather early after 1983, I think it is not impossible that something significant may have been lost in one way or another.

Any reconstruction of what is in the papers must be selective, not only because the papers themselves as they are extant today are a selection of the materials which Sraffa produced in his research work, but also because, given the sheer amount of documents, any interpretation of their contents must choose to emphasise one aspect rather than another, according to the interpreter's tastes, inclinations, and whims; and also because some parts must be left out altogether given that not everything which is found is comprehensible. It is almost certainly possible to reconstruct Sraffa's path in different (even perhaps to some extent contradictory) ways, basing one-self on some papers rather than others.

It is also important to bear in mind another element, which Luigi Pasinetti also rightly emphasises in his chapter, i.e. that in reading the documents in the archive at Trinity we must as much as possible forget the results which Sraffa reached in the end, and which we know from *Production of Commodities by Means of Commodities:* obviously enough the results were not available to him when with great labours he was slowly progressing in his research. It is however clear (and to some extent surprising) that from very early (actually,

as early as 1927) Sraffa conceived that the outcome of his research would be the writing of a book. This is remarkable, and I think it also shows that Sraffa must have had a deep conviction from the very beginning that there was something important in what he was trying to do–notwithstanding all his worries till the very last moment before publication about the worth of his results.

In reading Sraffa's papers it is also necessary to use caution with respect to the problem of chronology. Sraffa dated most of the papers himself, and quite a few of them must have been dated more or less as he was writing, because not only the year, but also the month and the day are normally given, something which would have been impossible had they not been dated immediately. This habit (which however Sraffa seems to have developed after the 1920s) was due I believe to the fact that Sraffa left the work dormant for long periods of time, and dating every document meant that a mix-up could not happen. It must have been basically a way of numbering the pages[4] However, apart from some particular problems which could arise even with the later papers,[5] it must be kept in mind that the papers of the initial period, the late 1920s, were clearly not dated immediately, and in fact they are dated with less precision, often as 'Winter 1927', 'Summer 1929', or the like, and often what is dated is not the single document, but the folder wherein it is contained. It is far from impossible that for one reason or another documents may have been attributed the wrong date (also simply because they have ended up in a file which, at least as far as its date is concerned, is the wrong file). Moreover, some documents have additions, corrections, etc, which are often not dated; and some of the additions may be even impossible to detect.[6]

After this much too long premise, I should like to briefly comment on a point made by Garegnani that Sraffa's thought underwent a radical change between 1927 and 1928.[7] Garegnani has provided no evidence on this alleged change, and one may assume he will provide it in the future. I should, however, be very surprised if this happened: it seems to me that such a radical change in Sraffa's thought did not really take place.

Garegnani's point appears to be that between 1927 and 1928 Sraffa introduced, or at least went much more deeply into the notion of 'physical real cost', and this allowed him to abandon his Marshallian or semi-Marshallian interpretation of Classical economic theory, as a theory which could be reconciled with Marshall's own, once the latter had been amended of its mistakes. This radical change Garegnani ascribes in particular to 1928, which he regards as the starting point of the interpretation of Classical economic theory that Sraffa will put forward in his 1951 introduction to Ricardo. Now, the notion of 'physical real cost', i.e. the idea that the cost could somehow be reduced to a quantity of commodities, Sraffa had derived from Petty and the Physiocrats, and is different from the idea that cost could be reduced to labour. Indeed, Sraffa initially thought that only Petty and the Physiocrats had the right notion of cost (as 'physical real cost'), and disapproved of Smith, Ricardo

and Marx, who, as Sraffa put it in a document of November 1927, 'began to corrupt the old idea of cost–from food to labour' (*SP*, D3/12/04/02/i, date on folder). But while it is certainly true that at the very beginning of his research Sraffa's heroes were William Petty and the Physiocrats–'it was only Petty & the Physiocrats who had the right notion of cost as "the loaf of bread"'[8] (*SP* D3/12/04/04, in folder dated End of November 1927)–this, I believe, was only a brief and very early mood, perhaps a sort of reaction to Marshall. Indeed, the great appeal of the notion of cost as 'the loaf of bread' for Sraffa was that its firm materialistic approach was the farthest possible from what he regarded as the study of 'illusions',[9] of which marginalist theory according to him consisted. Labour, or 'toil and trouble', could easily be seen as 'sacrifice' and therefore be corrupted into a subjectivist notion. Very soon, however, Sraffa came to regard Marx as his hero, and in papers contained in the same folder (therefore bearing the same date) as the passage on Marx's (and Smith's and Ricardo's) 'corruption' of the idea of cost as the 'loaf of bread', Sraffa already envisaged that 'the ultimate result' of his own work would be 'a restatement of Marx, by substituting to his Hegelian metaphysics and terminology our own modern[10] metaphysics[11] and terminology'.[12] As far as the more general interpretation of Classical theory is concerned, however, I believe that it was only during the 1930s, when Sraffa appears to have more or less completely abandoned work on his own research for a decade to plunge himself 'like a maniac' into the editing of Ricardo,[13] that he really reached a vast first-hand knowledge of the Classical economists, and understood their theory as a completely different theory from that of Marshall and the other marginalists. Before the 1930s, it seems rather clear that his knowledge of the classical economists was not extensive, and much filtered through, and influenced by, other authors, notably Cannan, whose lectures at the London School of Economics he had attended in 1921–2. and who is very often (and far from uncritically of course) referred to by Sraffa in his notes of the 1920s.[14] Another obvious source is Marx himself: indeed, it seems to me that in the 1920s his knowledge of Marx was both more extensive and deep than that of the Classical economists. In particular, Marx's *Theories of Surplus Value* was for him, in addition to Cannan's works, the principal authority on the Classical economists. Also, it seems to me that Sraffa's idea that what he was doing was to follow a sort of intermediate path between Marshall and the Classics, incompatible with neither of them, is an idea which survives well beyond 1927–8.

I must also say that it is not very clear as to what the alleged great difference between the conception of 'physical real cost' that we find in 1928 with respect to the one which we already find in 1927 would consist. It is obviously true that 1928 comes after 1927, and that in 1928 some conceptions (including perhaps that of 'physical real cost') are more developed than in 1927, but it is far from clear that what takes place between 1927 and 1928 is a radical break rather than a normal development and clarification in his thought.

If I was to attempt an exercise in chronology, and say which are the points of time when the most important developments of Sraffa's thought took place, I should say they are 1927 and the early 1940s.

Very early in 1927 an important change took place, in that Sraffa, having finished work on his *Economic Journal* article on Marshall, must have started the preparation of his Cambridge lectures (the first intimation that he could be appointed lecturer seems to have come from Keynes in January 1927, while the official letter is of 31 March). My idea is that when he started preparing his *Lectures on Advanced Theory of Value* for Cambridge (which were supposed to start in October 1927) Sraffa began a (re)reading of the Classical economists, and in particular of Marx. After all, even the expression 'Theory of Value' was associated more with the Classics and Marx than with the marginalists, by whom the theory of value was often referred to as the theory of 'prices'. That Sraffa must at this stage have gone back to studying Marx however is not mere conjecture, because there are many notes from reading Marx (but also the Classical economists, and others, like Pareto, Cassel, etc.) dated 1927 and 1928. As far as Marx is concerned, there are several notes from *Theories of Surplus Value,* the socalled volume IV of *Capital,* which had been edited for publication by Kautsky in 1905–10. Sraffa quotes *Theories of Surplus Value* from the French edition, and in fact he used to refer to *Theories of Surplus Value* as the *Histoire* (even in his letters to Antonio Gramsci), from its French title *Histoire des doctrines économiques*. In fact it is clear (also from an annotated copy of the book), that he must have studied *Theories of Surplus Value* from the French edition. This implies that this study could not have started earlier than 1924–5, when the French edition was published.[15] (Also the quotations from the other volumes of *Capital* in Sraffa's 1927–8 notes are from the French edition.) Although the French edition of the first three volumes of *Capital* had been published much earlier than the *Histoire* volume, and Sraffa had almost certainly read *Capital* (at least volume I) earlier,[16] it is safe to assume that probably in early 1927 he read (or rather at least in part *reread*) Marx's *Capital,* and in particular the *Histoire,* with a view to preparing his *Lectures on Advanced Theory of Value.*[17] (Significantly enough, when asked by the imprisoned Gramsci for relevant literature on Ricardo, Sraffa mentioned Marx's *Histoire* as an obvious reference.[18])

A sketch (in a folder dated End of November 1927) of how he intended to start his lectures reads:

> I shall begin by giving a short 'estratto' of what I believe is the essence of the classical theories of value, i.e. those which include W.Petty, Cantillon, Physiocrats, A.Smith, Ricardo & Marx.
>
> (*SP* D3/12/04/12)

Although he only partially followed this project (the initial part of the lectures, on Classical theory, being more historical than analytical), the fact

that in 1927 he had this intention gives support to my contention that while preparing his lectures he must have (re-)read Marx and the Classical economists. It is impossible to say with certainty whether it was the need to prepare lectures on the theory of value that brought him to read Marx and the Classics, or the other way around, i.e. that he decided to give lectures on the theory of value because he had started a study of Marx and the Classics. I think however that the former is probably the case.

The work on his lectures, and particularly the study of Marx, merged very soon with work on what Sraffa calls his 'equations'; in the autumn of 1927 he was able to show these equations (in a version which has not reached us, at least not as such) to Keynes and Pigou. And in fact Sraffa stated in a provisional version of the introduction to his book that '[t]he matter of the first two chapters was completed by 1928, when it was submitted in a preliminary form to Mr J[ohn] M[aynard] K[eynes]' (*SP*, D3/12/46/22). What I think is important to understand is that Sraffa's work on the 'equations' had less to do with his study of the Classical economists (Ricardo included) than with Marx. Sraffa himself directly and explicitly linked it with Marx, and not with the Marx of Volume III of *Capital,* but with the schemes of reproduction of volume II.[19]

Much was still to be done when in the early 1930s Sraffa interrupted his research for about a decade, and it seems possible that at this point Sraffa had serious doubts as to whether any relevant results could be obtained from the 'equations', first and foremost the demonstration that they had a (unique non-negative) solution. However, he resumed his work in the early 1940s, and the crucial breakthroughs happened in those years: it was in 1942–4 that all the major points were reached. Sraffa himself, in the book's introduction, tells us that after 1955 what he did was basically 'put together' the book 'out of a mass of old notes'. Taking into account that from 1945 to 1955 he seems to have done little work on the book,[20] we must say that by the end of the war all the major results had been obtained. Indeed, this is borne out by the contents of the papers now in the archive at Trinity: the conception of the standard commodity, and the related construction of the wage-profit-price relationship, its implications for the critique of marginalist theory, the uniqueness and non-negativity of solutions for his system (at least in the case of single product industries) were all points which had been established by the end of the war. We cannot here go into any detail on this,[21] but I should like to point out that although Marx was certainly the main inspiration behind Sraffa's book, Ricardo proved very important at one crucial juncture at least. Very early in 1942, starting from Ricardo's corn ratio theory of profits, Sraffa got to see the point that in every circular system of production (even when the special corn/corn reasoning does not apply) there must be a maximum rate of profits. This was a crucial step in the route which brought him to reach the conclusion that a linear relationship could be built between wages and the rate of profits, and therefore to show that his 'equations' had a solution. This is of course rather interesting in itself. The point I should like

to make here however is that this appears to be inconsistent with what Sraffa writes in his 'References to the Literature' (Appendix D), where Sraffa (§3) in fact gives Marx as the source for the notion of a maximum rate of profits. Also, Sraffa writes that 'it was only when the Standard system and the distinction between basics and non-basics had emerged in the course of the present investigation' that his interpretation of Ricardo's 'corn-ratio' theory of profits 'suggested itself as a natural consequence' (§1). Both these points are contradicted by what emerges from the papers.

So far as the first point is concerned, the evidence for Marx being the source for the notion of a maximum rate of profits is given in the Appendix as (1) 'an incidental allusion to the possibility of a fall in the rate of profits "even if the workers could live on air"',[22] and (2) Marx's opposition to Smith's idea that the price of every commodity ultimately resolves itself into wage, profit and rent (which would imply that production is not circular, and therefore there would be no maximum limit to the rate of profits). Both points have always struck me as rather slim evidence: I think that it would be almost impossible for anybody who did not already have the notion of a maximum rate of profits to derive such a conception from those two points of Marx's. As a matter of fact, the source for Sraffa's conception of a maximum rate of profits was Ricardo. If I was to speculate about why Sraffa overlooked Ricardo, in his a posteriori reconstruction of his sources on the maximum rate of profits, I should say there might have been two reasons. The first could be that once Ricardo abandoned the corn-ratio theory, and came to rely on the reduction to labour (in a finite number of stages), the maximum rate of profits disappeared from his system. The second reason could be that use of the maximum rate of profits to reach the results about the wage-profit relationship was made by Sraffa in a context much nearer to Marx than to Ricardo.

As for the alleged derivation of Sraffa's interpretation of Ricardo's cornratio theory from the conception of the Standard System, this is flatly contradicted by what one finds in the papers, where indeed the corn/corn reasoning (explicitly attributed to Ricardo) is (repeatedly) spelled out years before the appearance of any hints of the conception of a Standard commodity (which first appears in May 1943). For this apparent contradiction between the statement in the 'References to the Literature' and the facts as they emerge from the papers in the archive I find it difficult to even conjecture an explanation, but it seems to me that it is worth pointing out that there is this unresolved problem.

Notes

1 The present chapter is a revised version of the paper given in Turin for the Sraffa centenary conference on 15 October 1998, where the present writer acted as the discussant of the papers given by Pierangelo Garegnani and Luigi Pasinetti.

2 He had to move at least twice during his life in Cambridge: he first lived in accommodation rented from King's College, at 17b St Edward's Passage, whence he moved to Trinity when he was awarded a fellowship in 1939 (his mother continuing to live at St. Edward's Passage until her death). At Trinity, he changed his rooms at least once.

3 It is remarkable that Sraffa in 1939 must have sent Keynes drafts of some introductions, on which Keynes commented. The only remnant I have been able to find of what Sraffa had written is a single sheet of paper, a sort of title page to one of them. It bears the typed inscription 'Introduction to the Essay on Profits [2nd version of the opening]', and some comments jotted by Keynes in pencil. None of the text which must have followed this sheet is preserved (*SP* D3/11/65/33).

4 I don't think that Sraffa dated his papers having in mind future readers of his MSS, although it is clear that (at least late in life) he must have thought that his MSS would probably be studied by others (see Sraffa's statement about his MSS quoted by Pasinetti in the appendix to his chapter, document 7).

5 As is only too natural to expect, there are slips in the dating of some papers. Some of these slips are detectable, because Sraffa (curiously similar in this to Ricardo) was in the habit of often writing notes on paper 'recycled' from other uses: old bills, leaflets, letters, etc. Sometimes these bills etc. have a date of their own (or can be dated in some other way) and in a few cases at least it comes out that the date which Sraffa writes on his notes must be wrong. I am not aware of many such cases, but there might be others which it could be impossible to detect.

6 The typical instances are additions which might have been made at the bottom of a sheet, and additions made by deleting something which had been written previously, in documents written in pencil (of which there are quite a few).

7 For Garegnani's position on 1927–28 as the 'turning point' in Sraffa's thought, see Garegnani (1998a):152–3).

8 We may notice that Sraffa saw with favour Ricardo's early usage of the strange term 'price of wages' among the many expressions he used for this distributive variable. Sraffa actually maintained that 'price of wages' was the 'proper name', and criticised Marshall for criticising Ricardo (see *SP* D3/11/37). The reason was that, according to Sraffa, Ricardo's wording emphasised a distinction between wages (the real thing, the 'loaf of bread') and their price.

9 'P.[olitical] E.[conomy] was a science of things, Economics is a science of illusions' (*SP* D3/12/10/61).

10 This word is inserted.

11 At this stage Sraffa does not use the term 'metaphysics' in a negative sense. It appears to be a critical reworking of the distinction between the 'philosophical' and the 'empirical' theory of value which is to be found in Wieser's *Natural Value* (1893), in particular pp. xxvii–xxix, and in Whitaker (1904), in particular chapter I. Also, Sraffa quotes Edgeworth's mention of the 'higher theory of value' as 'the methaphysics of political economy' (Edgeworth 1925, vol. III:59).

12 *SP* D 3/12/04/15. We may also notice that in another document in the same folder Sraffa writes that it is 'terrific' that '[i]n the middle of the 19th century a man [i.e., Marx] succeeds, either by accident or superhuman effort, in getting again hold of the classical theory: he improves it, and draws its practical consequences from it' (*SP* D 3/12/04/17, folder dated 'End of November 1927').

13 Thus Keynes wrote in a letter to Gregory of 18 March 1930 (*SP* D 3/11/62).

14 Cannan's 1929 book is based on the lectures which he for many years gave at the London School of Economics. Sraffa of course knew and used also Cannan's *History*.

15 It is by no means certain that before the Second World War Sraffa owned a copy of the German edition (which had been published in 1905–10 by Kautsky). And there was no Italian edition until the 1950s, when an Italian translation was published by Einaudi at Sraffa's suggestion.
16 Probably in the 1915 Italian edition of volume I (no complete Italian edition of volumes II and III of Marx's *Capital* were available in Italian until after the Second World War).
17 In Sraffa's library there are heavily annotated copies of volumes I–III of *Capital* in the French edition, which was published in 1872–5 (Vol. I), and 1900–1 (Vols. II–III). There is a copy of the 1915 Italian edition of Volume I of *Capital,* also containing many annotations, but I think they are likely to be of an earlier date than the annotations in the French edition. Sraffa's references to Marx's *Capital* in the papers are generally to the French edition.
18 See Sraffa (1991:74).
19 Sraffa for instance annotated his copy of the 1900 French edition of volume II of *Capital* to the effect that the equations of simple reproduction are his '1st equations', i.e. the equations without surplus.
20 In particular, in 1948 the work on Ricardo restarted in earnest, and did not end until 1955, with the publication of volume X.
21 For a full discussion of the route followed by Sraffa in reaching the results of his book I may refer the reader to another work of mine: 'On Sraffa's path to *Production of Commodities by Means of Commodities*', paper presented to the conference *Piero Sraffa's work and personality. Contributions in the centenary of his birth,* Rome, December 1998, mimeo. An Italian version of this paper is forthcoming in the proceedings of that conference.
22 The reference is to p. 290 of vol. III of *Capital* (Chicago 1909).

10 From the 1925–6 articles to the 1960 book

Some notes on Sraffa's not so implicit methodology

Andrea Salanti and Rodolfo Signorino[1]

1 Introduction

Despite the great bulk of the existing literature on Sraffa and the presently growing outpouring of studies in economic methodology, methodological appraisals of Sraffian economics and of the economics of Sraffa have been so far confined to some occasional discussions. Moreover, with only a very few exceptions,[2] possible methodological issues lying behind Sraffa's critical assessment of Marshall's theory of value in 'Sulle relazioni fra costo e quantità prodotta' (1925a) and in 'The laws of returns under competitive conditions' (1926a) have been almost completely ignored. As an explanation it is possible to point out that (1) in his published works Sraffa never tackled extensive discussions of methodological issues and that (2) *Production of Commodities by Means of Commodities* (1960a) and the related debates on the theory of capital and distribution have attracted most attention from commentators.[3] Nonetheless, it seems possible to reconstruct some features of Sraffa's methodological (pre)conceptions through a methodologically-aware reading of his published writings, supplemented with reference to his unpublished manuscripts stored in the Wren Library of Trinity College, Cambridge (UK), and to the methodological positions subsequently advocated by scholars who at various times were closely associated with Sraffa in study and research.[4]

Our main goal in this chapter is to provide a tentative answer to the following question: what are the methodological similarities and differences between Sraffa's criticism of Marshallian supply and demand theory of value in the 1920s and his 1960 proposal to bring back the classical notion of prices of production as the mainstay of economic theory? The answer to this question will also serve as a starting point for a more comprehensive assessment of the inner methodology of Sraffa's economic thought.

The chapter has six sections. Section 2 is devoted to an appraisal of Sraffa's 1925–6 critiques of Marshallian partial equilibrium models. We argue that Sraffa was not interested in unearthing possible non sequiturs within the Marshallian theory of value; but that he tried to reconstruct the latter in a logically consistent way in order to determine the exact boundaries

of its empirical domain. The point is further developed in section 3 where we focus on possible differences between Marshall and Sraffa on the relation between theoretical propositions and empirical facts. Here our interpretation is clarified by reference to Sraffa's unpublished manuscripts.[5] Section 4 deals with the related issue of the supposed priority of logical consistency over empirical relevance in the assessment of economic theories. This is a basic but still controversial point in the reconstruction and assessment of Sraffa's methodology (and of the methodology of the so-called 'Sraffian' economists as well). More precisely, we claim that right from the 1920s Sraffa considered the existence of a trade-off between logical consistency and empirical relevance within an economic theory as a fatal vice. Any theory which 'cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain' is to be discarded. This methodological stance is characteristic of Sraffa's entire whole intellectual career. In contrast, Section 5 focuses on a possible element of methodological discontinuity between the 1920s articles and the 1960 book. The peculiar way Sraffa builds up his formal models in *Production of Commodities* raises the problem of the methodological status of exogenous variables in economic theory. We suggest that Sraffa's 1960 choice of what has to be considered as 'given' may be justified in the light of a strategy of research which is usually labelled as 'piecemeal theorizing' (Hausman 1981). What needs to be stressed is that, within such a methodological framework, each model is built to solve a specific problem or very few specific problems: the assumptions or the formal structure of a given model cannot be mechanically extended to face other problems. This turns out to be a radically different approach from that generally adopted by neoclassical economists (Hausman 1992): the neoclassical view on theory-making may, in fact, be summarised by saying that all economic models must be built on the common basis of a few basic premises, individual maximising rationality and subjectivistic equilibrium being the most well-known examples. Granted our interpretation, we may add that Sraffa endorsed 'piecemeal theorizing' as a fruitful strategy of research in economics only *after* the long decades devoted to a careful study and rational reconstruction of the 'submerged and forgotten' point of view of Classical economists. Finally, section 6 sketches some tentative conclusions.

2 A methodological appraisal of Sraffa's 1925–6 critiques of Marshall

As has been shown by one of the present authors (Signorino 1998), Sraffa's 1925–6 articles are basically directed at identifying on purely logical grounds the empirical domain of Marshallian partial equilibrium models in a competitive framework and in a monopolistic framework. To put it in a nutshell, Sraffa reconstructed in a logically consistent way Marshallian partial

equilibrium models in order to single out the logically admissible accounts of empirical situations to which those models could be applied and those situations to which they could not. In both cases Sraffa's strategy seems to be as follows: first, he clarifies the basic explicit premises of the model under scrutiny, then he works out the additional implicit assumptions which are required to gain logical consistency and, finally, he looks for the empirical situations logically deducible from the model so reconstructed.

According to Sraffa the explicit basic premises of a Marshallian competitive model are:

1a the symmetrical theory of value,
2a the methodology of partial equilibria, and
3a the static theory of perfect competition.

Such premises require us to rule out:

ia constant returns to scale,
iia any non-negligible interdependence among the cost functions of different industries,
iiia any non-small variation in the quantity produced, and
iva firm-internal economies.

The emerging conclusion is that Marshallian economists may consistently draw a positively-sloped supply curve only in the case of those industries which employ the whole amount available of a given factor (specific factors industries) and a negatively-sloped curve for those industries which benefit from 'external-internal' scale economies (specific external economies industries). On the other hand, it appears that positively-sloped supply curves cannot be drawn in the case of industries employing non-specific factors; while negatively-sloped supply curves are ruled out for industries whose cost function are affected by 'external-external' economies (non-specific external economies industries).[6]

Similarly, the explicit basic premises of a Marshallian monopolistic model are:

1b the symmetrical theory of value,
2b the methodology of partial equilibrium, and
3b the static theory of monopoly.

Such premises require us to rule out:

ib constant returns to scale,
iib any non-negligible interdependence among the cost functions of different industries,
iiib any non-small variation in the quantity produced,

and to assume that:

ivb variable returns arise solely from firm-internal economies,
vb the commodities produced by the individual firms within a given industry are not perfectly homogeneous, and
vib each entrepreneur is a price-maker inside his own 'special market'.

Finally, the empirical situations logically admissible within this model are a multiple monopoly with indeterminate equilibrium à la Edgeworth, a multiple monopoly with determinate equilibrium, and a situation similar to that of a single monopolistic association. In the first case market equilibrium is not even determinate. In the third case market equilibrium is determinate and it is possible to state general propositions about the equilibrium market price; but Sraffa considers this case as empirically irrelevant. Finally, in the second case of a multiple monopoly with determinate equilibrium, market equilibrium is determinate but:

> [t]he conclusion that the equilibrium is in general determinate does not mean that generalising statements can be made regarding the price corresponding to that equilibrium; it may be different in the case of each undertaking, and is dependent to a great extent upon the special conditions affecting it.
>
> (Sraffa 1926a:546)

Moreover, the individual demand and supply curves 'can by no means be compounded so as to form a single pair of collective demand and supply curves' (Sraffa 1926a:546). Therefore, to drop the theory of perfect competition and to replace it with the theory of monopoly allows us to encompass firm-internal economies within a Marshallian partial equilibrium model; but this articulation of Marhall's theory of value does not greatly improve its explanatory power: non-specific factors industries as well as non-specific external economies industries fall outside the boundaries of the empirical domain of Marshall's theory.

Hence a full appreciation of the relevance of Sraffa's findings requires an explicit discussion of the role played by factors specificity and external economies specificity within a logically-reformulated Marshallian model. As is well known, such a model involves *cæteris paribus* assumptions. Assumptions of this kind, when considered from the point of view of formal languages, may appear relatively simple and unproblematic. Indeed, almost always, they can be reduced to the standard forms f(X)=0 or f'(X)=0 for any X involved in the clause (Xs are often referred to as 'exogenous variables').[7] On the other hand, their semantics is terribly ambiguous if considered from the point of view of their methodological status. To put some order in this matter it is possible to follow Alan Musgrave's (1981) proposal of distinguishing among 'negligibility',

'domain' and 'heuristic' assumptions as further refined by Uskali Mäki (1994 and 1998). Instead of giving formal definitions, an example will give a better idea of what such a classification is about.[8]

Consider the following *cœteris paribus* assumption:

[CPA] Factor supply curves are infinitely elastic.

This sentence can be rephrased in at least three different ways, each one corresponding to a different interpretation of the same formal clause. A first possibility is to interpret it as a *domain* assumption:

[DA] The theory only applies if [CPA] is factually true.

This is, in a sense, the most restrictive interpretation. Note, however, that [DA] may be true even if [CPA] is factually false and this is always the case when [CPA] is empirically false but the theory is logically sound. A second possibility is to interpret [CPA] as asserting the *negligibility* of the involved variable(s):

[NA] Any actual change in factor prices has negligible effects on the matter under investigation.

As far as the empirical relevance of the theory is concerned, [NA] makes a stronger claim than [DA]. It requires, indeed, not only logical consistency, but also the possibility of disregarding the concerned causal links. In a sense it might be said that [NA] implies a wider domain than [DA], because [NA] may be true only if [DA] is true but not vice versa.[9] The third possibility is to interpret [CPA] as a *heuristic* assumption:

[HA] Assume, as a first step, that [CPA] holds.

In this case there is little scope for discussing the truth-value of the proposition. [CPA] is implicitly maintained to be descriptively false (otherwise there would be no need to reinterpret it as [HA]), and [HA] makes quite a loose assertion. The relevant issue about [HA] is not its truth but rather its fruitfulness, which can be demonstrated only if further steps will follow.[10],[11]

Having in mind Musgrave's classification it is possible to rationalize Sraffa's view concerning factors specificity and external economies specificity within a Marshallian model. A first possibility is to interpret them as heuristic assumptions. This would mean to defer the analytical treatment of non-specific factors and non-specific external economies to future articulations of Marshall's theory. This interpretation does not seem to be Sraffa's own. In connection with the case of a non-specific factor Sraffa writes:

> Our argument is not concerned with the greater or lesser approximation of the assumption that the prices and quantities of the other commodities which use a factor in common with the commodity under consideration, remains unchanged. Our argument is that assumption is absurd, and contradicts the preceding hypotheses, for the increase in production of a commodity leads to an increase in cost that has equal importance for that commodity and for the others of the group; so that it cannot be taken into consideration for one and ignored for the others.
>
> (Sraffa 1925a:326; English translation 1998:361)

The same argument applies to the case of non-specific external economies. Sraffa's opinion appears to be that it is not possible, within a partial equilibrium model, to work out successive approximations taking into consideration non-specific factors and non-specific external economies.[12] A second possibility is to interpret factors specificity and external economies specificity as negligibility assumptions. Once more, this does not seem to be Sraffa's interpretation. On the one hand he notes that: 'These causes of variation of cost…must of necessity be considered to be negligible in the study of the particular equilibrium of an industry' (Sraffa 1925a:328; English translation 1998:361).

On the other hand, the element of necessity in Sraffa's statement comes from the fact that non-specific factors and non-specific external economies do affect the predictions of a Marshallian model and cannot, therefore, be interpreted as negligibility assumptions in Musgrave's sense. The conclusion is thus ready at hand: factors specificity and external economies specificity within a logically-reformulated Marshallian model belong to the category of domain assumptions. This model simply does not hold when non-specific factors and non-specific external economies are involved.

An appraisal of Sraffa's results may thus be the following:

a Marshallian theory is endowed with an empirical domain much narrower than that assumed by Marshallian economists;
b the narrow domain of Marshall's theory causes serious problems when such a theory is employed in applied research such as those advocated by Clapham in his famous 1922 article on 'empty economic boxes'.

The last point deserves to be stressed since testability and scientific relevance depend closely on the realism of domain assumptions. Indeed:

> If we value testability, we must hope that our domain assumptions are not always false; indeed, we must hope that they are true of as many actual situations as possible…. [C]oncerning domain assumptions [Friedman's] dictum that 'the more significant the theory, the more unrealistic the assumptions' is precisely the reverse of the truth. The more unrealistic domain assumptions are, the less testable and hence less significant is the theory.
>
> (Musgrave 1981:382)

Almost the same conclusion may be reached through a different route. Suppose that Sraffa was appraising Marshall's theory according to the requirements for a nomological-deductive model. This amounts to evaluating the explanatory power of that theory according to the logical and empirical conditions of adequacy put forward by Hempel and Oppenheim in their famous contribution on the logic of scientific explanation:

> Logical conditions of adequacy: (R1) The *explanandum* must be a logical consequence of the *explanans*.... (R2) The *explanans* must contain general laws and these must actually be required for the derivation of the *explanandum*.... (R3) The *explanans* must have empirical content; *i.e.* it must be capable, at least in principle, of test by experiment or observation.... Empirical conditions of adequacy: (R4) The sentences constituting the *explanans* must be true.
>
> (Hempel and Oppenheim 1965 [1948]:247–8)

As we have seen, according to Sraffa the *explanans* of any logically-reformulated Marshallian model includes the symmetrical theory of value and the methodology of partial equilibria. The *explanandum* should include industries employing specific and non-specific factors, as well as industries whose cost functions are affected by specific and non-specific external economies. Sraffa's analysis can be interpreted as implying that Marshall's model does not fulfil (R1), in the sense that only part but not the whole of the *explanandum* can be logically deducted from the *explanans*. All this implies that the model is at least in principle testable (that is, (R3) is fulfilled), but it is quite difficult to test it because of the limited empirical content of the portion of the *explanandum* actually explained by the model. Finally, as is made clear by Sraffa in his 1930 reply to Robertson, the implicit assumptions in the *explanans* of Marshall's model are not in general true of real world economies: 'I am trying to find what are the assumptions implicit in Marshall's theory; if Mr. Robertson regards them as extremely unreal, I sympathise with him' (Sraffa 1930a:93). Hence, the fulfilment of (R4) may be doubted. Therefore it is possible to claim that Sraffa does not confine himself to the very narrow exercise of searching for possible violations of (R1), that is, his criticism is not simply a logical or internal one. On the contrary, he provides a careful scrutiny of the explanatory power of a logically reformulated Marshallian model. As Hempel and Oppenheim clearly show, for such a wider exercise one must consider *both* the logical and the empirical implications of the model under scrutiny.

To sum up: the logical structure and the theoretical goals of Sraffa's 1925–6 criticism of Marshall's theory can be reconstructed starting from the presumption that Sraffa held an 'aggressive' methodological approach. Whenever the logical reconstruction of a theory shows that the theory under scrutiny is endowed with a negligible empirical domain, systematic failures in

the empirical corroboration of its theoretical propositions are to be considered as the manifest and compelling proofs of the lack of explanatory power of the theory itself. The theory is thus to be discarded.

3 Marshall vs Sraffa on the relation between theoretical propositions and empirical facts

In the first section of 'The laws of returns under competitive conditions', Sraffa explores the reasons that controversies in the field of the theory of value have almost ceased in contemporary economics. First he reminds his readers that the theory of value in the period of Ricardo, Marx and of the 'bourgeois economists' constituted the core of some highly influential and strongly competitive socio-political doctrines. This explains the heat of nineteenth-century controversies in this field. According to Sraffa the situation has radically changed since the publication of Marshall's *Principles of Economics:* the controversies have been replaced by an almost unanimous agreement based on the universal acceptance of the symmetrical theory of value. Sraffa offers two explanations for this change. The most immediate and comforting is that 'from these clashes of thought the spark of an ultimate truth had at length been struck' (Sraffa 1926a:535). The alternative and more sceptical explanation is that the theory of value has become, in John Maynard Keynes' words, 'an apparatus of the mind, a technique of thinking' (ibid.), an accepted theory to be learnt by all young students of economics and which is not questioned by mature scholars. The reason why the symmetrical theory of value no longer stimulates political or intellectual passions is that it does not provide a 'vision' of the society as it is or as it should be: theoretical debates on value have dried up because of the indifference of the participants towards keeping them alive. The folder D1/20, 'General', dated pre-1928, of Sraffa's unpublished manuscripts contains an interesting note in this regard:

> Nel 1848 J.S.Mill scriveva che 'nulla ormai rimane che abbia più bisogno di essere delucidato nelle leggi del valore'. Questa opinione doveva esser ben diffusa perché per vent'anni un ristagno generale nello sviluppo della teoria mostrava come ormai più nessuno si curasse di rivederne le basi. Oggi sembra dominare fra gli economisti la stessa impressione che nulla ci sia da aggiungere o modificare di essenziale nella teoria economica. V[edi]. p[er]. es[empio]. quanto scrivono Pantaleoni (necr[ologio]. di Pareto in E[conomic]. J[ournal].) e Keynes (Introd[uzione]. ai Cambridge Ec[onomic]. Man[uals].). A tale impressione corrisponde di nuovo un arresto negli studi teorici. Come dice Keynes 'important improvements in its elements are becoming rare'. E conclude che il compito dell' 'economista di professione' oggi consiste o nel cercare di conoscere i fatti importanti e nell'applicare ad essi i principii economici o diffondere la

> conoscenza dei metodi dello studio. Dopo l'affermazione di Mill sono occorsi vent'anni perché sorgesse Jevons: eppure allora l'attacco era facilitate dall'essere la scienza economica compattamente costituita da una serie di leggi chiare e unanimamente accettate. Ma oggi 'the Theory of Economics does not furnish a body of settled conclusions immediately applicable to policy. It is a method rather than a doctrine…' e stavolta probabilmente dovremo aspettare Jevons per più di vent'anni! Tuttavia la dottrina odierna è ben lontana dall'essere del tutto soddisfacente.
>
> (*Sraffa Papers (SP)* D1/20:7)[13]

But, for Sraffa, that is not the only reason why controversies in the field of the theory of value have ceased. The other reason is that when theorists overtly report the deficiencies of supply curves, they are 'promptly silenced':

> That [the] foundations [of the supply curve] are less solid than those of the other portions of the structure is generally recognised. That they are actually so weak as to be unable to support the weight imposed upon them is a doubt which slumbers beneath the consciousness of many, but which most succeed in silently suppressing. From time to time someone is unable any longer to resist the pressure of his doubts and expresses them openly; *then, in order to prevent the scandal spreading, he is promptly silenced, frequently with some concessions and partial admission of his objections, which, naturally, the theory had implicitly taken into account.*
>
> (Sraffa 1926a:536; emphasis added)

As stressed by one of the present authors (Signorino 1998), Marshallian economists have developed a 'defensive' methodological strategy which allows them to counteract criticisms concerning the uncertain empirical foundation of their theoretical categories and to consider them as 'implicitly foreseen exceptions' to an otherwise general rule. It is like saying that, although it is a matter of common observation that balloons full of a gas lighter than air rise in the sky and do not fall down to earth, nobody stops believing in the general validity of the law of gravity. In this way both Marshall (1961 [1890]) and John Neville Keynes (1891) defended pure theory from the criticisms of the economists of the historical school and defined (what they assumed to be) the 'correct' relation between pure theory propositions and empirical evidence. Moreover, it was generally maintained that the heuristic task performed by the symmetrical theory of value is to provide just a first approximation model to the problem of price determination in real world competitive markets. Empirical criticisms should take into account this fact: to stress the existence of a mere quantitative difference between the conclusions of the theory embodied in a first approximation model and real world economies or to stress the present lack of empirical content in some of the theoretical categories of pure economics,

as was done by Clapham in his 1922 paper on 'empty boxes', is quite an ineffective criticism of a Marshallian model of supply and demand of given commodities.

An analytical strategy of successive approximation is forcefully advocated by Marshall in his *Principles of Economics:* given the complexity of real world phenomena–basically because of the action of the element of time–and given the limited analytical power of the economist, the economist must '[break] up a complex question, [study] one bit at a time, and at last [combine] his partial solutions into a more or less complete solution of the whole riddle' (Marshall 1961:366). This means that economists should:

i select a certain class of forces which they consider to be the most relevant and persistent in determining the phenomenon under scrutiny;
ii build up a first approximation model which takes into account just those forces, in order to reach some well-defined theoretical results; and finally
iii elaborate a series of (more general) models which progressively consider the other forces previously 'frozen' in the *cœteris paribus* clause.

Sraffa appears to be aware of this defensive strategy. In the folder D1/32 ('Semi-monopoly', dated pre-1928) there is a long note whose content is basically similar to the printed version of 'The laws of returns under competitive conditions', probably one of the preparatory notes of that article.[14] This note is of particular interest since it reveals what in printed version is kept in the background, that is, Sraffa's methodological reasons underlying his criticism of the Marshallian approach and of the theory of perfect competition. The former provides *prima facie* the basic elements for devising a fruitful first approximation model to tackle the problem of price determination in competitive markets:

> È un fatto che, nonostante l'estendersi del monopolio, resta un largo campo di industrie nelle quali vi è una molteplicità di produttori indipendenti. È un fatto che, col variare della quantità prodotta in ciascuna industria spesso varia il costo e con esso il prezzo. Questi fatti sono bene rappresentati dallo schema marshalliano della simmetria fondamentale che si esplica in due curve collettive di D[omanda] e O[fferta]. *La rispondenza fra le conclusioni della teoria e i fatti è così soddisfacente che si comprende facilmente che la teoria trovi così larghi consensi.*
>
> (*SP* D1/32, p. 21, 1; emphasis added)[15]

On the other hand, the theory of perfect competition appears to be 'un'ottima prima approssimazione allo studio dell'equilibrio economico, la migliore conosciuta' (*SP* D1/32:21, 6).[16] It is of course plainly acknowledged by the Marshallians, Sraffa adds, that some differences still remain between the conclusions of the theory and the facts the theory is designed to explain;

but these differences were largely foreseen, 'era nella natura stessa della teoria'[17] as a first approximation since the theory 'deve considerare solo le forze dominanti, le tendenze principali che determinano l'equilibrio, mette in evidenza le caratteristiche essenziali, insomma semplifica la realtà in modo da renderla dominabile dalla nostra mente: trascura poi le minori complicazioni, gli attriti che ritardano e modificano leggermente gli effetti delle forze maggiori senza mutarli sostanzialmente'.[18] What Sraffa is here claiming is that a first approximation model is, by its very nature, protected from being knocked down by a single failure or even by repeated failures in its empirical verification: the worth of a first approximation model is to be assessed in its heuristic content and not in its direct empirical content. Hence Sraffa, endorsing a different kind of criticism than Clapham, aims to show that:

1 the differences between the conclusions of the theory embodied in a Marshallian first approximation model and real world economies are not simply quantitative but rather qualitative, 'a difference not of degree but of kind'[19] (*SP* D1/32:21, 7); and that
2 the 'boxes' of pure Marshallian theory are empty because they are incapable of being filled with some relevant empirical content, '[l]e scatole sono vuote perché fanno acqua da tutte le parti' (*SP* D1/32:21, 2).[20]

Sraffa devotes almost all of 'Sulle relazioni fra costo e quantità prodotta' and the first part of 'The laws of returns under competitive conditions' to provide a thorough proof of the point *sub* 2. In what follows attention is focused on the point *sub* 1. The crucial element in Sraffa's reasoning is the distinction between a fruitful and a fallacious first approximation. In order to build a fruitful first approximation model to explain a given empirical phenomenon, a theorist must select a well-defined subset of forces and provide a formal analysis of their interaction. This amounts to considering those forces as *the* most relevant ones determining the *explanandum* phenomenon and to assuming that the other forces which exert a non-negligible influence on the latter will be taken into due account at a later stage of the analysis: there should be reasonable grounds to believe that the first approximation model may be generalised. Taking into account the very nature of a first approximation model, there are two ways of providing a compelling criticism of it:

a a logical criticism, aimed at detecting the logical inconsistencies, if any, between the assumptions and the conclusions of the formal apparatus of the model;
b a 'sophisticated' empirical criticism, aimed at showing that the model is not in keeping with its own basic empirical claims.

The latter is the option chosen by Sraffa. He argues that Marshall's theory does *not* consider 'the predominant forces, the main tendencies which

determine equilibrium' in real world competitive markets. Marshall's model is a *fallacious* first approximation model which does not consider the truly relevant forces and which can *not* be generalised. In Sraffa's manuscripts of the mid-1920s it is possible to find two passages which provide some clear hints about his criteria for theory appraisal. The first is taken from the folder D1/32, the second from the folder D1/40 ('Notebook of miscellaneous items including 'Produttività decrescente', dated pre 1928):

> La teoria deve considerare solo le forze dominanti, le tendenze principali che determinano l'equilibrio, mette in evidenza le caratteristiche essenziali, insomma semplifica la realtà in modo da renderla dominabile dalla nostra mente: trascura poi le minori complicazioni, gli attriti che ritardano e modificano leggermente gli effetti delle forze maggiori senza mutarli sostanzialmente. Risponde a questi requisiti la teoria che ci sta innanzi? No: le differenze fra essa e la realtà sono di carattere ben diverse. Poiché è proprio nel *modo* con cui quei risultati vengono raggiunti (e non solo nella *misura*) che la realtà e la teoria differiscono. Dato ciò, la teoria non solo diventa inutile perché non ci aiuta a vedere in forma semplificata lo svolgersi del processo reale, ma è positivamente dannosa e pericolosa perché ci mette su una via sbagliata nei nostri tentativi di spiegarci quello svolgimento. *Non bisogna però dimenticare che la sola coincidenza dei fatti osservati con le conclusioni della teoria non è di per sé sufficiente a stabilire che la teoria ha questi attributi essenziali È infatti possibile che, pur essendo quelle uguali, la route per la quale son raggiunte sia fondamentalmente diversa*. La storia delle scienze fisiche, e sopratt[utto] dell'astron[omia], può fornire molti esempi di sistemi, le cui conclusioni coincidevano con i fatti grossolanamente osservati, e che poi furon dimostrati fallaci. *È sotto questo rispetto che il metodo di M[arshall] di avvicinare il problema del valore in cond[izioni] di c[oncorrenza] sembra prestare il fianco alle maggiori obbiezioni*
>
> (*SP* D1/32:21, 1–2; second and third emphasis added)[21]

> *e per quanta si tratti solo di schemi teorici che devono servire non ad applicazioni pratiche,* neppure indirette, ma soprattutto ad allenare la mente alla considerazione dei complessi problemi della realtà; *pure le ipotesi su cui si fondano devono essere per quanta mena possibile arbitrarie*… . Se vi è coincidenza fra gli effetti teorici e quelli pratici ma una profonda divergenza fra le ipotesi o cause, la teoria non solo è inutile ma è pericolosa perché ci mette su una falsa strada nella ricerca delle spiegazioni dei fatti concreti…. *Una teoria che presenta queste fallaci apparenze è particolarmente pericolosa perché mette su una falsa strada chi inizia gli studi economici: una prima approssimazione può restare lontanissima dall'obiettivo; ma deve approssimarci nella giusta direzione*.
>
> (*SP* D1/40:20–1 and 30; emphasis added)[22]

Here, Sraffa appears to endorse a methodology based on the realism of the assumptions. In testing the explanatory power of a theory, the realism of the conclusions of the theory is not a sufficient condition for the theory to pass the test: the theory may be founded on arbitrary or unrealistic assumptions and still produce, by flukes or faulty reasonings, conclusions which appear *prima facie* consonant to the facts that the theory is designed to explain. The two passages from the manuscripts quoted above help to identify the reason why Sraffa in 'Sulle relazioni fra costo e quantità prodotta' singles out the (explicit and implicit) assumptions of a Marshallian competitive model and reconstructs it in a logically consistent way: Sraffa wants to verify whether the apparent agreement between the conclusions of Marshallian theory and the empirical fact that the theory was designed to explain is due to flukes or faulty reasoning or whether it is due to a consistent reasoning from realistic premises, that is, premises concerning the most relevant forces acting in real world economies.

4 Logical consistency vs empirical relevance?

Scientific theories can be appraised from a great variety of standpoints, the two most common being the logical consistency of the deductive steps from assumptions to conclusions and the empirical relevance of a subset of possible semantic interpretations of their deductive skeletons.[23] Sraffa's contributions to economic theory as well as 'Sraffian' economics have undergone both scrutinies. During the 1960s controversies on the theory of capital and distribution, much intellectual energy was devoted to the task of establishing or denying the logical possibility of such 'perverse' phenomena as reswitching of techniques and capital reversal. It was not until Samuelson in his 1966 'summing up' paper frankly admitted that these phenomena could not be denied on merely logically grounds, that the issue of their empirical relevance was raised.[24] In this respect two recent skirmishes between Mark Blaug and Ian Steedman are particularly interesting since they jointly appraise the two sides of the question.[25] Blaug, one of the staunchest supporters of 'Popper in economies', acknowledges that the 'neoricardian' criticism of neoclassical economics is unquestionable from a purely logical point of view. Nevertheless he points out that the principal task of any economic theory is not to be logically watertight, but to shed light on real world economies: from this point of view 'Sraffian' economics is, in his opinion, almost empty of any relevant empirical content. On the other hand, Steedman concedes to Blaug that 'Sraffian' economics is far from being the typical case of a Lakatosian empirically progressive research programme; yet he does not support Blaug's dismissive attitude towards the negative and positive contributions of 'Sraffian' economics since:

1 the latter is a budding (and heavily understaffed) research programme;
2 empirical progress in economics is hard to attain even by a wellestablished research programme; and

3 logical consistency is to be ranked very highly in appraising economic theories.

Note that to satisfy Blaug's requirements is far from easy. Even granted that 'Sraffian' economists are not particularly interested in the empirical applications of their pure theory, they cannot be blamed too severely, at least until it is shown how these empirical applications might be worked out satisfactorily. For instance, the possibility of identifying the empirical probability of actual reswitching and capital reversal is almost non-existent. Consider a system in which n goods are produced through m different techniques. In such a case it is necessary to consider mn different wage-profit curves, through it is only possible to observe one combination at a time. Moreover, there is no guarantee of observing a really cost-minimising system belonging to the wage-profit frontier (the possibility of joint products, which involves a further complication, is not considered here). Popperian (or Lakatosian, for that matter) critics of 'Sraffian' economics should therefore be aware that detecting actual reswitching is much harder than, say, finding actual examples of Giffen goods!

The debate on the relations between logical consistency and empirical relevance within 'Sraffian' economics is surely interesting as, inter alia, it sheds light on the problems deriving from the application of Popperian (or Lakatosian) methodology to the assessment of economic theories. Nonetheless, in the light of what has been shown in the previous section, it may be doubted whether Sraffa would have accepted the (implicit) assumption on which that debate seems to be based, that is, the admissibility of a tradeoff between logical consistency and empirical relevance within an economic theory. On the contrary, as soon as Sraffa reached the conclusion that such a trade-off is inevitably embodied in Marhallian economics, he started working to an entirely different theory. The two passages where Sraffa's thought in this respect emerges most clearly may be found in the final remarks of his replies to Robertson (1930) and Hayek (1932):

> I am trying to find what are the assumptions implicit in Marshall's theory; if Mr. Robertson regards them as extremely unreal, I sympathise with him. We seem to be agreed that the theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr. Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that, in the circumstances, I think it is Marshall's theory that should be discarded.
>
> (Sraffa 1930a:93)

> Nobody could believe that anything that logically follows from such fantastic assumptions is true in reality. But I admit the abstract possibility

> that conclusions deduced from them by faulty reasoning may, by a lucky accident, prove quite plausible.
>
> (Sraffa 1932b:250)

These two quotations show that Sraffa did not assume the existence of a trade-off between logical consistency and empirical relevance as an unavoidable feature of theoretical reasoning: the theorist should strive to set forth a theory able to explain all its quantities-*explananda* by means of correct deductive inference. Moreover, as already stressed in the previous section, it seems that Sraffa endorses a methodology based on the realism of the assumptions: whenever, starting from realistic premises, no logical mistakes are made, conclusions should be considered as empirically sound (provided that the set of premises is suited to frame a complete description of the problem); although it may happen that, starting from empirically false premises, seemingly plausible conclusions are reached through *defective* reasoning.[26] Finally, Sraffa considered the logical reconstruction of an economic theory only as a preliminary step in the assessment of its empirical domain: only when the whole set of its (explicit *and* implicit) assumptions is highlighted and the logical consistency of the whole construction is checked, may the empirical domain of an economic theory be precisely determined.

5 Piecemeal theorising

In our view, there is no evidence that Sraffa changed his mind on the relation between logical consistency and empirical relevance in the almost forty years which separate the publication of *Production of Commodities* from the 1925–6 articles. Had Sraffa not considered his 1960 theory as both internally consistent and empirically relevant (in some sense), he most probably would not have licensed it. Hence, the theory of value and distribution set forth in *Production of Commodities* should be regarded as a first approximation that, unlike Marshall's, Sraffa regarded as pointing in the right direction.[27]

Nonetheless, at first sight, it might appear that this first approximation dramatically reduces the empirical domain of pure economic theory (mainly because quantities of produced commodities are taken as exogenously given). Therefore, it might seem that the same inescapable trade-off between logical consistency and empirical relevance Sraffa had detected in Marshall's theory is also hidden in Sraffa's. In the light of what has been said above this would be highly ironical! Therefore, the first important question is whether such a trade-off actually exists in *Production of Commodities*. If Sraffa's theory of prices of production is compared, say, with general equilibrium theory according to a criterion of generality ultimately based on the number of endogenous variables 'explained' by the theory, the answer is almost unavoidably affirmative.[28] In order to avoid such a conclusion, other criteria or, rather, a

different research strategy which Sraffa may have considered as more suited to deal with the subject matter of economic science must be identified. This is *the* crucial point, as Dan Hausman rightly points out:

> Indeed one might question whether Sraffa's work has any interesting economic content at all. That many talented economists find in Sraffa's work the germ of an alternative approach to economics may seem puzzling. I shall now solve this puzzle by emphasizing the contrasts between the method or strategy of [neoclassical] equilibrium theorizing and the method or strategy implicit in Sraffa's work.
>
> (Hausman 1981:173)

Neoclassical economics (and general equilibrium theory in particular) can be reconstructed around a very few basic principles upon which the whole theoretical structure is supposed to be grounded. In a sense the domain of economics becomes implicitly defined by the possibility of applying its basic analytical devices (*in primis,* of course, rational and self-interested individual choices modelled as problems of constrained maximisation). Robbins' famous definition of economics as the discipline that studies human behaviour as a relationship between ends and scarce means that have alternative uses is a classic example of this approach. Such a definition, indeed, does not determine *a priori* what is the concrete subject matter of the discipline; but establishes a criterion for recognising as 'economic' a peculiar feature of every human action. No wonder, therefore, that economic theory is now applied to whatever happens to be minimally suited for the application of the familiar apparatus of technicalities.[29]

But this cannot be the starting point for an author who is consciously and explicitly attempting to rescue the point of view of the Classical tradition. In this tradition the empirical domain of the discipline is strictly defined in terms of the economic phenomena to be analysed, such as the causes of the level and growth of the wealth of nations and the consequences of growing wealth on the distribution of income among different social classes. In this connection an entirely different perspective on the proper research strategy is called for:

> Sraffa does not take economics to be a separate science with its own distinctive laws and causes. It is to be distinguished roughly from other social studies by its concern with production, distribution, exchange and consumption of commodities and services, and with social phenomena closely connected with these. Its laws are not necessarily individualistic or psychological, although some may be. In seeking to explain given economic phenomena one should draw freely upon the results of other social studies. No special set of causal factors is predominantly responsible for all major economic phenomena. In each given problem situation, the

> economist must isolate the major causal factors by empirical investigation and theoretical ingenuity…. Economists should seek many different (but mutually consistent) explanations at different levels of detail.
>
> (Hausmann 1981:183–4)

All this amounts to advocating an approach to theory-making usually labelled as 'piecemeal theorizing'. A failure to understand this methodological perspective is probably the very reason why mainstream economists usually perceive Sraffa's theory as unintelligible and devoid of any empirical interest. In our view, Hausman's assessment (that has been to some extent unduly neglected by 'Sraffian' economists themselves) should be carefully considered for at least two reasons:

1 it provides an interesting rebuttal of the idea that the Sraffian approach must be methodologically flawed;[30] and
2 it may give the clue for understanding the common origin of a number of methodological propositions repeatedly advocated by scholars who at various times were closely associated with Sraffa in study and research.

Indeed, what these authors seem to have in common[31] it is the idea that the Sraffian approach (and their own particular interpretation and/or farther development of it, for that matter) is in a sense more 'general' than is usually perceived, being suitable for further theoretical achievements to be obtained by 'adding' to it other pieces of theory, possibly at a different level of abstraction.

6 Conclusions

All the previous sections, in a sense, deal with one main question: what, from a *methodological* point of view, may be regarded as similar and what different when Sraffa's criticism of the Marshallian theory of value in the 1920s is compared with his 1960 reconstruction of Classical economics? To the first part of the question it seems possible to answer in quite a simple and straightforward way: during his whole intellectual career Sraffa remained convinced of the necessity of logically sound theoretical arguments. He was always inflexible in requiring logical consistency, though not only because of its necessity for assuring the material truth of conclusions derived from true premises. His critical appraisal of Marshall's theory of competition clearly shows that he was also concerned with two more subtle possibilities, that is:

1 the possibility of reaching, through faulty reasoning, seemingly true conclusions even from doubtful (to say the least) or incomplete initial assumptions; and

2 the possibility of placing excessive confidence on the explanatory power of theories with a quite restricted empirical domain (hidden in implicit assumptions, which, once made explicit, drastically reduce the ground for possible applications of the theory).

The second part of the question, concerning the possible elements of discontinuity, requires a more elaborate answer. In particular, it is necessary to carry out a comparative assessment of the methodological attitude Sraffa seems to have adopted in 'Sulle relazioni fra costo e quantità prodotta' as well as in 'The laws of returns under competitive conditions' with his advocacy of 'piecemeal theorizing' which in our interpretation characterises *Production of Commodities*. The first two works, while highly critical of the marginalist theory of value both in its Marshallian and Paretian version, do not seem to openly challenge, per se, the marginalist view on theory-making in economics. The same cannot be maintained for *Production of Commodities*.

As already pointed out, Sraffa's 1925 conclusions are that, setting aside the empirically irrelevant cases of specific factor industries and of specific external economies industries, only constant costs industries fall within the empirical domain of a logically consistent analysis of perfect competition in a partial equilibrium model. This entails that the Marshallian 'fundamental symmetry' between supply and demand breaks down *and* that the treatment of more general cases requires a general equilibrium approach. Such findings may be summarised by saying that, if we want to cover all the conceivable theoretical cases, we should abandon the hope of simplifying the problem by means of the analytical device of partial equilibrium analysis. This particular analytical method, one might thus argue, ends up by assuming the role of the main target of criticism.

In the same vein, Sraffa's dismissive 1926 judgement about the empirical content of a general equilibrium model, 'a well known conception, whose complexity, however, prevents it from bearing fruit' (Sraffa 1926a:541), together with his proposal 'to abandon the path of free competition and turn in the opposite direction, namely, towards monopoly' considered as 'a well-defined theory in which variations of cost connected with changes in the dimensions of the individual undertaking play an important part' (Sraffa 1926a:542), do not seem to point to a dramatically different epistemological foundation for pure economic theory. A decreasing demand curve facing the single firm may be indeed regarded as a move towards a more realistic hypothesis (and, correspondingly, as a departure from the more strictly shaped case of pure competition). In this case, the target of criticism is not so much partial equilibrium analysis as the soundness of the notion of perfect competition. In both cases, Sraffa's criticism of Marshallian economics and his trenchant condemnation of Paretian economics do not go as far as to deny that a fully-fledged theory of value should be based on the notions of individual maximising rationality and of subjectivistic equilibrium. In

particular, Sraffa i) seems to accept that the theory of value should take into account the problem-situation facing the individual firm and ii) does not deny the importance of a theory of demand (even if a different justification of its decreasing shape is hinted at, with the scope of expunging any subjective element from the analysis).

By the time of *Production of Commodities* the notions of individual maximising rationality and of subjectivistic equilibrium as well as a theory of the firm and a theory of demand have disappeared from the picture. Meanwhile a theory of prices has emerged, which requires, according to our interpretation in terms of 'piecemeal theorizing', to be completed through the addition of other pieces not necessarily made from the same theoretical bricks. The view of theory-making in economics which emerges from *Production of Commodities* appears thus quite different from that which we can see in the works of the young Sraffa. Sraffa probably thought that the solution he offered in his 1960 book had the advantage of escaping the dilemma(s) encountered following Marshall's route (or Pareto's, for that matter). Only future research, however, will tell us if his belief was well-founded. All depends on the possibility of successfully developing a comprehensive economic theory following the path mapped out by Sraffa.

Notes

1 We wish to thank without implicating Neri Salvadori for his comments on a previous version of this chapter and Pierangelo Garegnani, Sraffa's literary executor, for his kind permission to quote from Sraffa's unpublished manuscripts. Grants from the University of Bergamo, the University of Pisa and MURST are also gratefully acknowledged. Translations from Italian parts of Sraffa's manuscripts are ours.

2 See Becattini (1986), Davis (1993 and 1998b) and Signorino (1998).

3 See, for instance, Backhouse (1995), Blaug (1975 and 1995), Davis (1993 and 1998b), Kurz and Salvadori (1997), Lunghini (1975), Napoleoni (1989) Roncaglia (1978, Ch. 6), Salanti (1997), Steedman (1991 and 1995) and Zamagni (1991).

4 The first references that come to the mind in this respect are Bharadwaj (1978), Garegnani (1987 and 1990a), Roncaglia (1978 and 1990a) and Pasinetti (1994).

5 One of the present authors (R.S.) gratefully thanks Mr Jonathan Smith, Chief librarian, together with the whole staff of Wren Library for their assistance during his summer 1998 research period in Cambridge.

6 'External-internal' economies are those economies to scale which are simultaneously external to the individual firms and internal to a given industry. 'External-external' economies are those which are jointly enjoyed by a plurality of industries.

7 In Mäki (1994:151) the two cases are respectively called 'nullifying idealisations' and 'stabilizing idealisations'.

8 Following the distinction between 'core' and 'peripheral' assumptions as set forth in Mäki (1998), what follows is meant to apply only to the latter. About the former something will be said in the final section. Economists usually refer to this distinction with slightly different terminologies, such as fundamental postulates/assumptions of specific conditions (Machlup 1955), generative/auxiliary assumptions (Melitz 1965), axioms/assumptions (Hahn 1985).

9 To be precise, we should distinguish between 'negligibility' and 'joint negligibility'. As Mäki (1998:6) puts it: 'While [Musgrave's] negligibility assumptions state that factor F is singly negligible in an explanation, joint negligibility assumptions state that factors F_l, F_k are jointly negligible…it is important to understand that the logic of negligibility assumptions is such that a conjunction of true singular negligibility assumptions is not necessarily true. The separate effects that are negligible singly may add up so as to generate a non-negligible joint effect. Secondly, it may be assumed that the actual separate effects of single factors…–whether or not singly negligible–cancel out each other to the extent that their actual joint effect is negligible relative to a given purpose.'

10 In this sense there would not be any problem whatsoever with "as if" arguments if they were intended as [HA]s. Fierce controversies can originate just because this is not the case.

11 Note that Musgrave's original scope was to show how the very same assumptions within a given theory could be (synchronously or diachronously) interpreted in different manners. One possibility is that '…criticism may change the status of an assumption: what in youth was a bold and adventurous negligibility assumption, may be reduced in middle-age to a sedate domain assumption, and decline in old-age into a mere heuristic assumption. Such changes can be almost imperceptible if the same form of words is employed for all three "assumptions".' (Musgrave 1981:385).

12 In 'The laws of returns under competitive conditions' we may find a clearer statement of his thought about the issue of first and second approximations. Sraffa argues first that the hypothesis of constant costs can be considered a good first approximation in the analysis of perfect competition. Then he goes on to explore the possibility of further approximations, that is, the possibility of treating the cases of non-specific factors and of non-specific external economies within a model of partial equilibria: 'When we proceed to a further approximation, while keeping to the path of free competition, the complications do not arise gradually, as would be convenient; they present themselves simultaneously as a whole. If diminishing returns arising from a "constant factor" are taken into consideration, it becomes necessary to extend the field of investigation so as to examine the conditions of simultaneous equilibrium in numerous industries…. If we pass to external economies, we find ourselves confronted by the same obstacle, and there is also the impossibility of confining within static conditions the circumstances from which they originate' (Sraffa 1926a:541).

13 'In 1848 J.S.Mill wrote that "there is nothing in the laws of value which remains to clear up". This judgement must have been widely shared, since there followed twenty years of little theoretical development and disinterest in the re-examination of basic principles. Nowadays a similar mood seems dominant among economists, as if there were nothing essential to add or modify within economic theory.

S[ee] f[or] in[stance] what has been written by Pantaleoni (obit[uary] of Pareto in [The] E[conomic] J[ournal]) and Keynes (Intr[oduction] to Cambridge Ec[onomic] Hand[books]). Such an impression aligns with a new relaxation in theoretical study. As Keynes puts it: "important improvements in its elements are becoming rare". And concludes that the task of the "professional economist" now consists either in trying to know the relevant facts and applying economic principles to them, or in spreading the knowledge of the proper method of economic study. Twenty years passed before Mill's statement was challenged by Jevons, despite the fact that criticism was facilitated by the compact set of clear and unanimously maintained laws which framed economic science at that time. Nowadays "the Theory of Economics does not furnish a body of settled

conclusions immediately applicable to policy. It is a method rather than a doctrine…", hence this time we will probably have to wait for a Jevons for more than twenty years! Nevertheless, the present doctrine is far from being fully satisfactory.'

14 This conjecture is strengthened by a passage on the back of page 23, 22 in which Sraffa explicitly rejects the explanation of the empirical deficit of the Marshallian 'boxes' on returns proposed in 1922 by Pigou 'su questo Giornale' (in this Journal).

15 'It is a fact that, notwithstanding the widening of monopoly, there are still many industries with a great number of independent producers. It is a fact that with a variation in the quantity produced in each industry the cost and therefore the price often change. These facts are well represented by the Marshallian model of the fundamental symmetry emerging from the collective D[emand] and S[upply] curves. *The similarity between the conclusions of the theory and the facts is so close that it is easy to understand the reason why the theory is so widely accepted*'.

16 'a very good first approximation to the study of economic equilibrium, the best known.'

17 'it was in the very nature of the theory.'

18 'must consider only the predominant forces, the main tendencies which determine equilibrium, it highlights the basic features, in short it simplifies reality in order to make it manageable to our minds: it neglects the minor complications, the frictions which slow down and slightly modify the effects of the major forces without substantially changing them.' The opening sentence of Sraffa's 1928–31 *Lectures on the Advanced Theory of Value* reads as follows: 'The general theory of value being intended to take into account the common characteristics of the most diverse conditions under which values of different commodities are determined, it is necessarily very abstract in character. It moves from a relatively small number of assumptions and deduces from them the way through which an equilibrium is reached' (*SP* D2/4:1).

19 This sentence was written by Sraffa himself in English.

20 '[t]he boxes are empty because they leak from everywhere.'

21 'Theory must consider only the predominant forces, the main tendencies which determine the equilibrium, it highlights the basic features, in short it simplifies reality in order to allow our minds to control it: it neglects the minor complications, the frictions which slow down and slightly modify the effects of the major forces without substantially changing them. Does the present theory meet these requisites? No: the differences between reality and [the theory] are of a very different nature. Since it is in the very logical procedure through which those results are achieved (and not simply in the measure [of the results]) that reality and theory part company. This granted, the theory is not only useless in that it does not help us to see in a simplified form the course of the real process, but it is positively harmful and dangerous since it leads our efforts to explain that course on a wrong direction. *We must not forget that the mere coincidence of the observed facts with the conclusions of the theory is not in itself enough to establish that the theory is in accordance with the above basic characteristics. It is, in fact, possible that, though the conclusions are similar [to the facts], the logical procedures through which [the conclusions] are arrived at are basically different.* The history of physical sciences, in particular] of astron[omy], provides many examples of [theoretical] systems whose conclusions agreed with the facts roughly observed but which were later shown to be fallacious. *It is in this respect that M[arshall]'s method of approaching the problem of value under competitive] c[onditions] appears to be open to the most relevant objections.*'

The same argument is restated almost *verbatim* in a note kept, page 4, in the folder D1/45 ('Probable form of demand curves and efforts', dated pre-1928).

22 '*though they are only theoretical models which are designed not for their practical applications,* not even indirect, but mainly to train our minds to consider the complex problems of reality, *nevertheless the assumptions on which they are based must be, as far as it is possible, the least arbitrary*…. If there is coincidence between the theoretical results and the facts but a deep divergence between the assumptions or causes, the theory is not only useless but also dangerous since it deceives us in the search for an explanation of the concrete facts…. *A theory characterised by these fallacious appearances is particularly dangerous since it leads those who begin economic studies in the wrong direction: a first approximation may remain very far from the target; but it must take us in the right direction.*'

23 While it is relatively easy to specify what logical consistency is supposed to mean, empirical relevance is a somewhat vague and obscure concept. Sometimes it is taken to imply the material truth of the premises (in which case logical consistency would assure the material truth of the conclusions). This commonsensical and simplistic notion, apart from the puzzle of 'as if arguments, completely disregards the fact that any conceivable set of premises unavoidably constitutes a partial (i.e. incomplete, abstract, idealised, approximate, inexact, and so on) description of the empirical reality.

24 In addition to the classical work by Harcourt (1972), updated summaries of the analytical and methodological issues involved in such controversies may now be found in Kurz and Salvadori (1995, Ch. 14 and 1997).

25 See Blaug (1991 and 1995) and Steedman (1991 and 1995).

26 Of course it might also happen that seemingly plausible conclusions are reached through correct reasoning starting from false (but unobservable or untestable) premises. This could explain why Sraffa was always highly suspicious of premises about unobservable entities.

27 Similarly, Sraffa explicitly considered the critique of the marginalist theory which can be derived from *Production of Commodities* as a prelude to a fully-fledged criticism.

28 Even without going so far as embracing Frank Hahn's (1982) radical view of Sraffa's model as a 'particular case' of general equilibrium model, we cannot deny the existence of the problem. Remind, for instance, that Joan Robinson (1961) reviewed it as a half equilibrium system.

29 For a proud and optimistic view of this wider application, see Hirshleifer (1985).

30 As Hausman (1981:184) puts it: 'Is there anything wrong with piecemeal theorizing? Are there any methodological grounds upon which to dismiss Sraffa's work? I think not. I can see no justification for the requirement that explanation in economics should be unitary.'

31 See note 4 for more precise references.

11 Sraffa's edition of Ricardo's *Works*

Reconstruction of a reconstruction

Annalisa Rosselli

Foreword

Between Ricardo and Sraffa there is a two-way relationship: Sraffa places Ricardo among the sources of *Production of Commodities by Means of Commodities,* asserting the classical, Ricardian inspiration behind the entire work, and behind certain propositions in particular, while our present knowledge of Ricardo depends crucially on Sraffa's critical edition of his works. In this chapter we shall be looking at one side of the relationship. I shall not be dealing with Ricardo's influence on Sraffa and the analogy between the problems they both tackled, but with certain aspects of Sraffa's interpretation of Ricardo. The question I address is not the nature of Sraffa's debt to Ricardo, but rather what Ricardo owes to Sraffa.

The history of the edition

Work on the edition of David Ricardo's complete works officially began for Piero Sraffa on the last day of February in 1930, when Keynes persuaded the Royal Economic Society, for which he was acting as secretary, to entrust Sraffa with the task of editing the publication. Behind this decision of Keynes there were two intentions: to speed up work on the publication, which had been entrusted to T.Gregory years before and seemed to be making little headway, and at the same time to find some other task for Sraffa at Cambridge that he might find more congenial than the work of teaching, which he had already declared his intention of giving up.

On the first point Keynes' choice was not to prove so happy–it was in fact to take twenty-four years to get the work published–but on the second he seems to have had resounding success in arousing Sraffa's enthusiasm. 'Sraffa is now tackling the Ricardo job like a maniac', Keynes noted with satisfaction on 21 March 1930.[1] Within a few weeks, and thanks also to the collaboration of Richard Kahn and Maurice Dobb, comparison between the three editions of the *Principles* was completed;[2] Sraffa made his first visit to Gatcombe, where Ricardo's library could still be consulted; Keynes' letters informing Ricardo's descendants and the expert Ricardians of the Anglo-Saxon world

of the fact that Sraffa had officially been entrusted with the work were written and sent–the first of a long series of letters that were to continue throughout the preparation of the work, and with which Keynes succeeded in obtaining help and collaboration for the relatively obscure foreign scholar. 'Though an Italian citizen, [Mr. Sraffa] is specially qualified for the work by his knowledge and interest in Ricardian subjects' was more or less the recurrent formula.[3]

Thus work began under every good omen, and went ahead in an atmosphere of general collaboration throughout 1930 and 1931. T.Gregory showed no surprise when the task was taken from his hands, offering advice and confiding to Keynes: 'I am sure Mr. Sraffa will love the Principles' (Letter from Gregory to Keynes, 19 March 1930, *SP* D3/11/62.38). Keynes sent Sraffa to J.Bonar for 'blessing, advice and help' (letter from Keynes to Bonar, 16 March 1930, *SP* D3/11/57.3) and assistance was duly guaranteed. Foxwell generously loaned the first and second edition of the *High Price of Bullion* for comparison (*SP* D3/11/61.26) (and got them back several years later). E.Cannan offered suggestions, taken up and acknowledged, on the way to publish the first chapter of the *Principles* allowing for comparison between the various editions. Moreover, the hunt for unpublished material, also pursued with advertisements in the British press,[4] was yielding excellent results: in particular, Frank Ricardo, David's great grandson, came upon what were to be known as the Ricardo Papers, including letters from Malthus and James Mill to Ricardo, while Ricardo's notes on Bentham were unearthed in Geneva.

From the very outset a number of key decisions taken on the editing show a Sraffa immediately convinced about certain criteria. All his choices were inspired by the principle of limiting the editor's intrusion to the minimum and producing as functional as possible a work for the purposes of consultation. Sraffa rejected Gregory's advice to divide up the writings according to subject (those on 'corn' being separated from those on 'currency'), preferring division by typology (essays, pamphlets, letters, manuscripts).[5] He ruled out the idea of compiling a bibliography of the works on Ricardo in the conviction that 'with little more effort one could prepare a complete economic bibliography of the period' (letter from Sraffa to Viner, 31 January 1932, in *SP* D3/11/74.5), choosing to confine himself to a complete list of the editions of all Ricardo's writings up to a certain date, including the translations. He decided to publish the letters in chronological order together with those of the correspondents prompting them or which they answered:[6] 'The reader is thus placed as it were behind the Ricardo's desk at Gatcombe Park and reads the letters as Ricardo writes them or receives them' (Ricardo 1951–73, vol. VI, p. xiv[7]), as we read in the general introduction to the volumes of correspondence (and the principle is followed so scrupulously that even postal delays are taken into account in the order of publication).

So it was that a rapid conclusion to the work seemed in sight. In fact, Sraffa wrote to Antonio Gramsci's sister-in-law Tania on 2 October 1931 saying that he hoped the entire work 'will come out in a year and a half or two' (Sraffa 1991:36; my translation). In 1933 Keynes announced publication 'in the course of the year' (Keynes 1933:96). In the same year the Royal Economic Society deliberated an advance on royalties of £350.[8]

Then, as we know, work slowed down, but not, it seems, due to any loss of interest on the part of Sraffa. Examination of Sraffa's papers shows two main reasons for the long-delayed conclusion. In the first place the diminishing returns on the search for manuscripts. After the enthusiasm of the first finds, Ricardo's letters to Mill were still missing, which Sraffa rightly imagined the most interesting–together with the letters to Malthus–for reconstruction of the development of Ricardo's economic thought. Sraffa, who 'has long maintained that important manuscripts are never destroyed in this country and can always be discovered if you hunt long enough' as Keynes wrote,[9] could not resign himself to it. Untiringly, he wrote scores of letters to all the descendants of James Mill, now scattered over the four corners of the world and by no means easy to track down, as also to the descendants of Miss Taylor, executor of John Stuart Mill's will, and finally, in desperation, to his academic friends, urging them to rake through the manuscripts in their respective libraries. On top of this came the lamentable boycott of Jacob Hollander. Having expressed his opposition to the whole enterprise as doomed to commercial failure,[10] Hollander hastened to publish a previous pack of manuscripts found by Frank Ricardo, which he had been entrusted with as long ago as 1919, but the existence of which he had never mentioned to Sraffa, who learnt of it by indirect channels. From then on Sraffa set himself the task of finding out what material was in Hollander's possession, while Hollander set out to prevent Sraffa from consulting it. For example, Hollander made no mention of the letter of Ricardo to Barton (*RW* VII:155–9) which he had obtained on the antique market in 1916, and which was of crucial importance for reconstruction of Ricardo's position on unemployment and the introduction of machinery. After years of conflict, Sraffa vented his indignation in extremely harsh criticism of Hollander as a clumsy editor (*RW* II:xviii) and appallingly behaved human being: 'I have myself, together with Keynes, been struggling with this brute for the last six years; and we have succeeded (in some cases almost threatening legal action) in compelling him, if not to lend, at least to publish nearly all the Ricardo MS that I had traced to him' (letter from Sraffa to Laski, 19 August 1937, *SP* D3/11/66.32).

The second reason for the slowing down of the work was the standards of quality Sraffa had set himself. Kaldor explained (1985:630–1) that 'in Sraffa's own view the only comparable foreign work is the German edition of the *Collected Works* of Friederich list, published in the 1920s, after more than twenty years' work at a large committee of scholars'. For his part, Sraffa

enjoyed only occasional help, which he used mainly for the simpler tasks of proof-reading and transcription of manuscripts, keeping to himself all the work of combing through archives and examining texts. For example, he spent a great deal of time trying to work out the original order of the first edition of the *Principles,* convinced that the division into chapters and the order in which they were published had been decided by Ricardo at the last moment, with the text already in the first proof, thus representing Ricardo's further organisation of his own ideas. To provide grounds for this conjecture which appears in the Introduction to the *Principles* (*RW* I:xxvi-xxx),[11] Sraffa checked throughout the whole volume on: the number of lines on the first page of each chapter; the frequency of printing errors, to single out the pages composed by the same printer; the distance between the letters in the word 'Chapter' at each chapter heading and the length of the words 'Adam Smith' with the same purpose in mind; the use of the pronouns 'I' and 'we'; all references to other sections or chapters with the words 'former' and 'following' to see if the reference is in fact in the previous or following chapter, and the chapters indicated in all the footnotes where some oversight might reveal a previous order.[12] One may well understand how twenty-four years went by. The fond but somewhat caricatured picture of Sraffa hesitating for hours over the addition of a comma, as passed down by Cambridge oral tradition (see, for example, Austin Robinson's account in Pollit 1990) may lead one to overlook the huge amount of work that went into research for this edition of Ricardo's works. Among Sraffa's papers, the notes and, above all, the correspondence related to the search for manuscripts fill 240 files.

In 1933 Sraffa seemed to realise that the work would take longer than anticipated at the outset. Joan Robinson wrote to Richard Kahn on 13 March 1933: 'I called on Piero just now and found him in a state of despair. He says that Ricardo will take another 20 years.'[13] In 1938 the Royal Economic Society began to show some impatience, and above all Keynes was now getting worried. He wrote to Sraffa asking him to set a deadline, while suggesting that what was really standing in the way of completion of the work might be the drafting of the introductions,[14] adding:

> I feel that if someone were to prepare a draft on the basis of your material, however badly it were done, it would be extremely helpful. For there is nothing you would enjoy more than correcting the mistakes of fact, taste, relevance and lucidity. In fact, the worse the preliminary draft, the better.

The suggestion was to be taken up ten years later, with the intervention of Maurice Dobb. For the time being Keynes obtained further extensions for Sraffa from the Royal Economic Society–to December 1939, to December 1941–without any great difficulty since, as Austin Robinson recalls (Robinson, 1990:166), 'there was no question how the society ran. Keynes

ran it, and reported what he had done and what he proposed to do. The meetings served to validate his actions.'

Sraffa assuaged Keynes' impatience by sending him the editorial notes to the *Essay on profit* (August 1939) and the *Bullion Essays* (March 1940), as well as the appendix on the identification of the anonymous Continental merchant who testified to the Bullion Committee (*RW* III:427–34), arousing Keynes' enthusiasm to the point that he proclaimed it 'a jewel'.[15] In July 1942, in a letter to Viner, among other things seeking safekeeping across the Atlantic for the only copy of the corrected proofs of the Correspondence, Sraffa summed up the present state of progress: Six volumes already paged up in proof (*Principles, Pamphlets and Miscellaneous papers, Notes on Malthus,* three volumes of correspondence), *Speeches and Evidences* in galley proof, yet to be compiled–although much material had already been collected–the two volumes of bibliographical notes and index.[16] By March 1943 little had changed: still lacking were the introductions 'in Roman numerals' to the volumes of the *Principles, Notes on Malthus* and Correspondence. It was at this point that Keynes announced to Sraffa his decision to have the material printed in its present state. Keynes was worried not only about the fate of the publishing venture he had launched–Cambridge University Press was insisting that it was impossible to keep 13 tons of lead and 11 tons of paper unused when everything was going into the war effort–but was now also convinced that Sraffa had fallen prey to a sort of psychological cramp: 'We must make an end of it somehow, at least as much for your sake as for the book' (letter from Keynes to Sraffa, 26 March 1943, *SP* D3/11/65.27). We cannot tell how well-founded Keynes' conviction was,[17] or whether dictated by a certain failure to understand the standards of precision Sraffa had set himself. Keynes could hardly conceal his impatience: 'it is not possible to allow any further time for checking the cross-references. They have already been checked almost to death, and it is the life of the thing and not its death in which I am interested' (letter from Keynes to Sraffa, 2 April 1943, *SP* D3/11/65.18). However, Sraffa admitted that the introductions were in fact the real obstacle to conclusion of the work[18] despite all the labour lavished on it Unfortunately, scarcely any sign of this labour remains in Sraffa's papers. We know from a letter of Sraffa's old friend Mattioli that Sraffa himself, after volume 10 came out, destroyed most of the papers for the preparatory work.[19]

Three months later, in July 1943, the Mill-Ricardo Papers, including important manuscripts and letters from Ricardo to Mill were found, offering Sraffa every good reason for further delay since he had to reorganise all the material and add two more volumes to the work as originally conceived. It is worth noting, in the light of recent discussion, that Sraffa's original idea had probably been to publish also Ricardo's notes to the 'Measure of value' by Malthus, which had come to light among the Mill-Ricardo Papers and which were eventually left out of publication. In a note in Dobb's handwriting with an index for volumes 3 and 4, Ricardo's notes to the book by Malthus are included as part of volume 4.[20]

The war ended, activity resumed but the introductions were still unready for printing, which prompted Austin Robinson–who had succeeded Keynes as secretary of the Royal Economic Society and had always been convinced that Sraffa needed the assistance of someone 'between a servant and a master'[21]–to press Maurice Dobb into collaboration with Sraffa.

The collaboration between Sraffa and Dobb lasted from February 1948 to June 1954 and it has been reconstructed by Pollitt (1990) on the evidence of Dobb's papers. According to Pollitt, Dobb's role was to note down Sraffa's remarks emerging from discussion of the texts, edit them, write them up and hand them back to Sraffa, who would then consider whether they truly reflected his thoughts. So it was that–fruit of Dobb's pen but Sraffa's mind, as Pollitt puts it–the introductions to volumes 2, 5 and 6 were written, and most importantly the Introduction of introductions, namely that to the *Principles,* which kept them busy with few interruptions from the autumn of 1948 until the end of 1950, after much 'drafting and redrafting' (Pollitt 1990:524). Of this 'drafting and redrafting' hardly any traces remain in Sraffa's papers, except for a scheme by Dobb[22] which sheds some light on an aspect of Sraffa's hesitations. Probably drawn up when collaboration began, the scheme seems to summarise Dobb's ideas on the subjects to address in the Introduction with a fairly traditional approach. Themes suggested are: the problems of social conflict in Ricardo's times, the reception accorded to the *Principles* and interpretations by McCulloch, J.S.Mill, Marx and Marshall. None of these proposals were taken up in the final version, and we may reasonably suppose that Sraffa rejected them as too intrusive for the role of editor, clashing with the style respected in the rest of the work. In all nine volumes of the writings of Ricardo, excluding the Introduction to the *Principles,* interventions by the editor other than historical annotations or references to other parts of the work probably number four or five in all.[23] As the final result shows, reconstruction was to prevail over evaluation in the Introduction, but this meant a far harder task.

At last, in the spring of 1951 the *Principles* came out together with the three successive volumes.[24] Dobb wrote triumphantly to Sraffa: 'it is at last the Sraffa's Ricardo that will make people sit up and start talking of it as an event' (letter from Dobb to Sraffa, 8 March 1951, *SP* D3/11/59.12). Dobb had rightly anticipated a glowing reception for the *Works and Correspondence,* unanimously hailed as a 'truly monumental work of scholarship', as Lionel Robbins wrote in his review of August 1951 in the *Spectator*. However, the question one cannot help asking at this point is: was it Sraffa's scholarship or interpretation that aroused such enthusiasm and admiration?

Sraffa's 'unfailing neutrality'

A point worth noting is that the Introduction to the *Principles,* later considered an interpretation of Ricardo greatly influenced by Sraffa's own

particular interests to the point of misinterpreting Ricardo (Peach 1998:612), did not seem so to the earlier reviewers, who almost unanimously concentrated on the two points the new material Sraffa had found cast new light on: the role Mill played in the development of Ricardo's thinking and Sraffa's confutation of Ricardo's supposedly waning interest in the theory of labour-value.

There can be no doubting that those closest to Sraffa were well aware of his intentions to rehabilitate Classical theory as opposed to the economic theory then prevailing. Reviewing the first volumes published, Raffaele Mattioli wrote:

> We may therefore well wonder what services to economic science might be paid by the Ricardian theory of value reconsidered in its original, objective methodology, over and above any ideological problems. Sraffa's masterly 'Introduction' encourages us in our wish that he himself might be prepared to set about satisfying this need.[25]

And–surely reflecting his own interests, and not necessarily Sraffa's–Dobb could say that Sraffa had demonstrated that '[Ricardo] reached at the end of his life a position rather close to that of Marx, so that the true line of descent is certainly from Ricardo to Marx, and not from Ricardo to cost-of-production theory as in Mill to Marshall as the bourgeois tradition has it.'[26]

However, the other reviewers do not seem to have noticed in the Introduction the significance of the break with Ricardo's interpretative tradition: indeed, they were struck by Sraffa's 'unfailing neutrality' (Stigler 1953:587), to the extent of expressing the hope, as did Viner, that in the last volume 'Sraffa will not refrain from acting as arbitrator between Ricardo and his early and late critics.'[27]

It was above all after the publication of *Production of Commodities by Means of Commodities* that the Introduction began to be viewed in a new light as part of a more general research programme. As we well know, in the Preface to the book, asserting that he would be dealing 'exclusively with such properties of an economic system as do not depend on changes in the scale of production or in the proportions of "factors"', Sraffa announced that he had returned to the 'standpoint, which is that of the old classical economists from Adam Smith to Ricardo,…submerged and forgotten since the advent of the "marginal" method' (Sraffa 1960a:v). Thus Sraffa returned to the presentation of Classical theory as antagonist to the Marshallian 'fundamental symmetry' already much on his mind in the articles of 1925 and 1926.[28]

In fact, the project of reconstruction of the Classical standpoint had begun many years before. The Sraffa who set to work on his edition of Ricardo not only already had a thorough knowledge of and sensitivity to the nuances of Ricardo's text (demonstrated, for example by his observations in the 1925

article on Ricardo's decision to deal with extensive rent in the *Principles,* paying little attention to intensive rent (Sraffa 1925a [1998:335]), but also had clear ideas about the difference between Classical and marginalist theory in methods, aims and structure. Sraffa already saw something of an abyss between the Classical approach and that of contemporary theory–differences lying not only in the determination of value in the two theories (independence or dependence between value and quantity), but also in the very meaning of the verb 'to determine'. In some notes on the concept of the cause and measure of value prior to 1930 (*SP* D1/22), Sraffa noted that the formal point of view prevailed in the determination of value for modern economists: the problem was to specify the conditions indicating the equation of equilibrium and seek a value for the price constituting the unknown. The 'causes' of value are the conditions that allow us to ascertain or, more precisely, calculate the value. On the other hand, the Classical economists saw the causes of value in the 'genetic sense'. Looking to the problem from the point of view of reformers, inspired as ever by their concern for economic policy, the Classical economists were interested in ascertaining the causes of value in the sense of identifying those factors it was necessary to pay or not to tax in order to prevent the sources of production from drying up (to use Sraffa's metaphor). Thus the Classical theory of value has 'direct bearing upon practical politics' (Sraffa 1926a:535): excluding an item from the cost of production–like rent–means that it can be taxed without affecting production, but the same cannot be applied to labour, a 'cause' of value.[29] In any case, we are dealing here with material causes, quantifiable and objective, in sharp contrast with the subjective 'human elements' of disutility, abstinence and sacrifice that crop up in marginalist theory.

Here many problems remain to be settled.

a How did Sraffa become convinced of the contrast between Classical and Marshallian theory, and above all how much was this affected by his reading of Marx?
b How did Sraffa's interpretation of Classical theory develop–if indeed it did–through his work on the edition of Ricardo and the drafting of *Production of Commodities by Means of Commodities?*
c What are the themes in the Introduction that Sraffa used, without flagging in his 'unfailing neutrality' and without so much as a word against marginalism, to retrieve the point of view of Classical theory from oblivion? Here, in fact, the literature offers contrasting positions, both among Sraffa's friends and foes, on what the key points are in Sraffa's interpretation of Ricardo.[30]

Clearly, these are questions that still call for a great deal of research and analysis, going far beyond the scope of this chapter. Here I shall confine myself to two aspects of Sraffa's interpretation of Ricardo: one that has

attracted all too much attention, and one that may have attracted too little. Both are based on two celebrated sentences in Ricardo's correspondence.

The 'profits of the farmer' principle

Much of the most recent controversy on the interpretation of Sraffa has concentrated on the celebrated explanation he gives of a passage in a letter of 8 March 1814 where Ricardo illustrates to Trower the contrast between his and Malthus' position on the decline in the rate of profit with the accumulation of capital, asserting his conviction that 'it is the profits of the farmer which regulate the profits of all other trades' (*RW* VI:104).

Sraffa's interpretation is well known: in his attempt to determine how variations in the conditions of the production of agricultural goods, which constitute the majority of wage goods, affect the rate of profit, Ricardo is taken to have been reasoning as if the agricultural sector showed homogeneity between input and output, so that both the product and the capital needed for production could be compared in physical terms and profit determined without recourse to any theory of value. Thanks to the principle of uniformity in the rate of profit, the variations in prices in the other sectors would adjust to the rate of profit emerging in the agricultural sector, which would thus take on the role of 'regulator' or guide, as mentioned in the letter cited above.

Sraffa specifies that in none of the writings that have come down to us is this hypothesis explicitly formulated (*RW* I:xxxi), although he suggests that it might have been in the notes 'on the profits of Capital' that Ricardo is known to have written in early 1814, but which have never been found. Criticism of Sraffa's interpretation has concentrated on what is judged insufficient textual evidence to support Sraffa's reconstruction.

It is not my intention to weigh up here the arguments presented by critics of what has been ill-defined as the 'corn model' hypothesis (it would be more correct to call it the 'corn-ratio theory', which is the term Sraffa used in the Index to Ricardo's *Works*), nor the–as I see them–valid answers of its supporters.[31] Rather, I wish to contribute to the debate with two observations: the first on the weight of this particular hypothesis vis-à-vis the overall interpretation of Ricardo offered by Sraffa, the second on the role that textual evidence can play within the debate.

Hypothesising that Ricardo had initially considered product and capital in terms of corn alone, Sraffa set himself two objectives:

a to offer a reconstruction of how Ricardo arrived at rational formulation of a theory of profits, which normally requires comparison between heterogeneous commodities, despite having somewhat vague and changeable ideas about the principles regulating the relative values of commodities, as was the case prior to publication of *Essay on profit* and up to formulation of the labour theory of value in the course of 1816;

b to bring out the unity and consistency in the development of Ricardo's theory of distribution, singling out two themes constantly recurring from the first formulation to the last, namely: i) Ricardo's conviction that the rate of profit depends on the conditions of production not of all the commodities, but only of a subset of commodities constituting the wage; ii) Ricardo's preference for using division of the social product in physical terms to determine or illustrate the laws of income distribution. The fact that Sraffa recognises that the hypothesis of homogeneity between capital and product in the agricultural sector coincides–in the language of *Production of Commodities*–with a role for corn as standard commodity in a system with one basic commodity must not affect evaluation of Sraffa's interpretation of Ricardo. In the *Production of Commodities* appendix on references to the literature (Sraffa 1960a:93) Sraffa tells us that the corn-ratio theory was prompted by his own investigations on the standard commodity and the distinction between basics and non-basics. This does not mean that he forced interpretation of Ricardo to create an illustrious predecessor to his own theories.

We shall have more to say about the reconstruction of Ricardo's early theory of profit later on. As for the coherence Sraffa discerned in Ricardo's approach to the problem of distribution, there can be no doubt that the two themes mentioned above–the role of wage goods in determining the rate of profit and the expression of distribution of income in physical terms–are to be found in the mature work of Ricardo. The idea that only the conditions of production of wage goods affect the rate of profit is repeatedly asserted in Ricardo[32] and used, for example, in discussion of the effects of taxation on certain goods.

Similarly, we find frequent recourse to examination in terms of physical quantities of the effects on distribution of variations in the production conditions of wage goods or in the quantities of goods attributed to the workers. However, it is a method that serves several functions. When Ricardo is convinced he has a satisfactory theory of value, as in the first edition of the *Principles,* the example in physical terms serves a primarily illustrative purpose, casting revealing light on the inverse relationship between wages and profits (for example, representing distribution of the product in terms of the ratio between labour engaged in the production of wage goods and total amount of labour (*RW* I:49) has an essentially illustrative function). When, on the other hand, as in the third edition of the *Principles,* Ricardo is evidently aware of the difficulties involved in the theory of labour-value, recourse to measurement of the aggregate product in terms of invariable standard was–if identification of an invariable standard of value had been a solvable problem–meant to afford the possibility to represent distribution between wages and profits as shares of the given social product. In fact, Ricardo's definition of the characteristics of the invariable standard meets the need to supply a way of measuring the value

of the *aggregate* product that does not vary with distribution since the fall in some prices is exactly offset by the rise in prices of other commodities, so that the total product remains constant in terms of the standard. This is the 'price-balancing' role played by the standard mentioned by Sraffa in a sentence–inserted during proof-reading[33]–in the Introduction (*RW* I:xliv–xlv), and which has recently come in for criticism (Peach 1998:608) on the grounds that Ricardo would have no macro distribution, but only micro analysis at the level of individual farm. However, Ricardo's choice as invariable standard of a commodity produced with a capital-labour ratio representing a mean between the two extremes (no fixed capital or virtually only fixed capital [*RW* VIII:193]) makes sense only when arguing in aggregate terms. If the use of the invariable standard had been to measure changes in the value of a single commodity, then the commodity chosen as standard would have had to be one produced with the capital-labour ratio employed in the production of the majority of the commodities, which is by no means a medium between the two extremes.

Hypothesising analysis in terms of corn in Ricardo's initial formulation of the theory of profits, Sraffa thus dates such features of Ricardian analysis to 1814, and by so doing shows his theory developing with unity and coherence. Sraffa's decision to bring in his hypothesis on the 'corn-ratio' in the Introduction to the *Principles,* where the entire evolution of the theory of value and distribution is reconstructed, and not in the introductory notes to the *Essay on Profit,* was probably dictated by this need to underline this essential consistency and highlight a dominant theme in Ricardo's thought.[34] Let us, therefore, suppose that new material might be found showing Sraffa's 'corn-ratio' hypothesis to be completely wrong, and that Ricardo had come to argue the fall in the rate of profit along other lines. What we would lose in Sraffa's overall interpretation would essentially be a reconstruction of Ricardo's thought as a system for investigation that always followed the same research strategy.

Obviously, another thing we would lose is the hypothesis of Ricardo's rationality that guided Sraffa in suggesting that the corn-ratio was the 'rational foundation' of Ricardo's argument. This brings us to the second theme, namely the role played by textual evidence in the debate. As I see it, the only relevant textual evidence here is to be found where Ricardo sets out his convictions regarding the movement of wages and prices. Given that Sraffa himself recognised that nowhere, in the writings that have come down to us, does Ricardo explicitly state that in the agricultural sector capital consists solely of corn with the aim of determining the rate of profit through comparison of physical quantities, the debate on how close to or far from this hypothesis Ricardo's extant observations are can never be brought to a satisfactory conclusion. What lends support to Sraffa's interpretation is the fact that this is the only reconstruction we have that is compatible with Ricardo's observations on the relative variations in wages and prices as

recorded in the textual evidence, and compatible with the hypothesis that Ricardo had some rationale for his thesis.[35] This point may be clarified by reconstructing the terms of the problem.

The problem for all Ricardo's interpreters has always been to explain how Ricardo could–from early 1814, when it was explicitly formulated for the first time in the letter to Trower[36] until his last works–have maintained the thesis that capital accumulation determines a decrease in the rate of profit while his views on prices and value were going through a substantial change. In fact three stages in the development of Ricardo's theory of price have been recognised:

a Throughout 1814 Ricardo believed that a rise in the price of corn determined a rise in wages and, therefore, in the price of all other goods, whether or not corn entered as input into their production. This thesis was maintained by Ricardo as late as the 23 October 1814 (*RW* VI:149);
b In the *Essay on Profit* (March 1815) prices are assumed to be constant in the face of a rise in the price of corn and to depend only on the 'difficulty or facility of production'. Nevertheless a precise theory of what exactly determined the exchange value of commodities, so that the effects on prices of changes in the conditions of production of wage goods could be analysed, was yet to be presented;
c Beginning with 1816, what determines the exchange value of commodities is investigated and is made to depend on the labour necessary to their production.

At each stage Ricardo's proof of the effects of accumulation on the rate of profit consists of three parts:

i why the rate of profit in agricultural production diminishes;
ii why the rate of profit diminishes in all other sectors;
iii why no other cause (variations in demand, variations in the conditions of the labour market) could offset the effects on the rate of profit of diminishing returns in agriculture, or in other words, why the conditions of production in the agricultural sector are a sufficient ('permanent' in Ricardo's terminology) cause of variations in the rate of profit.[37]

Let us reconstruct the framework in which the debate between Ricardo and Malthus on the effects of accumulation on the rate of profit took place. The rate of profit on the capital invested in agricultural production on the marginal land, under the assumption that there is only circulating capital, may be defined as

$$\pi = \frac{pQ}{(w+k)L} - 1 \tag{1}$$

where:

p = price of corn
Q = quantity of corn produced, in real terms
L = number of labourers employed to produce Q
w = corn per labourer, in money terms
k = non-corn capital per labourer, in money terms

Wage goods other than corn are therefore included in k.

In stage (a) above Ricardo still believed that a rise in the price of corn increased all other prices. However, in order to show that the rate of profit diminishes at a general level, Ricardo has to assume an asymmetrical increase in prices, so that the price of corn increases (almost) proportionately with the price of labour, but the prices of other goods do not (*RW* VI:120).

If all prices increased proportionately, the proof that the rate of profit in agriculture diminishes would be very easy.[38] In fact, if we rewrite (1) as:

$$\pi = \frac{Q}{\left(\frac{w}{p} + \frac{k}{p}\right)L} - 1 \tag{2}$$

i.e., if we transform a ratio in money terms into one in terms of corn (as Ricardo and Malthus often did in their debate), it is clear that while the proportionate increase in p, w and k leave ratios w/p and k/p unaffected, the increase in L–the labour necessary to produce the same quantity of corn–implies a decrease in the rate of profit. Given that all prices increase in the same proportion, the argument is identical in monetary or in real terms, i.e. whether we assume that output and capital actually consist of corn (this means that k=0), or are just measured in corn. But if all prices increased in the same proportion, there would be no decrease in the rate of profit in all other sectors where the productivity of labour does not change, as the increase in wages would be exactly compensated for by an increase in the price of output.

Ricardo had therefore to conclude that the increase in the price of goods other than corn was lower than the increase in wages. It should be noted that, in strictly logical terms, this makes proof of the diminishing rate in agriculture more difficult, as the decrease in k/p at the denominator of (2) has a tendency to raise and not to lower profits. However, we could think that Ricardo *assumed* that this effect was not great and, offset as it was by lower productivity, could be neglected:

> not only will the rate of wages rise, but more labourers will be employed without affording a proportional return of raw produce. The whole value of wages paid will be greater compared with the whole value of the raw produce obtained.
>
> (*RW* VI:146)

But at this stage the actual logical difficulty lies in the proof that the rate of profit also diminishes in other sectors: why should prices of commodities other than corn increase less than wages? The answer lies in the equality between expenditure and production which is assumed by Ricardo, according to Say's Law:[39]

> The rise of the price or rather the value of corn without any augmentation of capital must necessarily diminish the demand for other things even if the prices of those commodities did not rise with the price of corn, which they would (tho' slowly) certainly do. With the same capital, there would be less production, and less demand.
>
> (*RW* VI:108)

Thus the less than proportionate increase in the prices of manufactured goods determines a fall in the rate of profit also in all non-agricultural sectors of the economy:

> 'If every person is determined to live on his revenue or income, without infringing on his capital, the rise of his goods will not be in the same proportion as the rise of labour, and *consequently* his percentage of profits will be diminished.
>
> (*RW* VI:120; emphasis added)

Up to this point, therefore, Ricardo could equally well have argued the fall in the rate of profit reasoning in terms of value, of course with the theory of value he then had at his service.

However, one of the first objections to be raised by Malthus, and one that has a logical foundation, refers to the possibility of a decrease in terms of corn of that part of capital that does not consist of agricultural goods, k/p. This criticism, which also applies to stage (a), is particularly serious in stage (b), when Ricardo, in the *Essay on Profit,* abandons the assumption that the prices of goods other than corn increase following a rise in wages. In this case the difficulty of proving that an increase in wages diminishes profits in all sectors is overcome, since this outcome is obvious if costs increase while the constancy of output and prices leaves the revenues unchanged.[40] Ricardo does not need to resort to explanations based on Say's Law.

The difficulty that arises at this stage is the effect of a diminishing k/p over the rate of profit, which is far greater with constant prices than with increasing prices–it is greater with constant k than with increasing k–and it could offset the decrease in the rate of profit caused by the increase in the number of labourers required for the production of the same quantity of corn. As Malthus wrote to Ricardo:

> The expenses estimated in corn will be less, owing to the power of purchasing with a less quantity of corn, the same quantity of fixed capital,

> and of the circulating capital of tea sugar cloths &c: for the labourers; and consequently more clear surplus will remain in the shape of rents and profits together, (no matter which) for home demand.
>
> (*RW* VI:185)

There is only one way to eliminate this possibility logically, as Ricardo was well aware,[41] and that is to assume that k/p variations are negligible even with a constant value for k, namely that $k=0$, or in other words that the entire capital consists of corn.

This is the 'rational foundation' of Ricardo's theory of profits, which thus leaves only two alternatives open: either we must suppose that Ricardo formulated a theory of profits without basing it on any rationale, or Sraffa's corn-ratio theory remains the only plausible explanation.

Ricardo's 'sheet anchor'

While Sraffa's interpretation of the guiding role played by the farmer's profits has fuelled a heated debate, little has ever been said on another significant part of Sraffa's reconstruction of how Ricardo's theory of value evolved. Transition from what we term stage (b) in the previous section to stage (c) in the theory of value–i.e. the beginning of thorough investigation into the determinants of exchange values–was according to Sraffa (*RW* I: xxxiv) marked by the letter Ricardo wrote to Mill on 30 December 1815, the points of which were returned to in very similar terms in a letter to Malthus two days later:

> This invariability of the value of the precious metals, but from particular causes relating to themselves only, such as supply and demand, is the sheet anchor on which all my propositions are built; for those who maintain that an alteration in the value of corn will alter the value of all other things, independently of its effects on the value of the raw material of which they are made, do in fact deny this doctrine of the cause of the variation in the value of gold and silver.
>
> (*RW* VI:348–9)

This is one of the strongest assertions we find in all the writings of Ricardo, who was rarely so explicit about the assumptions behind his theory, and we may therefore reasonably wonder what this 'sheet anchor' his thesis rested on actually consisted in.

According to Sraffa (*RW* I:xxxiv), three themes converge in this passage: the distinction between causes influencing the value of commodities and the value of money; the invariability of the value of the standard for the currency; and opposition to the prevalent doctrine that a rise in the price of corn brings about rises for all the other commodities.

However, Sraffa adds little to explain what this interweaving of themes consists of, just as he chose to ignore the role Ricardo's convictions on

monetary matters might have played in the development of the theory of value and distribution, thus treating the theory of value and the theory of money as two completely separate fields in Ricardo's investigations, with scant reciprocal influence. And yet the theory Ricardo set out in his monetary writings is crucial to our understanding of just what the 'sheet anchor' was that Ricardo resorted to at this stage in his formulation of the theory of value.

The thesis that the prices of all commodities increase with an increase in the price of corn struck Ricardo as contradicting three convictions that he returned to repeatedly, and which he held most firmly:

i that there are three causes of variations in the price of commodities: depreciation of the currency, which can be ascertained by looking at its purchasing power over gold or silver, which are the standard of the currency; a change in the value of the precious metals; a change in the value of the commodity itself;
ii that a change in the value of a commodity, whatever this means, is reflected in its exchange value with all other commodities;
iii that gold and silver are commodities, in spite of the fact of having been chosen as standard for the currency, and therefore their value is subject to the same laws that regulate the value of all other commodities.

Without attempting to summarise Ricardo's entire monetary theory, a few words are needed to illustrate points (i) and (iii), given that point (ii) requires no particular explanation.

The whole point of Ricardo's participation in the debate on the monetary questions of his times–to which he contributes with all his writings previous to the *Essay on Profit*–was precisely to provide a criterion to distinguish between a rise in the prices of commodities and depreciation of the currency. These are not, as supposed by most interpreters of Ricardo, two names for one thing. The criterion proposed by Ricardo was to measure the value of the currency in terms of gold, the commodity chosen as standard for the currency.[42]

On the basis of this principle, Ricardo's analysis pivots on the definition of the value of money as measured by the purchasing power of the currency over gold, and not over commodities. In other words, changes in the value of the currency are identified with changes in the market price of gold, at home or abroad, no matter what happens to all other prices. It was only the changes in prices accompanied by change in the price of gold–and not any rise in the prices of commodities–that had monetary causes and depended on an increase in the quantity of money beyond its 'natural' level.

If gold and commodities rose together in price, the purchasing power of the currency over gold changed, and the cause of inflation was the depreciation of the currency. It is only in this case that an increase in prices

implies a previous increase in the quantity of money, since the only cause of depreciation, in Ricardo's view, is a quantity of money beyond its natural level. But if prices rose while the price of gold was constant, there was no change in the value of money and the cause of inflation lay in other 'real' phenomena such as taxation, changes in the conditions of production, or even variation in the value of gold itself.[43]

Whenever prices rise the first thing to do, Ricardo argues, is to see what has happened to the price of gold, since only thus, verifying some variation in it, can we understand whether we are up against a real depreciation of the currency or some other economic phenomenon. Ricardo argues this thesis without any substantial changes from the first of his works to the last.

The fact that gold (or silver) are commodities like any other is another conclusion of Ricardo's monetary theory, and he always argued that the fact that gold had become the standard of money did not affect its properties as a commodity. According to Ricardo, money itself can in no way modify the value of gold, and if the price of gold varies the cause must be sought in the quantity of pounds in circulation, and not in variations in the value of gold. It is gold that determines the value of money, and not vice versa. As Ricardo wrote to Malthus:

> I have observed in the bullion pamphlet that many who say they consider money as a commodity, and subject to the same laws of variation in value from demand and supply as other commodities, seldom proceed far in their reasoning about money without shewing that they really consider money as something peculiar, varying from causes totally different from those which affect other commodities.
>
> (*RW* VI:203)

Thus Ricardo's 'sheet anchor' is the following line of reasoning, which has the nature of a *reductio ad absurdum*. Let us suppose for the sake of argument that a rise in the price of corn brings about rises in the prices of all the other commodities. We shall admit no depreciation of the currency, and thus assume that its equivalent in gold is as legally fixed. In this case a rise in the prices of all commodities except gold, whose price is fixed since gold is the standard and, as we have said, the currency has not depreciated, would be tantamount to saying that the value of gold had changed, since its exchange value with other commodities has changed. But changing conditions in the production of corn cannot bring about variation in the value of gold since this would contradict the 'invariability of the value of the precious metals, but from particular causes relating to themselves only'. Therefore it is impossible for all prices to change as a result of variation in the price of corn.

Of course, Ricardo has still not explained what determines the value of the commodities. The value changes only 'from particular causes, such as supply and demand' or 'things neither rise nor fall but from difficulty or

facility of production' (*RW* VII:3). Nevertheless, whatever determines their value, the simple fact that commodities have a value implies that variations in the price of corn cannot give rise to a general increase in prices.

It is from this point that Ricardo embarks on his investigations into the theory of value and prices, which eventually led him to abandon the hypothesis of the guiding role played by the agricultural sector and demonstrate the inverse relation between wages and profits in more general terms.

Conclusions

The interpretation of Ricardo's 'sheet anchor' offered here does not clash with Sraffa's interpretation, but complements it. It is after all to Sraffa that we owe a renewed interest in Ricardo, and the opening up of numerous paths exploring his theory. We might indeed wonder what–without Sraffa–would have become of Ricardo: disseminator of a theory of rent that was not his own, supporter of a defective theory of wages and population, confused theoretician of labour-value. Of the many examples of good luck that Stigler attributes to Ricardo–living in one of the most interesting periods from the point of view of economic theory, being able to reconcile a life of business with theoretical speculation–the fact that he had Sraffa as editor must in the long run be considered the most important.

Notes

1 See letter by Keynes to T.Gregory, 21 March 1930 in the *Sraffa Papers,* Wren Library, Trinity College, Cambridge (henceforth indicated as *SP*), D3/11/62.47.
2 Annotations in Sraffa's diary of 1930 in *SP* E4 and letter from Sraffa to Keynes of 22 March 1930: 'We have finished yesterday with Dobb and Kahn and Isles our five days work of collating the editions of the Principles' (Isles had been a student of Sraffa in the academic year 1929), Keynes Archive, King's College, Cambridge, US 44.
3 See letter from Keynes to J.Hollander, 3 April 1930, in *SP* D3/11/63.71 .
4 In November 1930 a letter from Keynes asking for unpublished material appeared in *The Spectator, Times Literary Supplement, The Nation & Atheneum, The Economist* (*SP* D3/11/56).
5 The choices for bibliography and correspondence were set out in the above-cited letter from Keynes to Bonar. They are illustrated and explained in a letter from Sraffa to Viner of 31 January 1932, where the choice by subjects was deemed unfeasible above all for the parliamentary speeches by Ricardo, in which 'it would be impossible to disentangle corn and currency' (*SP* D3/11/74.5).
6 A newspaper cutting with a 1930 review of the works of Vincenzo Monti is among Sraffa's papers, full of underlinings as if Sraffa found his own ideas reflected. It says: '…Monti has the luck of having papers published and illustrated as papers should be published and illustrated to be useful not only to "specialists", who may have very particular data recorded in their files, but to all scholars, to whom too many things would remain unintelligible without such minute, diligent illustration, who too many times would ask "Who on earth is it?"–Not only, but it will be worth having access, as far as it can be found, to the letters of the very many who prompted a letter from Monti or answered him. An epistolary can be a soliloquy' (my translation from Italian) in *SP* D3/11/13.10.

7 Henceforth indicated as *RW,* VI:xiv.
8 Notes on the expenses for the edition, *SP* D3/11/65.66.
9 See letter from Keynes to O'Brien, 28 July 1943, *SP* D3/11/67.79.
10 Letter from Bonar to Sraffa, 18 July 1930: 'He [Hollander] said (by the way) that he doubted the wisdom of republishing Ricardo's works. Much that is published already lies unsold and commercially unprofitable' (*SP* D3/11/57.49).
11 It is worth noting that Sraffa had originally supposed that apart from chapters 4 and 5, and chapters 9 and 10 considered in the Introduction, chapters 2 and 24 were originally one single chapter. We do not know whether he abandoned the idea as unfounded or for lack of sufficient proof.
12 Material for preparation of the Introduction to the *Principles* in *SP* D3/11/4.
13 Kahn papers at King's College, Cambridge (*RFK* 13/90). My thanks go to Cristina Marcuzzo for drawing my attention to this letter.
14 Letter from Keynes to Sraffa of 7 February 1938: 'I feel that is probable, though I may be wrong, that the main hold up is in the literary drafting of the material already collected for introductions and certain footnotes; and I am fearful that the more or less routine work of cross references and the like may be used, as perhaps it has been for years past, as an alternative occupation and an excuse for putting off the drafting' (*SP* D3/11/65.37).
15 See letter from Keynes to Sraffa, 5 August 1939, *SP* D3/11/65.28.
16 See letter from Sraffa to Viner, 30 July 1942, *SP* D3/11/74.24.
17 When the volume of the *Principles* finally came out, Barbara Lowe, who had assisted Sraffa for many years in the Ricardo's edition, wrote to him in disbelief: 'How did you ever bring yourself to finish the introduction?' (*SP* D3/11/83.214).
18 See letter from Sraffa to Keynes, 31 March 1943, in *SP* D3/11/65.26.
19 See letter of Raffaele Mattioli to Sraffa, 15 March 1955, in *SP* D3/11/83.6.
20 *SP* D3/11/59.6. The discussion on inclusion of the notes on Malthus' 'Measure of value' in the *Works and Correspondence* of Ricardo is in Porta (1995, 1996) and de Vivo (1996).
21 Letter from Austin Robinson to Keynes, 2 February 1938, in the Archive of the Royal Economic Society. I am grateful to Nerio Naldi for drawing my attention to this letter.
22 Note by Dobb in *SP* D3/11/59.4.
23 Among which the remark that the Fragments on Torrens anticipates Marx's distinction between constant and variable capital (*RW* IV:306), an observation on the importance of the manuscript on Absolute value and exchangeable value (*RW* IV:359), and a note opposing identification of Ricardo with the originator of all marginal theory, Ricardo being erroneously attributed with having invented the theory of rent (*RW* IV:6n.).
24 The volumes from 5 to 9 came out in 1952, volume 10 in 1955, and volume 11 with the index many years later, in 1973.
25 My translation from Italian. The review appeared in *Lo Spettatore Italiano,* October 1951.
26 See letter from Dobb to Theodor Prager, cited in Pollitt (1990:524).
27 Review by Viner for the *New York Times,* 14 October 1951, copy typed for Sraffa, differing from the version published, in *SP* D3/11/84.54.
28 '…all classical writers accept implicitly, as an obvious fact, that cost is independent of quantity, and they do not bother to discuss the contrary hypothesis', Sraffa (1925a [1998:325]). See also Sraffa (1926a:537).
29 This point of view receives extensive reconsideration by Sraffa in his notes for lectures given in the academic years 1928–9, 1929–30, 1931, in *SP* D2/4
30 See, for example, the importance Bharadwaj (1988) attributed to Sraffa's reconstruction which saw Ricardo successfully identifying the effect on profits of variations in the production conditions of wage goods before arriving at a theory of rent. In contrast, Sraffa's critics focus on the corn-ratio theory, the role of the

invariable measure of value and the analysis of distribution with a given output (Hollander 1979; Peach 1993).

31 For the debate on the corn-ratio theory following upon Hollander's criticism of Sraffa's interpretation (1979), see Garegnani (1982), Vianello (1998); for the debate following on Peach's interpretation (1993), see De Vivo (1994), Mongiovi (1994), Kurz (1994) Hollander (1998) and Peach (1998).

32 See for example I, 132.

33 See *SP* D3/11/93.

34 Obviously it is possible that in 1939, when the preface to the *Essay on Profit* was completed, Sraffa had yet to form a clear idea about the measurement of profit in terms of corn, although his choice to use the phrase about the regulating role of farmer's profits in the editorial notes to the *Essay on Profit* lends little support to such a supposition.

35 This is the line taken in support of Sraffa's interpretation by Vianello (1998).

36 The letters of 10 and 17 August 1813, which are sometimes referred to as 'the first statements of Ricardo's theory of profits' (Garegnani 1982:67), do not contain a formulation of Ricardo's theory of profit as explicit as the letter to Trower of 8 March 1814, although it is clear from them that Ricardo already believed that agricultural prices affected the rate of profit, as noted by most commentators. But in the letters of August 1813 Ricardo does not say that the rate of profit is determined exclusively by the prices of wage goods. Rather, he maintains only that lower prices of wage goods could have offset the effects on the rate of profit that an increase of capital would have produced, according to the Smithian theory of the 'competition of capitals'.

37 On the distinction between temporary and permanent causes in Ricardo's analysis, and the identification of the latter with sufficient conditions, see Marcuzzo and Rosselli (1994).

38 This is the argument advanced by Peach (1993:67).

39 It may also be argued that Ricardo was a firm believer in the principle of equalisation of the rate of profit in all sectors of the economy and that therefore had *to assume* that the rate of profit diminished in all sectors when it decreased in agriculture. In this case the less than proportionate increase in the prices of commodities other than corn would be the consequence and not the cause of the assumed fall in the rate of profit: 'as the profits of the farmer must necessarily decrease with every augmentation of Capital employed on the land …all other profits *must* diminish and therefore the rate of interest must fall.' (*RW* I:104; emphasis added). However, the textual evidence to support this interpretation is scanty.

40 The sole effect then of the progress of wealth on prices, independently of all improvements, either in agriculture or manufactures, appears to be to raise the price of raw produce and of labour, leaving all other commodities at their original prices, and to lower general profits in consequence of the general rise of wages' (*RW* IV:20).

41 Beginning with a letter of 5 August 1814, Malthus had drawn Ricardo's attention to the presence of commodities other than corn among those constituting capital. The objection based on the decrease in the value of capital subsequent to an increase in the price of corn was explicitly formulated by Malthus on 12 March 1815, but certainly does not seem to have been greeted by Ricardo as anything new.

42 On this point, in far greater detail, see Marcuzzo and Rosselli (1991) and (1994).

43 'While gold is exclusively the standard in this country, money will be depreciated, when a pound sterling is not of equal value with 5 dwts. and 3 grs. standard gold, whether gold rises or falls in general value' (*RW* I:149).

12 *Production of Commodities by Means of Commodities* between criticism and reconstruction

The given quantities assumption

Alessandro Roncaglia[1]

1 Introduction

Forty years have passed since *Production of Commodities by Means of Commodities* was first published, but interpretation of the text still rouses lively debate. Of course, any particularly concise dissertation–and Sraffa's certainly is that–may be open to various interpretations, but the extraordinary precision of Sraffa's prose should leave little room for misunderstandings to arise. What they arise from, however, is an additional difficulty, namely the radical difference between his type of analysis and the lines of argument customarily followed by the vast majority of contemporary economists.

Sraffa himself refers to the problem in the opening lines of his book: 'Anyone accustomed to think in terms of the equilibrium of demand and supply may be inclined, on reading these pages, to suppose that the argument rests on a tacit assumption of constant returns in all industries' (Sraffa 1960a:v).

Two related themes emerge from this short passage (and, of course, from the following pages), and they will be the subject of our considerations here. In the first place, Sraffa suggests that at least two categories of economists exist: those who are 'accustomed to think in terms of the equilibrium of demand and supply', and those who are not. Second, Sraffa points out that a crucial difference between these two groups of economists–or between these two approaches, paradigms or theoretical frameworks–lies in the role played by the quantities produced in analysis of prices and their relationship to income distribution.

We shall broach the matter (in §2) by underlining a philologically irrefutable fact: in his analysis Sraffa takes the quantities produced in the various industries as given.[2] This raises a number of questions, particularly in relation to the role played by demand in price determination. We may get these aspects into clearer perspective by reconsidering (in §3) the distinction between Sraffa's approach and the approach dominating contemporary theory in relation to the analytic structure and 'vision' of the economic process. We then go on (in §4) to address–again in brief outline–the problem of the differences between the marginalist and the Sraffian approach at the

level of method, recalling the influence Sraffa exerted over Wittgenstein. Finally, in §5, we shall take a look at the relationship between Sraffa's and Keynes' analysis as it emerges from our interpretation.

2 The quantities produced assumption

In his analysis Sraffa is quite unequivocal on the point that he takes the quantities produced as given. In a text of exemplary concision, he actually repeats himself to stress the point:

> No changes in output and (at any rate in Parts I and II) no changes in the proportions in which different means of production are used by an industry are considered, so that no question arises as to the variation or constancy of returns. The investigation is concerned exclusively with such properties of an economic system as do not depend on changes in the scale of production or in the proportions of 'factors'.
>
> (Sraffa 1960a:v)

For Sraffa the point is not only crucial, but also a potential source of misunderstanding. It is, indeed, an assertion that can hardly go down with readers taking demand and supply equilibrium theory to their perusal of the book. For such readers–the overwhelming majority of contemporary economists–it is easier to see *Production of Commodities by Means of Commodities* as half (the half they consider the supply side) of a system of general economic equilibrium. Indeed, flying in the face of these explicit statements (which, moreover, are not *obiter dicta* but the pondered opening to a deeply pondered text), a number of economists have advanced this interpretation.[3]

Close on a century after the event, this interpretative error re-evokes the error Marshall made in relation to the theory of Ricardo, and of the Classical economists in general. Marshall, as we well know, held that they were aware of only one of the two blades of the scissors determining price–the supply side, but not the demand side.[4] In this case, too, Classical analysis was rendered comparable to the analysis in terms of demand and supply equilibrium by introducing in it the assumption of constant returns.[5]

Sraffa, who in his critical edition of Ricardo's *Works and Correspondence* had, among other things, also disputed Marshall's interpretation, foresaw quite clearly that the same error would once again crop up in connection with his own analysis. Indeed, he appeared ready to accept the inevitable up to a point. If you really cannot help reasoning in terms of demand and supply equilibrium, the gist is, then go on and assume–but only as an initial step–that I am considering the case of constant returns: 'If such a supposition is found helpful, there is no harm in the reader's adopting it as a temporary working hypothesis. In fact, however, no such assumption is made' (Sraffa

1960a:v: these lines come between the first and second of the two passages quoted above).

Here a problem arises. If the hypothesis of constant returns constitutes such a dangerous misunderstanding, how can Sraffa possibly deem it acceptable for the first few steps?

Luckily, the answer here is simple enough. The fact is that Sraffa's aim in writing *Production of Commodities by Means of Commodities* was two-fold. On the one hand, he set out to provide the 'prelude to a critique of economic theory', as indicated by the subtitle (where 'economic theory' means 'the marginal theory of value and distribution', as Sraffa himself takes care of specifying in his Preface, Sraffa 1960a:vi); at the same time, on the other hand, he intended to solve certain analytical problems–in particular the link between relative prices and distribution of income–that the Classical economists had left unsolved, and which had contributed to the crisis of the classical approach and thus the dominance of the marginalist approach. Now, those brought up on the marginalist tradition must first of all learn to recognise the logical difficulties inhering therein; only then will it prove useful to discover that the Classical approach is rather more solid than is generally granted, and so discover that it does not collapse simply because the labour theory of value does not hold. Criticisms, however–or the premises for a critique–of the marginalist theory of value and distribution can be perfectly well advanced, studied and discussed referring to one particular case of marginalist theory itself, namely that of constant returns, considering Sraffa's analysis as 'internal' to the theory of general economic equilibrium *solely to this end*.[6] One point that must be quite clear here, however, is that when we go on from criticism of marginalist theory to reconstruction of the Classical approach, the hypothesis of constant returns must be abandoned: as Sraffa repeated, 'no changes in output…are considered' or, in other words, the quantities produced by the various industries are given.

3 The clash between the Classical and marginalist approaches

Thus, at one and the same time *Production of Commodities by Means of Commodities* constitutes a critique from within the marginalist approach and a contribution within the Classical approach. If such a thing is possible, it is thanks to the fact that certain logical relations must hold in any case; however, they occur in different contexts, as attested by the fact that the hypothesis of constant returns is necessary if we are to read these propositions in the context of marginalist theory, while it is not if we read them as part of Classical theory.

The point will emerge more clearly if we turn our attention to the basic differences between the Classical and marginalist approaches, considering them as two 'paradigms' (in the sense suggested by Kuhn 1962) expressing two different conceptions of the way the economic system works. It is a

difference that Sraffa points out in the conclusion of his book, in Appendix D, 'References to the literature'. Here Sraffa contrasts 'the picture of the system of production and consumption as a circular process', characterising the Classical approach 'to the view presented by modern theory, of a oneway avenue that leads from "Factors of production" to "Consumption goods"' (Sraffa 1960a:121).

These expressions sum up radical differences in the 'vision' of the economic world, both in the conceptual apparatus used to represent it and the theoretical structures constructed on those bases.

Let us begin with the Classical approach. The economic system is organised on the basis of the division of labour, which does not derive from differences in the original endowment of resources but rather from the intrinsically social nature of men and women.[7] The division of labour is both 'macroeconomic', between sectors, and 'microeconomic', within each production process.[8] As a result of the macroeconomic division of labour, each economic subject–whether individuals or firms–must at the end of the production process enter into relations of exchange with the other economic subjects to procure the wherewithal to survive and relaunch the production process. In the economic system as a whole, the quantity of each commodity produced is usually more than enough for these purposes.[9] That portion of the total output that exceeds the strict needs of reproduction–the surplus–may be channelled into consumption exceeding subsistence, or into investments, the choice here being associated with the way in which the surplus is distributed between the various economic subjects.[10] Thus exchange relations are 'natural' when they express the conditions of reproduction in the circular process of production and consumption, or in other words when each economic subject recovers what is needed to repeat activities in the following period, and when they find it advantageous to do so, the distribution of surplus respecting the condition of uniform rates of profit in the various sectors and thus reflecting the fundamental idea of capitalist competition, i.e. the free flow of capital between the various sectors of the economy.[11]

In this tradition the concept of market does not correspond to a point in time and space upon which purchasers and sellers converge, but rather to a network of repetitive and sufficiently regular trade flows, and thus to a network of interpersonal relations underlying these flows, essential for the reproduction of the economic system. This conception can be found in all the Classical economists, at least from William Petty on (Roncaglia 1985:73–6). Here prices indicate the conditions for reproduction recalled above, and not the relative scarcity of commodities vis-à-vis the wants of consumers.

Thus we find a sharp contrast between the approach of the Classical economists and an even older conception, where the concept of market refers to a place in time and space upon which purchasers and sellers converge, and where trade relations are therefore determined by confrontation between demand and supply. The ideal reference point here is the medieval fair, and

then the Stock Exchange. It is from a development of this representation of the economic problem–as determination of the equilibrium arising from the demand/supply confrontation–that the subjective conception of value derives. The 'equilibrium' price (a term that found its place in economics alongside adoption of a methodological model inspired by physics, and in particular static mechanics) is that which ensures equality between demand and supply, or in other words allows for the balancing of opposed forces deriving from the scarcity of commodities and the desire for them. The problem remains essentially the same if it is the original factors of production that are scarce, equilibrium between demand for final consumption goods and the supply of original factors being mediated by production.[12]

In the Classical approach, the theory of value is based on technology and the principle for distribution of the surplus, taken as given, while the marginalist approach takes as given the endowment of resources and consumers' preferences (to which technology may be added). Here we come to the point of differentiation which Sraffa signalled; according to the Classical approach the 'problem of value' does not consist in determining the equilibrium values for prices and quantities exchanged (and quantities produced, where the model includes production) at the same time. More simply, it consists in determining the exchange ratios that satisfy conditions for reproduction of the economic system. It is only when the Classical and marginalist economic problem are put together that it appears necessary in any case–and thus within the Classical approach too–to determine quantities and prices simultaneously.

In the Classical approach, of course, separating the problem of 'reproduction prices' from that of quantities produced and exchanged does not imply that the problem of determining production levels lies outside the economist's field of work. An economist like Marx who takes reference from the Classics makes a clear distinction between three logical stages: the firms' decisions on the quantities to produce, the consequent theoretical analysis of the link between prices and distribution, and finally the problem of 'realising' the value of the commodities produced through sales on the market. As we shall see, the possibility of distinguishing various logical stages in economic argumentation, and indeed the utility of breaking the problem of representing the functioning of the economic system down into different 'theoretical pieces', correspond to a methodological line that Sraffa seems to have suggested in his exchanges with Wittgenstein.

4 Sraffa and Wittgenstein: the problem of method in economics

In his book Sraffa delimits with close rigour the object of his analysis and thus the data necessary to bring it to a conclusion.[13] The first given datum is the technology; in the absence of hypotheses on returns to scale, this means

that the technology corresponds to a given vector of production levels of the various industries.[14] Where a surplus is obtained, the manner of distribution must be specified: this Sraffa did taking as given one of the two distributive variables–real wage or rate of profits–and taking the competitive principle of uniform rate of profits as ruling the division of profit between the various sectors. On this basis, without there being any need for reference to demand, let alone for functions linking the quantities of each commodity in demand to their prices (and, in general economic equilibrium models, to the prices of other commodities, including the services of factors of production), Sraffa shows a way to determine production prices and the residual distributive variable, and to analyse the movements of these variables when the exogenous distributive variable changes.

While–as we have seen–there is no need for direct reference to demand, there is an indirect reference implicit in the assumption of quantities given. It is in fact obvious that the quantities to produce are determined by the decisions of the entrepreneurs, who take the foreseeable capacities of market absorption into account. However, what needs stressing here is that these are *ex ante* entrepreneurial assessments, and not *ex post* findings on consumers' demand; moreover, such assessments are not necessarily point estimates but, as often occurs in reality, may refer to discrete intervals. In practice, what is ruled out is any reference to a demand-supply mechanism for the determination of prices. Demand may have a significant but indirect effect on 'natural' prices since, over a period of time, it affects entrepreneurs' decisions concerning productive capacity and the normal degree of plant utilisation, and thus the technology and the relative bargaining power of wage-earners and profit-earners.[15]

This procedure–i.e. rigorous delimitation of the problem, reduced to the interplay of relationships between a limited number of variables–stands in contrast to the approach dominant in modern economic theory.[16] Within the framework of general economic equilibrium all the economic variables–prices, quantities, distributive variables (considered as prices of factor of production services)–are determined at one and the same time in one great analytic scheme. From this standpoint, the criticisms Sraffa raised against the Marshallian theory of the firm (contradiction between the hypothesis of competition and the *ceteris paribus* hypothesis typical of partial equilibria) are said to hold in relation to partial equilibrium analysis, but are considered irrelevant in relation to 'truly general' analysis, which is the only analysis acceptable for the pure theoretician.[17] An analogous evaluation is put forward concerning the 'Cambridge' criticism of the aggregate concept of capital, seen merely as a simplified parabola, a 'low level theory' compared with the 'true' theory, which is general equilibrium.[18]

In every field of science the idea that a general, all-embracing theory is superior to 'partial' theories has shown its appeal. The problem here–at least as far as the marginalist approach is concerned–is whether we are to sacrifice

rigour (in the case of the 'parabolas') or relevance (since the theory of general equilibrium offers scant heuristic scope, once the multiplicity and possible instability of equilibria are granted, and has little to do with the real world once we have recognised the need for hypotheses on the convexity of production and consumption sets, corresponding to the hypothesis of generalised decreasing returns for production and consumption alike) to the fetish of a general theory. This is no new problem. It has been addressed on various occasions in the philosophical and epistemological debate, and it is worth recalling that, thanks to his influence on Wittgenstein, Sraffa played a leading role here.

In short[19] we may put it as so. Originarily (in the *Tractatus logico-philosophicus* of 1922) Wittgenstein argued a correspondence between the 'facts' constituting the world on the one hand, and 'propositions' constituting our image of the world on the other. Thus we can describe the world with a set of propositions, each of which describes a 'fact'. Basically, the 'facts' are the atoms the world is composed of, while the set of propositions describing them offers an axiomatic description of the world itself–or rather, if not all the world, all of the world that can be described in a rational form. About anything else, that is in those cases where no rational description can be supplied, 'one must be silent'.[20]

The marginalist theory of general economic equilibrium seems to be founded on philosophical positions much like those of this early Wittgenstein: an atomist base ('economic subjects' and 'commodities'), correspondence between the facts of the world and the elements of theory, and the claim of a complete description according to general rules of all that is describable in the world (the general theory).

However, Wittgenstein eventually abandoned this conception, and he did so–as he himself notes in his preface to *Philosophical Investigations* (published posthumously in 1953)–under the influence of long discussions with Sraffa. In particular, Wittgenstein abandoned the idea of language as an axiomatic representation of the world and the idea of the 'unspeakable'. Instead, he developed the idea of 'language games'–models that focus the attention on particular aspects of real language, presenting them as the general language of a group of people. One commentator interpreted it thus: 'There is not…any unique analysis of propositions into their intrinsically unanalysable elements. What sort of analysis will be useful and provide a real clarification depends on the circumstances, on just what is problematic about the propositions under examination (Quinton 1968:12–13).

Of course, this is not to say that having criticised the early stages of Wittgenstein's reflections Sraffa then went on to endorse the conclusions. Nevertheless, we can see a distinct analogy with the method Sraffa follows in his book, focusing on a specific problem (fundamental as it may be) and on those variables directly relevant to the problem in question, but without denying the existence of other problems to be addressed with other language

games' and, in particular, without denying the indirect influence of other variables.

If this point is accepted, it will become perfectly clear how inappropriate any attempts are to extrapolate mechanically, from the analysis illustrated in *Production of Commodities by Means of Commodities,* Sraffa's theoretical position in other fields–for example, ascribing to him a quantitative theory of money (Boffito 1973:89). In other words, we cannot expect to 'extend' Sraffa's analysis by associating with his equations other equations taken to be 'on the same logical plane' or part of the same 'language game'. Nevertheless, this is precisely how neoclassical interpreters act when they set out to 'complete' the half system of the general economic equilibrium Sraffa is supposed to have analysed, adding to his 'supply' equations the appropriate demand equations.

A point worth stressing here is that this difference in method holds important implications for the significance to be attached to the concepts Sraffa analyses, generating appreciable differences from the corresponding concepts as approached with marginalist analysis. In particular, within the marginalist approach the concept of equilibrium refers to a state of equality between demand and supply (market clearing) throughout the economy while, within the Classical approach, as far as the concept is applicable,[21] reference is simply made to the absence of incentives to transfer capital from one sector of the economy to another ('competitive equilibrium'). Thus it is evidently a mistake to confuse Sraffa's prices of production (and the natural prices of the Classics) with 'normal prices' or 'long period equilibrium prices' in marginalist analysis.

At this point we come up against a problem which we shall very briefly outline here. If we accept the idea of separation between various 'language games', or in other words between the analysis of different problems–for example, if we distinguish the analysis of the prices-distribution link from the analysis of the factors determining levels of production or technology, or the distribution of income itself–there will no longer be any need to verify the possibility of constructing a single general model in which to include the various 'pieces of analysis' as fitting parts of a whole. Actually, each 'piece of analysis' implies a distinct process of abstraction, and thus belongs to its own 'analytic area', and no classification of decreasing generality can be determined between the various areas.[22] There is, however, the problem of the internal consistency of the conceptual framework–or conception of the way the economic system functions–within which the various 'pieces of analysis' addressing the different problems are inserted. For example, a 'monetary' explanation of the rate of profits as referred to by Sraffa (and which we shall be returning to shortly) is not compatible with a marginalist theory of value, where the distributive variables are the prices of the services of productive factors. Another issue we may consider in this light–as a problem of the consistency of the conceptual frameworks in which the two analyses are embedded–is the complex question of the relationship between Sraffa's and Keynes' analyses.[23]

5 Sraffa and Keynes

Thus, as we have seen, Sraffa's analysis may be located as falling within a Classical conception, where the task assigned to economic theory is to establish the conditions for reproduction of the system and to analyse its evolution over time. The various problems are obviously connected, but can be analysed separately. This applies in particular to the quantities produced by the various industries, which Sraffa–as we noted above–takes as an external given for the purposes of his analysis. Here we find a bridge stretching out in the direction of Keynes' analysis of the possibility of persisting situations of under-employment.

The best way to approach this issue is step-by-step, considering in succession the conception within which Sraffa's analysis is inserted, the applicability of 'Say's Law' to Sraffa's analysis, the relationship between prices of production and market prices, Sraffa's indirect reference to Keynesian theory and, finally, the 'bridge' between the two analyses.

As we have seen, although presented in a way that is formally compatible with marginalist analysis (in such a way that criticism to it can be developed from within), Sraffa's analysis was conceived in terms of a Classical approach, albeit making a great stride ahead at the level of analytic rigour and with precise delimitation of the problem addressed. The Classical approach revolves around the concept of surplus–its production, circulation through trade, distribution among the various social classes and the uses it is put to, i.e. accumulation or consumption beyond the bare necessities. Each of these aspects is related to the others, but for the sake of analysis it is better to take them in isolation: thus, for example, for the theory of production we have Smith's analysis (and Babbage's, and John Stuart Mill's) of the factors determining the division of labour; we then have the theory of value in connection with exchange ratios, and their relationship with distributive variables; analyses carried out by Smith, Ricardo, Marx and various others for the theory of distribution; the Classical theory of accumulation, and as a separate issue, what Marx described as the problem of realisation, i.e. sale of the quantities produced. In other words, we have a range of fields of analysis within each of which variables taken as given in other theories are to be accounted for, while variables explained in other 'pieces of analysis' are taken as given. This is, in fact, a procedure that Sraffa follows rigorously, 'cutting out' the problem of determination of technology or quantities produced which lie 'upstream' from his analysis, but at the same time isolating his problem from what lies 'downstream' like the question of realisation, or the relationship between prices of production and market prices.

Given this practice, there are clearly no grounds to argue that Sraffa adheres to 'Say's Law', which states that 'supply creates its own demand'. (Actually, there would be no good reasons why one should not argue the contrary, either, were it not for the requisite of consistency with the 'conceptual framework' Sraffa's analysis works in.[24]) Quite simply, the

problem of realisation is not addressed, and there is therefore no reason why there should be any automatic correspondence between aggregate demand and supply.[25]

Another point to clear up in this connection is the distinction between natural prices (or prices of production) and market prices. Sraffa confines discussion to pointing out, quite clearly, that his argument 'contains no reference to market prices' (Sraffa 1960a:9). This means that there is no textual evidence to ascribe to Sraffa the idea that prices of production are 'centres of gravity' for market prices, let alone attributing him with a conception of market prices as a theoretical variable determined (in some version of the Marshallian short period) by the interplay of demand and supply. Bearing in mind that the problem of realisation comes in logical sequence after the problem addressed by Sraffa, together with the fact that there is no good reason to establish any formal connection between prices of production and market prices (of the type of connection between long and short period to be found in Marshallian theory), there is no reason to assume that the quantities produced coincide with the quantities in demand when prices of production prevail (Smith's 'effectual demand'), commodity by commodity.[26] Obviously, this is a prerequisite for claiming that Sraffa does not adhere to 'Say's Law', which in fact concerns this equality in the aggregate. Of course, if technology is 'socially necessary', and thus corresponds to what entrepreneurs consider a normal utilisation of productive capacity, we must conclude that this equality is obtained over the average of a number of periods if the entrepreneurs' expectations are to be satisfied. However, in the course of time productive capacity changes (in general, grows). Consequently realisation on average, over a span of several years, of a normal degree of utilisation of productive capacity, holds no implications for any of the periods taken individually regarding the relationship between quantities taken as given (which may differ from those effectively produced if the degree of effective utilisation differs from what entrepreneurs see as normal) and quantities in demand at the natural price.

We may, moreover, wonder what possible reason there could be, if not respect for the marginalist (or, more generally speaking, subjectivist) tradition, for adding the condition of equality between demand and supply to that of uniformity of the rate of profits in the various sectors of the economy invoked by the Classics in their theory of competition. Indeed, we might say that, with his clean distinction between the various problems, Sraffa achieved a far greater clarity than those Classical economists who had sought a compromise with the subjectivist tradition.[27]

In the light of all these points we can begin to see some connection between Sraffa's analysis and Keynes'. Of course, the two analyses refer to different problems, and therefore cannot come into logical contradiction with each other. Moreover, if we avoid the neoclassical interpretations of Keynes (disregarding the question as to how much Keynes might have laid himself

open to them), the two analyses refer to a largely shared conceptual framework. In particular, both reject prices-quantities equilibrium associated with the full employment of resources–Sraffa with his criticism of the marginalist theory of capital and distribution, Keynes with his opposition to the orthodox theory of interest.

That Sraffa, for his part, considered his analysis open to integration with Keynes' is implicit in an often-quoted passage of his book: 'The rate of profits…is…susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest' (Sraffa 1960a:33). A dominant theme of Keynesian theory is that monetary and financial variables play a crucial role in determining the real variables (level of investments, income, employment).[28] In the passage cited above Sraffa seems to be opening the way for a similar thesis on the distribution of income; contractual wage bargaining between entrepreneurs and unions determines the monetary wage, but the level of real wage will depend upon money prices, which in turn depend on manifold elements including production and employment, but also the liquidity of the system and currency exchange rates.[29] The similarity between the two theses, and the fact that Sraffa did not intend to address the problem of distribution in depth with these observations, suggest that one of Sraffa's concerns here, if not his primary concern, was to underline the similarity between his outlook and Keynes'. Furthermore, in the Preface to *Production of Commodities by Means of Commodities* Keynes is mentioned with reference to the assumption of given quantities.[30]

The 'bridge' between Sraffa's analysis of prices and Keynes' analysis of production levels can be built along the following lines. In Sraffa's analysis, which looks to conditions for reproduction of the economic system, the prices of commodities used as means of production are equal to the prices of the same commodities included in the product, and the technology is given. When the technology changes, if we rule out the entirely hypothetical case of a proportional reduction in all the coefficients of production, the relative prices also change. If the changes in technology were known *ex ante,* we would have continual arbitrage between current and future production, with a mechanism of forward prices and own interest rates which, significantly, constitutes a theoretical contribution by Sraffa (1932a) taken up (and reworked, introducing expectations) by Keynes in the crucial Chapter 17 of his *General Theory*.[31] In general, however, it is impossible to take changes in technology as known *ex ante;* indeed, we may argue that it is precisely here that the major element of that all-pervasive uncertainty constituting a key feature of Keynes' vision arises, leading him to grant expectations a central role in his theory. For this reason the two problems–Sraffa's and Keynes'–must be kept apart. Nevertheless, given Sraffa's approach to his problem–isolating it from the problem of determination of quantities produced while avoiding any opening in the direction of 'Say's Law'–we may consider his analysis of the prices-distribution

link conceptually compatible with Keynes' analysis of employment once the latter has been cleared of marginalist encrustations.

Notes

1 This chapter is a slightly different version of Chapter 2 of my book *Piero Sraffa. His Life, Thought and Cultural Heritage,* London: Routledge 2000 (forthcoming). Thanks are due to Marcella Corsi, Nerio Naldi and Mario Tonveronachi for useful comments on a previous draft. The chapter is part of a research project on 'Italian economists' archives' (Murst, 40 per cent funds).

2 To be more precise, Sraffa takes the technology as given or, better, as the fundamental given element in his problem, i.e. the prices-distribution link; unless constant returns are assumed–and Sraffa explicitly declares he is not making such an assumption–this means taking the quantities produced as given. Cf. below, note 14.

3 See Johnson (1962); Robinson (1961); Hahn (1982). Joan Robinson did, however, eventually modify this interpretation: Robinson (1978:122).

4 See in particular the appendix to Marshall's *Principles* (1961:813–21; the reference to the blades of a pair of scissors is on p. 820).

5 As a matter of fact, Classical economists had different ideas on (dynamic) returns to scale: think, for example, of Smith's ideas about the relationship connecting division of labour (and hence productivity) to the size of the market, or of Ricardo's ideas (shared by Malthus, Torrens, West) about decreasing returns in agriculture.

6 In this connection it is worth pointing out that Sraffa himself refers to part III of his book, dedicated to the 'switch in methods of production', as an exception with regard to the absence of any hypothesis on returns. Here we must, in fact, consider changes–albeit only notional–'in the proportions in which different means of production are used by an industry' (Sraffa 1960a:v). However, essential as it is for criticism of the traditional marginalist theory of value and distribution, this part is of minor utility for understanding of the phenomena of technological change. To this end it is more useful to adopt a dynamic-evolutionary approach, as the Classical economists did from Smith's theory of the development of the division of labour to Babbage's of 1832, and of the links between division of labour and mechanisation. See Corsi (1991).

7 Smith, who insisted on this point in the *Wealth of Nations,* came up against for severe criticism from Pownall (1776: see Roncaglia 1995a). According to the marginalist conception (and Pownall might be considered a precursor of it from this viewpoint), by contrast, the division of labour arises from differences in the abilities of the various workers.

8 Analysis of the division of labour can be carried out from various viewpoints: for example, the distinction between the horizontal and vertical division of labour is relevant to analysis of the link between technological change and evolution in the social structure. Moreover, the microeconomic division of labour (or organisational division of labour) is itself a source of the macroeconomic division of labour: consider the case of certain areas of activity externalised by firms, giving rise to new firms. On these points see Corsi (1991).

9 Strictly speaking, this applies to a closed economic system. For an economy open to foreign trade, we might see exchange between domestic and foreign commodities as an additional production process, with a procedure much like the 'closure' of input-output tables.

10 Let us remember that product, total means of production and surplus are all vectors. The distribution of the surplus (between social classes and between

sectors) occurs in terms of value, and is thus related to the determination of exchange ratios.

11 The labour theory of value in this respect (disregarding, therefore, its 'metaphysical' aspect–labour as 'cause' or 'substance' of value) is merely a simple way of expressing the relative difficulty in the production of a commodity using a unidimensional variable. However, the second condition for reproduction (uniformity of rate of profit in the various sectors) calls for a multidimensional description of the 'production difficulty'.

12 Actually, the very idea of originary factors of production needs closely looking into. In fact, 'land' normally requires substantial investment before it can be used in the production process, but it cannot be considered scarce in absolute terms; as regards 'labour', we must bear in mind both the importance of professional training in contemporary economies and a whole range of elements (from customary practices and legal norms to the existence of social services such as kindergartens) determining both rates of activity (especially for women) and migratory flows. After long debate it has been concluded that it is erroneous to consider 'capital' an originary factor of production; as for 'entrepreneurial' qualities, their presence (and degree) are defined *ex post* on the basis of the economic results of the firms; hence they cannot be inserted in production functions representing alternatives between which the producer can choose.

13 In a certain sense the exact delimitation of a problem corresponds to its solution. Such is the case, for example, with the Ricardian problem of the invariable standard of value; for Ricardo, the standard of value must be unvarying with respect to changes in both technology and the distribution of income. However, set in these terms, the problem remains insoluble. With his analysis of the 'standard commodity' Sraffa delimits the problem, restricting the focus to changes in distribution and singling out a commodity that does not vary in terms of its means of production, since these are nothing but a certain quantity of the same commodity. (It is therefore a mistake to say that Sraffa 'solves' the original Ricardian problem of the invariable standard of value.) See Roncaglia (1978: Ch. IV).

14 Moreover, in the general case where fixed capital goods are present, the technology employed as given for the determination of prices corresponds to what is considered a normal degree of utilisation of plant; it is in fact to this specification of technology that firms make reference for decisions on prices. On this point, and on the concept of 'socially necessary' technique, see Roncaglia 1978:27–9, 1995b. It is a point worth stressing; in Sraffa's analysis it is technology that is taken as directly given (such that one may see technology implicit in the equations as deriving–through a procedure of abstraction–from the technology actually prevalent), while the production levels of the various sectors are taken as 'indirectly' given, being–in the absence of hypotheses on returns to scale–implicit in the technology (so that they do not have as 'direct' empirical correlate the levels of production actually prevailing at a given time).

15 This is the dynamic-evolutionary view that, for example, also includes 'Smith's theorem' according to which the division of labour (and thus the technology) is limited by the extent of the market (i.e. by demand, but in the broad sense, and not as a functional relationship linking quantities in demand with prices and incomes).

16 The method Sraffa follows is in some ways closer to the idea of Marshall (and later Keynes) of focusing on 'short causal chains'. The reason for this is that each link between cause and effect is an abstraction disregarding a great many secondary elements, and it seems likely that the distortions due to disregarded elements can add up in a long chain, leaving any connection between the initial and final terms extremely unreliable. We might say that Sraffa's method consists

in focusing on one link in the chain. Of course, while under this respect there is some analogy in the method between Marshall and Sraffa, there are wide differences in their conceptions of the way the economy functions. Let us recall that Marshall employs the concept of equilibrium between demand and supply, and thus evidently conceives partial equilibrium analysis (of the firm or the industry) as a segment of general economic equilibrium analysis.

17 See Samuelson (1987:458–9). Actually, the criticisms launched by Sraffa in the articles of 1925 and 1926 are far more radical, regarding the very foundations of analyses based on functional relationships between cost and quantities produced and the hypothesis on the convexity of production functions. See Roncaglia (1978:10ff. and 104ff.).

18 In reality the 'Cambridge' criticisms concerned the aggregate concept of capital only initially (Robinson 1953), but subsequent to the publication of Sraffa's book (and of Garegnani 1960) the emphasis shifted to the concept of capital as a 'factor of production' (and, correlatively, of profit as the price for the service of this factor of production).

19 For lengthier exposition, see Roncaglia (1978: Ch. VII).

20 1. The world is everything that is the case….
1.2 The world divides into facts….
3. The logical picture of the facts is the thought….
4. The thought is the significant proposition….
4.26 The specification of all true elementary propositions describes the world completely….
7. Whereof one cannot speak, thereof one must be silent.

(Wittgenstein 1922:31, 43, 61, 91, 189)

21 As we have seen, in the marginalist tradition the concept of equilibrium derives from physics, and more precisely from Classical mechanics, reference to conditions of equilibrium implying static analysis. By contrast, reference to the dichotomy between static and dynamic analysis appears inappropriate in terms of the Classical approach; see Roncaglia (1978:119).

Some post-Keynesian economists (Kaldor 1972) have argued that the concept of equilibrium is to be rejected *in toto,* given the frequent occurrence of increasing returns in the economy: there are good reasons for this idea if reference is to the notion of equilibrium imported from classical mechanics into marginalist theory, but there is some exaggeration if rejection also involves the competitive hypothesis of a uniform rate of profits in the various sectors of the economy, as employed by Classical economists and Sraffa.

22 For example, it would indeed be difficult to attempt such a comparison between Sraffa's analysis of prices and Harrod's analysis of the warranted growth rate.

23 For an attempt along these lines, see Roncaglia (1995b).

24 In the presence of savings and financial circuits, 'Say's Law' (in the interpretation now dominant, as a proposition regarding macroeconomic equilibrium) implies that the rate of interest is determined by the equilibrium between demand and supply of loanable funds, and thus implies the unicity of real equilibrium, also for distribution variables, in contrast with one of the mainstays in Sraffa's analysis.

25 Again, a notion that has absolutely nothing to do with Classical political economy is that of 'normal long period positions' of the economy employed, for example, by Garegnani (1988). See Roncaglia (1990b), where Smith's concept of natural price is discussed

26 Actually, the problem of the relationship between quantities produced and quantities in demand–the problem of realisation–simply does not arise in Sraffa's 1960 field of analysis.

27 On the 'shifting' of the post-Ricardian Classical economists in this direction–attributing market prices with the status of theoretical variable–see Bharadwaj (1978); the main references are to the late writings of De Quincey and John Stuart Mill. Smith's 'compromise', on the other hand, consisted in isolating the natural price as a theoretical concept, relegating the role of demand and supply to influences on the market price, although no theoretical analysis is made of how the latter is determined; see Roncaglia (1990b). However, as noted above (§3), in the subjective theory of value, demand and supply (scarcity and utility) are the key factors in the price determination mechanism.

28 On the basis of *Sraffa Papers* housed in Trinity College, Cambridge, Ranchetti (1998), offers new information on Sraffa's attititude to Keynes' theory. In fact, Sraffa's criticisms of Keynes' theory of liquidity preference seem to be looking for a greater degree of radicalism in Keynes' reversal of the traditional marginalist thesis of the 'real' determination of the natural interest rate. Sraffa's criticisms concern both the direction of the causal link (not from the 'quantity of money' to the interest rate but vice versa, with an endogenous theory of the supply of money much like the one subsequently developed by various post-Keynesians) and the attempt to express the demand for money for speculative purposes as a decreasing function of the interest rate defined in a sufficiently univocal way (although Keynes makes the attempt with far more caution than the 'Keynesian' manuals suggest, given the role he attributes to expectations and their extreme variability). Sraffa also seems to be looking for a greater degree of radicalism when criticising the confusion Keynes ran into between own rates of interest and the marginal efficiency of capital goods in Ch. XVII of the *General Theory*.

29 See Roncaglia (1993) for an indication of the lines along which to develop an analysis of income distribution conceptually compatible with Sraffa's prices-distribution link. Alternative suggestions based on the link between interest rate and rate of profits are offered by Panico (1988b), and Pivetti (1991).

30 'When in 1928 Lord Keynes read a draft of the opening propositions of this paper, he recommended that, if constant returns were *not* to be assumed, an emphatic warning to that effect should be given' (Sraffa 1960a:vi).

31 On the relationship between Sraffa's 1932 article and the chapter in Keynes (1936), see Kregel (1983) and Tonveronachi (1991). More generally, for interpretations of Keynes oriented in the direction suggested here, see Kregel (1976) and Tonveronachi (1983).

Comments

13 A comment on Salanti and Signorino

Marina Bianchi

Despite the fact that Sraffa's theoretical production has been mainly a critical assesment of economic theories, few attempts have been made to uncover and reconstruct its hidden methodological premises. The authors attempt to fill this gap and their effort is welcome.

The aim of their chapter is twofold. First, they wish to clarify the methodological criteria Sraffa was following when evaluating alternative economic theories. Second, they want to answer the question whether *Production of Commodities by Means of Commodities* does or does not follow *these* methodological premises.

In seeking Sraffa's methodological criteria the authors analyse closely his published and unpublished papers of the 1920s and early 1930s. They define their task as being to ascertain whether Sraffa accepted a trade-off between the logical coherence and empirical relevance of a theory and, consequently, what methodological status he assigned to the *ceteris paribus* assumption.

The conclusion reached in the chapter is that Sraffa invariably found great fault with theories which sacrificed empirical relevance for logical self-consistency, and conversely, those which gained realism at the expenses of logical coherence. This trade-off, rejected by Sraffa, is, on the contrary, often tacitly assumed by some of the very defenders of Sraffa's approach. The authors refer for example to the debate between Mark Blaug and Ian Steedman in the early 1990s on the problem of capital reversal. In this discussion, while the former maintains that the final test of the validity of a theory is its empirical content, the latter stresses the importance of its logical soundness. Both, however, seem to agree on the admissibiiity of the trade-off. Additionally, and importantly, in judging the explanatory power of a theory, Sraffa insisted on the realism of the assumptions, the realism of the conclusions being not a sufficient condition. It may happen, in fact, that if some conclusions seem to be in accord with facts, and the reasoning is incorrect, we can take as realistically sound premises which are in fact not. Sraffa then–this is the authors' main and original analytical point–was a sustainer of an aggressive methodological view, one which valued both the logical and empirical dimensions of a theory.

The 'proof of this argument is based on Sraffa's radical critique of Marshall's theory of value. After having reconstructed both the implicit and explicit premises of Marshall's partial equilibrium model, Sraffa deals with its empirical implications. In so doing he has to solve the ambiguous methodological status of the exogenous variables and the *ceteris paribus* assumption (CPA). As the authors explain, this CPA may be given three different interpretations. In the language of Musgrave, the domain assumption (DA) implies that a theory T applies only if CPA is true. The negligibility assumption (NA) says that any assumption different from the CPA in T is negligible. The heuristic assumption (HA) says that CPA is not a matter of truth but of usefulness. Sraffa's position on the CPA of Marshall's model (the authors argue) is that it cannot be interpreted either as HA or NA but as DA. If this is the case, however, the explanatory power of Marshall's theory is significantly reduced, since it applies only in the very restrained domain of industries that use their available specific factors completely and enjoy no external economies. Sraffa, then, does not limit his criticism to possible violations of logical coherence but uses these to unveil the poverty of the theory for generating empirical results.

On the basis of this analysis the authors address the second question: how to judge the methodological status of Sraffa's 1960 book. Since there is no evidence of a change in Sraffa's way of evaluating theories we must assume, they argue, that the same aggressive methodological approach just discussed applies also to *Production of Commodities by Means of Commodities*. Consequently, a key question becomes: how to interpret Sraffa's CPA of given quantities of produced commodities.

The answer given in the chapter to this important question is much less rich and articulated, and shorter than the one given to the first. What the authors seem to suggest is that Sraffa is giving a first approximation solution to the problem of value and distribution, a solution that is to be further completed by specific, problem-related analysis. Their argument is the following. Sraffa did not intend to contrapose to the neoclassical approach another all-encompassing theory which, from few premises, defined what was to be regarded as the economic domain and what analytical tools should be used to explore it. Rather, his theory was intended to be just one piece of a larger coherent framework in which many theories from different disciplines would have concurred, each defining and solving specific problem(s).

One might warmly agree with this method of 'piecemeal' theorising advocated by the two authors. But they fail to indicate precisely, nor do they explain and analyse, what piece of the framework Sraffa's book contributes. Is it, for example, a positive theory of value? From their earlier analysis which, as we have seen, rejects a purely logical interpretation of Sraffa's theory, the answer would seem to be yes. But in this case they do not explain why the DA should not apply to it, why in other words the exogeneity and givenness of the initial quantities of commodities should not restrict its empirical domain.

Alternatively, the authors might have argued that the aim of *Production of Commodities* was mainly to give a logically coherent reformulation of the theory of value that was consistent with the Classical approach, but fatal to the neoclassical one which was based on the possibility that supply and demand functions can be separately determined. In other words Sraffa was making a methodological point not different from that made in the papers of the 1920s, the years in which the ideas contained in *Production of Commodities* were conceived. As Fabio Ranchetti notes in his 'Introduction' to the recent Italian republication of *Production of Commodities,* Sraffa, when commenting on Gramsci's interpretation of Marx's law of the falling rate of profit, defines it as a purely methodological, not as an historical, law, since determinative forces such as technological progress and innovation were not considered. In this alternative, if Sraffa's positive contribution to economic theory must be considered to be that of the classics, only freed from their clumsy theory of value, the considerations connected to the domain assumption should not apply to Sraffa's determination of prices and distribution but to the contribution of the classics. But if this is the case new questions arise. What parts of the composite and varied Classical approach are elements of the alternative Sraffa was envisaging and which do the authors think satisfy his methodological restrictions? How do they combine with the contribution that might come from other theories and other disciplinary fields? What has been already achieved towards this goal? The authors do not say. But, in the name of that piecemeal theorising they are defending they might very soon provide us with another piece exploring this second important problem with the same richness of analysis they have devoted to the first.

14 A comment on Rosselli

Pier Luigi Porta

I shall divide my brief comment into two lilliputian sections: the former is on the points of agreement with Annalisa Rosselli, the latter on queries and possible disagreements. Let us start with the former, since that is the longer of the two and also because, of course, it is nicer to do so.

Points of agreement

I must confess feeling so very much at home from the very start on reading Rosselli's chapter that my first comment is immediately enthusiastic. It is, to say the least, unusual to come across a chapter, in the vast Sraffian literature, which puts the emphasis on what *Ricardo* owes to Sraffa, rather than the other way round. That must be rated among Rosselli's merits.

The acknowledgement that proper editing first is *creative* and, at the same time, second, is *not intrusive* (which may appear at first sight to mate two oxymoranic characters resulting in a paradox) is the pillar on which some of my own recent papers are founded.[1] As a general policy one should perhaps refrain from quoting one's own work; in the present instance, however, I must confess pleasure in so doing: Rosselli gives me a precious opportunity for she clearly builds on the very same foundations as I have done for quite a few years. It is not uncommon that new ideas sometimes soon come to 'fill the air' and turn rapidly (and happily) into public goods. If that is good news in this case (as I believe it is) I sincerely enjoy offering it to Rosselli herself and to the world: indeed, we are so few to move on that ground that we are at a loss to spot each other. When that happens, one may perhaps be forgiven for happily singing: 'Non son solo, siamo in due!', as Rodolfo does in a famous duet of Puccini's *La Bohème*.

Let me summarise the approach with a minimum of words.

Pure philology is a rhetorical construct; it does not exist in practice. That means that editing is inevitably interpreting. What both Rosselli and myself find interesting is the application of such principles to Sraffa's case. The case is indeed puzzling: so effective is the rhetorical construct in Sraffa's case, that–Rosselli notes–for quite some time the innovative interpretative content

of Sraffa's Introduction (and indeed, I should add, of the whole arrangement of the edition) escaped the attention of all commentators entirely.[2] I should only insist, what Rosselli does not do unfortunately, that Sraffa's extraordinary rhetorical effectiveness was at the root of the myth of the 'definitive interpretation' of Ricardo.[3] The idea of a 'definitive interpretation' is a very dangerous resistant virus: it embodies an aim at establishing orthodoxy. That is something, alas, still besetting us today, and not merely on Ricardian matters. In the curious debate on the use to be made or not to be made of Sraffa's literary remains, the problem evidently surfaces. The arrangements to which Sraffa's literary remains have been subjected only admit of one possible explanation: that their use in scientific work should be made free only *after* one particular line of interpretation of them collectively has been established and weaved into the general opinion.

That Sraffa's case, i.e. the question of reconstructing and interpreting Sraffa's intellectual and scientific experience, is far from closed today is a point that, again, associates me with Rosselli, who lists some of the open questions with admirable clarity. These can be summarised as follows (insofar as Sraffa's work on Ricardo is concerned): the spell of Marx; the parallel experience of the theorist and of the historian-of-analysis in Sraffa and the reasons for the selection and sequence of the arguments in Sraffa's masterpiece, the famous Introduction to volume I of the Ricardo edition.

Points of difference

I am puzzled that Rosselli does not make full use of the hermeneutic potential of her approach by criticising any pretence of a *definitive* interpretation. In my own view, that is a general important point on which Sraffa's case provides a model. Surprisingly (after what we have seen in the above), Rosselli concludes her chapter with observations of the following tenor: what a poor thing would be Ricardo without Sraffa! To my mind, that means that she is still to a large degree under the spell of the definitive-interpretation approach and therefore she appears unable to do full justice to what Ricardo actually owes to Sraffa. Of course, that Sraffa has changed the life for Ricardo and for us as (conscious or unconscious) Ricardians is, no doubt, true. If that indeed is true, at the same time that can*not* be the end of the story: thus it is a pity to put a 'full-stop' after that truth. That seems to me a sensible observation to be made, particularly today, when Ricardo *does* seem a poor thing anyway, due to a decline in attention, the causes of which are still not entirely clear. On the whole it would seem fairer to admit that great things, like Ricardo's spell and influence (or, indeed, for that matter, their decline), never depend on a single cause. That this is so does not diminish Sraffa's role in the least in this case: on the contrary, it allows us to value his contribution more fully.

On matters of detail I limit my comments to one point. Concerning Ricardo's *Notes on Malthus's Measure of Value,* notoriously published separately

as a pamphlet a few years ago, Rosselli quotes the recent discussion between Giancarlo de Vivo and myself in the *Cambridge Journal of Economics*. She observes, in particular:

> It is worth noting, in the light of recent discussion, that Sraffa's original idea had probably been to publish also Ricardo's notes on the 'Measure of Value' by Malthus, which had come to light among the Mill-Ricardo papers and which were eventually left out of publication. In a note in Dobb's handwriting with an index for volumes 3 and 4, Ricardo's notes to the book by Malthus are included as part of volume 4'.
>
> (Rosselli, p. 191)

Although that note in Dobb's hand cannot be dated, I think that Rosselli's reconstruction is entirely plausible. The only shade of difference here is that she is entirely neutral as to the reasons *why* that piece was *missed out* of publication: I cannot, in fact, reconcile myself to the idea that an overscrupulous editor as Sraffa was could *deliberately* have 'left out' the piece from the edition. Indeed Rosselli does not add the adverb thus perhaps leaving it open to interpret her phrase. Personally, I hate staying 'neutral' in this case.

Notes

1 See my paper 'Piero Sraffa, "Superb Editor"' (Porta 1992) and my forthcoming contribution to the Hollander *Festschrift* to be published by Routledge in 2000 (Porta 2000).
2 My only shade of difference, here, with Rosselli is that Dobb, as quoted by her (p. 192), must rated as an 'inside trader' rather than a commentator or an early reviewer.
3 On the issue, let me refer to my forthcoming paper (Porta 2000).

15 A comment on Roncaglia

Giorgio Lunghini

In this comment on A.Roncaglia's chapter I will touch on three points: the given quantities assumption and the role of consumer demand in *Production of Commodities by Means of Commodities;* the relationship between Sraffa and Wittgenstein; the relationship between Sraffa and Keynes.

The given quantities assumption and the role of consumer demand

The given quantities assumption, and the related question of the role of consumer demand, was one among the many surprising features of *Production of Commodities by Means of Commodities. Prelude to a Critique of Economic Theory*. As R.Harrod wrote in his 1961 review:

> Perhaps the most remarkable feature of the book is that, while the determination of prices is one of its central topics, no reference is made to the scale or elasticity of the demand for end-products. (The word 'demand' does not occur in the index.) It is surprising that one can get a system of price determination without reference to final demand. It might be thought that this was simply a reversion to early classical tradition, such as might come naturally from so profound a student of Ricardo…. I cannot find anything in this more elaborate account of price determination that justifies ignoring the influence of the commodity-mix that consumers wish to have…. It is to be hoped, however, that [Mr. Sraffa] may proceed to set out the interconnections of his system with the traditional system, rather than treat his system as the 'prelude to a critique of economic theory'. Surely what is true in the two systems can have a peaceful coexistence.
>
> (Harrod 1961:783–7)

Sraffa's comment (1962) is strictly analytical. As to Sir Roy's belief, 'that the system presented must be indeterminate because it fails to take into account the composition of consumer demand', Sraffa points out that 'this is clearly a misunderstanding, since the exchange ratios are, of course, determined by

the equations of production and not by the ratios between the excess production of the commodities'. Hence Harrod's conclusion, that a change in the composition of consumer demand 'would at once, *in accordance with Mr. Sraffa's own equations,* affect the price ratios', is false. Beyond correcting this logical flaw, Sraffa significantly keeps silent about the fact that the word 'demand' does not occur in the index of his book, and hands over the explanation of this occurrence to the exegetes. Neither does Sraffa fulfill Harrod's hope that he may proceed to set out the interconnections between his system and the traditional system, with a view to a peaceful co-existence. This is a silent way to suggest that the real title of *Production of Commodities by Means of Commodities* is the sub-title: *Prelude to a Critique of Economic Theory*. (The same is true for *Das Kapital*.)

Now I would like to recall the explanation of the given quantities assumption and of the role of consumer demand in *Production of Commodities by Means of Commodities* which was put forward by Claudio Napoleoni (1992); then I will propose my own conjecture about this issue (Lunghini 1996). Napoleoni's argument is as simple as it is radical:

> [In Sraffa's model] the problem of determining the quantities produced does not exist…. This means that, for Sraffa, determining the quantities produced is not a problem that belongs within the economic sphere. On this point, Sraffa must be credited for his consistency, for this idea is the only one really compatible with a distribution theory based on the type of surplus value adopted by him. If we look closer, the problem of determining the quantity of commodities only makes sense when we presuppose the existence of an economic system whose subjects are free individuals who base their choices on their desire to satisfy their needs in the best possible way. In other words we need to consider consumption as a real and autonomous component of the economic system, not just a subordinate aspect of it, simply a moment of the production process. It is clear, in fact, that in order for production to have any meaning, it needs to be related in some way to human requirements, and thus to the dimension of consumption. A distribution theory like Sraffa's presents us with the idea of an economy in which consumption in the real sense of the word, that is, consumption based on the satisfaction of freely expressed needs, cannot be taken into consideration.
>
> (Napoleoni 1992:251–64)

In the same essay Napoleoni added that 'Sraffa's work runs the risk of remaining largely incomprehensible from the point of view of its real importance, if it is not carefully placed in the context of the history of economic thought'. Sraffa himself, on this point, is explicit: his standpoint is that of the Classical economists from Adam Smith to Ricardo. My conjecture is that the exact position of *Production of Commodities by Means of Commodities* in

the conceptual coordinates of Classical political economy (after Ricardo's *Absolute Value and Exchangeable Value,* and before Marx) might be traced out starting from a passage of *Production of Commodities by Means of Commodities* (Chapter III, on 'Proportions of Labour to Means of Production'): 'The key to the movement of relative prices consequent upon a change in the wage lies in the inequality of the proportions in which labour and means of productions are employed in the various industries.' This is the same 'segreto'[1] investigated by Marx in Chapter 50, point 2, of *Das Kapital* Book III on the illusions created by competition:

> We have seen that a general rise or fall in wages, by causing a movement of the general rate of profit in the opposite direction–other circumstances remaining the same–changes the prices of production of the various commodities, i.e. raises some and lowers others, depending on the average composition of capital in the respective spheres of production. Thus experience shows here that in some spheres of production, at any rate, the average price of a commodity rises because wages have fallen. But 'experience' does not show that the value of commodities, which is independent of wages, secretely regulates these changes. …'Experience' shows that wages determine the prices of commodities. But "experience" does not show the hidden cause of this interrelation.
>
> (Marx 1985:875–6)

It is unlikely that in a Sraffa's writing a hidden coincidence comes out unintentionally. The analytical point is the same, and one could argue that *Production of Commodities by Means of Commodities* is a Ricardian gloss to *Das Kapital* as to the relationship between a change in the wage and the movement of relative prices, *other circumstances remaining the same:* including the scale of production and the proportions in which the various means of production are employed in each industry. For Sraffa, the given quantities assumption is expedient to an exact solution of the ricardian problem evoked by Marx.

Sraffa and Wittgenstein

Sraffa's influence on Wittgenstein (rather than that of Ramsey) is acknowledged by Wittgenstein himself. Wittgenstein's change of mind from the *Tractatus Logico-Philosophicus* to the *Philosophische Untersuchungen* is admittedly stimulated by Sraffa's critique.

Yet *Production of Commodities by Means of Commodities* and Wittgenstein's *Tractatus* exhibit some significant concordances, and there is no reason why Sraffa should not have modelled the epistemology of *Production of Commodities by Means of Commodities* upon the *Tractatus* rather than on the *Untersuchungen* on whose development he was so influential (Lunghini 1975). Keeping himself to Wittgenstein's summary of the *Tractatus* ('Its whole meaning could be

summed up somewhat as follows: What can be said at all can be said clearly; and whereof one cannot speak thereof one must be silent' [Wittgenstein 1922:27]), Sraffa emends both the grammar and the logic of the Classical political economy. How happy Ricardo and Marx (Marx's Ricardian *côté*) would have been for this achievement!

Sraffa and Keynes

With regards to the relationship between Sraffa and Keynes, I will spend a few words on the determinants of the rate of profit. According to Sraffa, the rate of profit is not determined in the sphere of production (as it is for Marx, whereas in *Production of Commodities by Means of Commodities* the unresolvable 'transformation problem' of the origin of value is simply erased).[2] On the contrary, the rate of profit 'is susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest'.[3] Sraffa obviously refers here to Keynes' *General Theory of Employment, Interest and Money,* Chapter 17, where Keynes attributes to the money-rate of interest, rather than to the own-rates of interest, 'the predominating practical importance' in determining the volume of output and employment. However, this does not suffice, in my opinion, for maintaining that Sraffa's analysis and Keynes' theory both 'refer to a largely shared conceptual framework'.

A radical critique of orthodox economic theory is the real common goal pursued by Sraffa and by Keynes, albeit with different strategies. The *General Theory of Employment, Interest and Money* reveals the effective determinants of the level of employment. *Production of Commodities by Means of Commodities* denies the effective universality of the marginalist theory of value and distribution. It is a pity, and an interesting question for the historians of economic thought, that these critiques did not achieve the same success as such a theoretical power would have granted them in other disciplines. Against the neoclassical thesis of the harmony of interests in capitalism, Sraffa gives us a definitive proof of the existence of an inner conflict between wages and profit. Keynes, on the other hand, convincingly argues that in an *entrepreneur economy* unemployment is our normal lot. Keynes' *General Theory* and Sraffa's *Prelude to a Critique of Economic Theory* have therefore provided the basis for a critical analysis of contemporary capitalism. As a matter of fact, the *General Theory* has been buried in the so-called 'neoclassical synthesis' and *Production of Commodities* normally is not even mentioned in textbooks.

Notes

1 See the Italian edition of *Production of Commodities: Produzione di merci a mezzo di merci. Premesse a una critica della teoria economica,* p. 16.

2 About the 'unresolvable problems', see L.Wittgenstein (1922):(4.003) 'And so it is not to be wondered at that the deepest problems are really *no* problem.'; (6.21)

'Mathematical propositions express no thoughts.'; (6.5) 'The *riddle* does not exist. If a question can be put at all, then it *can* also be answered.'

3 A relationship between the rate of profits and the rate of interest is also put forward by Smith and Ricardo. Ricardo (in Chapter 21 of his *Principles of Political Economy and Taxation* on the 'Effects of Accumulation on Profits and Interest') quotes Smith and reverses the causal order: Adam Smith has justly observed, that it is extremely difficult to determine the rate of profits of stock. "Profit is so fluctuating, that even in a particular trade, and much more in trades in general, it would be difficult to state the average rate of it. To judge of what it may have been formerly, or in remote periods of time, with any degree of precision must be altogether impossible." Yet since it is evident that much will be given for the use of money, when much can be made by it, he suggests that "the market rate of interest will lead us to form some notion of the rate of profits, and the history of the progress of interest afford us that of the progress of profits". Undoubtedly if the market rate of interest could be accurately known for any considerable period, we should have a tolerably correct criterion, by which to estimate the progress of profits.' See *The Works and Correspondence of David Ricardo,* I:296.

Part IV

Specific topics

16 *Production of Commodities by Means of Commodities* and the open economy[1]

Ian Steedman

It is a simple descriptive truth about real capitalist economies that they are open economies, open both to foreign trade and, in many cases at least, to international flows of both short and long term money capital. Moreover, such openness has been increasing, in some cases quite markedly. Hence to claim that one is studying real capitalist economies whilst analysing models of the closed economy is self-contradictory, at least from the descriptive perspective. Of course it might be, in principle, that closed economy theory nevertheless turned out to be very similar to open economy theory–but one could only establish this, if true, by conducting both analyses and then comparing them. Being in possession of the open economy analysis, then, one must use it if it differs significantly from the closed economy version and might as well use it even if it does not. It is against this background that we here consider Piero Sraffa's great *Production of Commodities by Means of Commodities,* which contains no overt reference to the open economy. One simple and reasonable explanation for this silence may perhaps be found in Sraffa's clear prefatory statement that 'the set of propositions now published [have] been designed to serve as the basis for a critique of [the marginal theory of value and distribution]' (Sraffa 1960a:vi). Since the marginal theory was presented by Böhm-Bawerk, Clark, Wicksell and many others in a closed economy setting, it was entirely appropriate to lay the foundations for a critique of that theory in the same terms. Indeed it would have been highly inappropriate to do otherwise; one can hardly base an internal logical criticism of a theory on alternative assumptions!

It does not follow, however, that there is nothing further to be said concerning *Production of Commodities* and the open economy–and this for two kinds of reason. In the first place, it might be wondered whether 'the marginal theory of value and distribution' could not be presented in a open economy setting and whether it might not then be less vulnerable to the Sraffian critique. After all, capital theoretic problems repeatedly flow from the dependence of relative commodity prices on distribution, whilst trade theorists often take many relative commodity prices to be fixed; could this disarm the Sraffian critique? Second (and quite separately), since *Production of*

Commodities by Means of Commodities is sometimes taken to underlie not only a critique of marginal theory but also a positive alternative to that theory, it may be of interest to consider how such concepts as basic and standard commodities, the Chapter XII analysis of switches in production methods, and so on, fare in the context of an open economy.

Our discussion will be set out as follows. Section 1 will consider some simple capital theory in the setting of a small open economy, in which not all commodities are tradeable, and show how familiar criticisms of marginal theory do or do not apply equally well in that setting. Section 2 will then be devoted to asking how the small open economy framework impinges on various well-known Sraffian concepts and arguments. And Section 3 will turn, albeit briefly, to consider open economy analysis without the 'small economy' assumption. To simplify matters we shall follow Sraffa's hint that, 'If such a supposition [of constant returns in all industries] is found helpful, there is no harm in the reader's adopting it as a temporary working hypothesis' (Sraffa 1960a:v)–but we shall here adopt it throughout. Similarly, taking note of Sraffa's remark that, 'Whilst the central propositions had taken shape in the later 1920's, particular points, such as the standard commodity, joint products and fixed capital, were worked out in the thirties and early forties' (ibid: vi) we shall ignore the 'particular points' concerning joint production and fixed capital and (almost) set aside Part II of Sraffa's book; we shall however refer to the standard commodity. To simplify yet further, we shall make much use of the familiar capital-theory-workhorse in which a given type of machine can be used, with homogeneous labour, to produce either a consumption commodity or new machines of the same kind; we shall thereby be able to avoid the use of much matrix notation, with little loss of generality in the nature of our conclusions.

1 Marginal theory in the small open economy

It is perhaps important to notice first, even if we need not dwell long on the point, that if *all* produced commodities were tradeable at fixed relative prices then *any* operated production process would immediately yield a linear frontier relating a uniform (ex post) real wage rate (however measured) to a uniform rate of profit. (Unless otherwise noted, we shall always take wage and profit rates to be uniform.) Moreover, such a frontier would always have a slope equal to (minus) the capital-labour ratio and an intercept on the rate of profit axis equal to the output-capital ratio. No matter how many alternative production methods and/or production specialisations were open to the capitalists, capital and distribution theory would be entirely straightforward from a marginalist point of view and would exhibit all the features of the familiar 'surrogate production function' economy.

Somewhat more pointedly, the above argument does not really require that all produced commodities be tradeable but, rather, that all produced means of production be so, together with one (or more) consumption

commodities in terms of which it is appropriate to measure the real wage rate. There could be any number of other, non-tradeable pure consumption commodities without the above reasoning being affected in the least. But one could now say, in addition, that the relative prices of domestically produced, non-tradeable consumer goods would depend on distribution in a very simple way; at a higher rate of profit, the relative price of a more capital intensive commodity would *always* be higher. Of course, none of this can be in the least surprising to anyone who fully grasps the root of many capital theoretic problems; the dependence on distribution of the relative prices of produced inputs, a dependence which has here been removed by assumption. So far as I know, no 'defence' of marginalist theory along these lines was put forward during the debates of the 1960s and 1970s.

Let us now enter into rather more detail by considering, first, an economy producing both a tradeable machine and a unique, non-tradeable consumption commodity and then, second, an economy producing both a tradeable consumption commodity and a non-tradeable machine. At this stage, we do not ask why these particular patterns of production and trade obtain but merely suppose that they do; of course we must later abandon this supposition.

1.1 A tradeable machine

Consider an economy in which the production of one machine requires the use of a such machines, b units of labour *and* a (column) vector m of non-competing, imported means of production: production of one unit of the non-tradeable consumption commodity requires the use of a machines, ß units of labour and a (column) vector μ of the non-competing imports. These latter have fixed foreign currency prices given by the (row) vector. Then with ex-post wages,

$$p = bw + (1+r)\left[ap + e(fm)\right] \tag{1}$$

$$\pi = \beta w + (1+r)\left[\alpha p + e(f\mu)\right] \tag{2}$$

where the notation is standard, except that e is the nominal exchange rate. In addition, of course, if f_* is the foreign currency price of the machine then

$$p = ef_* \tag{3}$$

Now the analysis of (1)–(3) is straightforward and even, in effect, very familiar if we define $a^+ \equiv [a + (fm/f_*)]$ and $\alpha^+ \equiv [a + (f\mu/f_*)]$. For then:

$$b(w/p) = 1 - a^{+}(1+r) \tag{4}$$

$$b(\pi/p) = \beta + (\alpha^{+}b - a^{+}\beta)(1+r) \tag{5}$$

and

$$\frac{w}{\pi} = \frac{1 - a^{+}(1+r)}{\beta + (\alpha^{+}b - a^{+}\beta)(1+r)} \tag{6}$$

In formal terms, (4)–(6) are no different from the corresponding closed economy results. But it may be noted explicitly that the rate of profit corresponding to w=0 is now given by

$$1 + R = \frac{f^{*}}{af_{*} + (fm)} < \frac{1}{a}$$

(unless fm=0) and that the condition for (6) to be linear is now that

$$[\alpha f_{*} + (f\mu)]b = [af_{*} + (fm)]\beta \tag{7}$$

If (and only if) it should be true that b/β lies *between* a/α and $fm/f\mu$ then f_{*} could be such as to yield (7)–but this would naturally be a mere fluke. Note that, when (7) does not obtain, π/p varies with r, and (6) is non-linear, even though so many relative prices are exogenously given. And to conclude this discussion of (1), (2) and (3) it may be observed that the real exchange rate, expressed for example in labour-commanded terms as e/w, is an endogenous variable depending on r; the same is true in sub-section 1.2 below.

If, now, we suppose there to be alternative tradeable machines each of which can produce either itself or the non-tradeable consumption commodity, there will be alternative versions of (6) and the pattern of specialisation will, in general, depend on distribution. And since it would be incredible that (7) should hold good for every alternative machine, we can see that marginal theory will face the familiar problems, *even though* so many relative prices are fixed.

1.2 A tradeable consumer good

Suppose now that (1) and (2) hold as before but that (3) must be replaced by

$$\pi = e\phi \tag{3a}$$

because it is the consumption commodity which is now tradeable, *not* the machine; ϕ is of course the given foreign currency price of the export. We now find that:

$$\pi / p = \frac{\beta + (\alpha b - a\beta)(1+r)}{b + \left[(fm/\phi)\beta - (f\mu/\phi)b\right](1+r)} \tag{8}$$

which is a more complicated relation than the corresponding (5). Again π/p will be independent of r if a certain relation obtains between the export price and the conditions of production; a relation which *can* only be satisfied if b/β lies between a/α and $fm/f\mu$ and is, even then, most unlikely to be so. The real wage in terms of the consumption commodity is now given by

$$\frac{w}{\pi} = \frac{1 - \left[a + (f\mu/\phi)\right](1+r) + \left[a(f\mu/\phi) - \alpha(fm/\phi)\right](1+r)^2}{\beta + (\alpha b - a\beta)(1+r)} \tag{9}$$

and (w/p) follows at once, of course, as the ratio between (9) and (8). It is immediately seen that (9) is more complex than (6), the numerator now being a quadratic in r. (Of course (9) will become linear in r when π/p is independent of r but this is, at best, a fluke condition.) From (1), (2) and (3a) it follows that, when w=0, r=R will be positive if and only if

$$\phi > (f\mu) + \frac{\alpha(fm)}{1-a} \tag{10}$$

Condition (10) may be read as: 'The foreign exchange revenue from one unit of the consumption commodity, ϕ, must exceed the direct and indirect foreign exchange cost of one such unit.' Note, for later reference, that the production conditions of the pure consumption commodity, α and $f\mu$, enter into both (10) and, from (9), the determination of R. It is readily shown that the value of p/π at w=0 must lie between a/α and $fm/f\mu$ and that $a(1+R) < 1 > (f\mu/\phi)(1+R)$.

Suppose now that there are two alternative non-tradeable machines, each of which can produce either itself or the exportable consumption commodity. There will be two such frontiers as (9) and switchpoints will, in general, be defined by a cubic in r. There is no apparent reason why this cubic should not yield up to three economically significant switchpoints. Hence with two, or many, such machines it would seem that the familiar Sraffa-inspired capital theory arguments will apply, notwithstanding the presence of many fixed relative prices.

In both sub-sections 1.1 and 1.2 above we have concentrated on the price–distribution relations but it is easy to consider also the physical relations with steady growth and *balanced* trade. In each case it is readily shown that the 'consumption-growth frontier' is the same as the corresponding wage-profit frontier ((6) or (9)). It must be stressed though that such 'duality' does *not* hold with steady growth but *unbalanced* trade. (See Steedman and Metcalfe (1981:136–7), where the point is made in terms of a different model of production.) However, when trade is indeed balanced, all the familiar arguments and constructions concerning $(r\text{-}g)k=c\text{-}w$ and so on can be replicated for the small open economy.

We may thus conclude this section in summary form by saying that the small open economy assumption will enable marginalist theory to proceed smoothly, flukes aside, only if all relevant produced inputs are tradeable and at least one consumption commodity involved in the definition of real wages is tradeable. If no such consumption commodity is tradeable *or* if some produced input used in the production of the real wage is non-tradeable, then familiar difficulties will arise for marginal theory, notwithstanding the assumption that many relative prices of produced commodities are fixed.

2 Some Sraffian concepts and the small open economy

Having seen that the Sraffa-based critique of marginal theory is readily transferred from the closed economy to the small open economy context, we now turn to consider what role various well-known elements of Sraffian analysis may play in the more constructive task of providing a positive analysis of the small open economy. We may begin by working our way through Parts I and III of *Production of Commodities by Means of Commodities*.

Already in §6, 'Basic and non-basic products', we encounter the central concept of a basic commodity. As is well known, it is said that if 'an invention were to reduce by half the quantity of each of the means of production which are required to produce a unit of a [non-basic], the commodity itself would be halved in price, but there would be no further consequences; the price-relations of the other products and the rate of profits would be unaffected' (Sraffa 1960a:7–8). Now reconsider sub-section 1.2 above in which a pure consumption commodity is exported. It is clear from (8) that the price ratio π/p would certainly not be halved, at given r, if α, β and μ were all to be halved; in fact the 'new' π/p would be above or below half of the 'old' π/p depending on the sign of $(fm)\beta\text{-}(f\mu)b)$. Moreover, each price ratio p/f, at given r, would be changed. And from (8) and (9) we see both that the real wage *in terms of the machine* would become a different function of $1+r$ and that the *maximum* profit rate, R, would change; more specifically R would be increased if α, β and μ were all halved. In this case, then, the pure consumption commodity has the properties which only a *basic* commodity is said to have. And in a brief reference to Sraffa's Part II, we may recall his observation in §65, 'Tax on non-

basic product, etc.', that an ad valorem tax on a pure non-basic 'will have no effect beyond the price of the taxed commodity.' (ibid.: 55). But if (3a) is replaced by $(1+t)\pi=e\phi$, it will be obvious from (8) and (9) that π/p, w/π and w/p will all be changed, at given r. Once again, then, the pure consumption commodity behaves as only a basic is said to do. Of course the reason for this is not hard to see; since it is exports of the consumption commodity that pay for the imported inputs referred to in fm and $f\mu$, it is 'as if' the pure consumer good were a necessary input to both industries and thus a 'basic' commodity. (See Steedman and Metcalfe (1981:139–40), for a similar point within a different model of production.) It would seem, then, that in the context of analysing the small open economy the concept of a 'basic commodity' must either be left aside or, at the very least, be significantly modified.

(It may be of interest to pursue a little further the point that the maximum rate of profit, R, may depend on the production conditions of a pure consumption commodity. Noting that (1), (2) and (3a) may be combined to yield

$$(p,\pi)=(b,\beta)w+(1+r)(p,\pi)\left[\frac{a}{(fm/\phi)}\frac{\alpha}{(f\mu/\phi)}\right] \tag{11}$$

we see that $(1+R)^{-1}$ will be the Perron-Frobenius root of the matrix in (11). Hence R is a decreasing function of both α and $f\mu$. It is also, of course, an increasing function of the ratio of the *price* ϕ relative to the *prices f;* more crudely, R rises as the terms of trade improve. We may note also that with $(1+t)\pi=e\phi$ in place of (3a), t being the tax rate, ϕ^{-1} would be replaced by $(1+t)\phi^{-1}$ in (11), so that R would be a decreasing function of the ad valorem tax rate on a pure consumption commodity (cf. Sraffa 1960a:55).)

In §14, 'Values when the whole national income goes to wages', Sraffa notes that when profits are zero 'the relative values of commodities are in proportion…to the quantity of labour which directly and indirectly has gone to produce them' (ibid.: 12). It is, of course, unclear that such a statement can always be appropriate for a small open economy, simply because it is not immediately evident that all the relevant labour quantities can always be properly defined. In the closely related context of the two commodity, two fully-specialised economies picture of trade, Sraffa (1930b) himself had explained clearly the well-known point that the international exchange ratio need not be equal to either country's labour cost ratio.[2] But in fully-specialised economies it may not be possible even to know what the 'labour cost' in the non-existent industries would have been, had they existed. However, if one has price equations such as (1), (2) and (3) or (3a), then one can determine the labour-commanded prices corresponding to zero profits and then *interpret* them as the quantities of (vertically integrated) labour required to produce the commodities. But it is to be noted that if those

quantities are denoted by *(l, λ)* respectively for the machine and the consumption commodity of section I then we shall have

$$l = b + \left[a + \left(fm / f_*\right)\right] l$$

and

$$\lambda = \beta + \left[\alpha + \left(f\mu / f_*\right)\right] l$$

in case 1.1 and

$$l = b + al + \left(fm / \phi\right)\lambda$$

and

$$\lambda = \beta + \alpha l + \left(f\mu / \phi\right)\lambda.$$

in case 1.2. In each case, then, both l and λ *depend on relative prices,* which is not the most familiar understanding of 'labour values'! (See Steedman and Metcalfe (1981:140–1), for this point made in a different setting.)

Sraffa moves on in §15, 'Variety in the proportions of labour to means of production', to argue that, 'The key to the movement of relative prices consequent upon a change in the [rate of profits from zero] lies in the inequality of the proportions in which labour and means of production are employed in the various industries' (Sraffa 1960a:12). This continues to be true in the small open economy setting, subject however to a minor reinterpretation. Thus consider (8) above showing π/p for the case in which the consumption commodity is exported. Clearly, π/p rises as r rises if and only if

$$\left[\left(\alpha / \beta\right) - \left(a / b\right)\right] b > \left[\left(fm / \phi b\right) - \left(f\mu / \phi\beta\right)\right]\beta \tag{12}$$

In (12) one has *two* terms (in the square brackets) referring to inter-industry differences in ratios of 'means of production to labour', that on the left concerning the 'machine-labour ratios' and that on the right the 'imported inputs-labour ratios'. Since it does not appear to be possible to reduce (12) to an inequality involving a *single* type of 'ratio of labour to means of production', Sraffa's quoted statement needs to be interpreted appropriately in order to hold good here. But the 'appropriate interpretation' is by no means a forced or artificial one.

Much of §§23–43 is taken up by Sraffa's concept of the Standard commodity, a particular composite commodity which can only contain 'basic' commodities in positive quantities (see §35, 'Non-basics excluded'). As we have already seen, one cannot at once identify a pure consumption commodity as a 'non-basic' in the small open economy context, so that one should not rush to exclude such a commodity from the Standard commodity. Let us return to our cases in sub-sections 1.1 and 1.2

For case 1.1, with w=0 equations (1) and (2) become

$$p = (1+R)\left[a + (fm/f_*)\right]p$$

$$\pi = (1+R)\left[\alpha + (f\mu/f_*)\right]p$$

Clearly the machine alone constitutes the Standard commodity, while R depends on both the production conditions of the machine (a and m) *and* relative prices f/f_*. For case 1.2 it may be best to return to (11), which makes it obvious that the Standard commodity will be defined by the positive right hand vector of the matrix and will thus contain in positive quantities *both* the machine and the pure consumption commodity. It is clear that the composition of the Standard depends not only on the conditions of production (a, α, m, μ) but also on relative prices f/ϕ. It was already noted above that similar remarks can be made concerning R. Of course, the Standard commodity continues in the present setting to be 'a purely auxiliary construction' (Sraffa 1960a:31) and just as Sraffa reached 'the central propositions' without using that concept (ibid.: vi), so too can the small open economy theorist proceed without it.

Sraffa concludes his (Part I) chapters on the Standard commodity by noting in §44, 'Wage or rate of profits as independent variable', that the 'rate of profits [is] susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest' (ibid.: 33). It need hardly be emphasised that this point continues to be of great significance in economies open not only to trade but also to substantial international flows of both short-term and long-term money capital.

Leaping over both Sraffa's Chapter VI, 'Reduction to dated quantities of labour', and the whole of his Part II, 'Multiple-product industries and fixed capital', we come to the single-chapter Part III, 'Switch in methods of production'. How does the argument of this Chapter XII work in the context of the small open economy? In part, quite straightforwardly—as we have already seen in effect in sub-section 1.1. For it was noted there that if there are alternative non-tradeable machines which can be used to produce the tradeable consumption commodity, familiar Sraffa-like arguments may be deployed to analyse the choice of technique. Even here, however, a question

arises (it also arises for the closed economy analysis in fact) concerning Sraffa's famous statement that, 'No changes in output…are considered' (ibid.: v). For by the very nature of the alternative techniques concerned, the economy's gross outputs *must* be different according as one kind of machine or another is produced and used. Nor would it help to say that 'changes in output' should be taken to refer only to net output, since in a growing economy even net output includes some amount of whichever machine is produced. Is it only the net-net-output, consisting of the consumption commodity, that is to be unchanged? It is, at the very least, not self-evident in the small open economy context that no assumption about returns to scale is required.

More broadly, it must now be acknowledged openly that we have, up to this point, been largely suppressing both the fact that, in an open economy, there is a choice of specialisation pattern and the fact that that choice is in many respects akin to a choice of production method. In his closed economy setting it was of course perfectly proper for Sraffa to have been assuming, before Chapter XII, that 'in a system of single-product industries only one way of producing each commodity is available' (ibid.: 81). In a small open economy, however, any given tradeable commodity may be 'produced' by the process of producing (in the ordinary sense) any *other* tradeable commodity, exporting it and then using the foreign exchange to import the given commodity. (Thus foreign exchange is perfectly malleable capital with respect to tradeable produced inputs.) In this slightly extended sense of 'producing', any one of $\mathcal{N}$ tradeable commodities can be produced by at least $\mathcal{N}$ alternative methods!

Suppose that *all* commodities are tradeable, both a single consumption commodity and any number of produced inputs. We adopt a notation similar to that of Section 1 except that there is now no point in separating out α from μ or a from m. If only the consumption commodity is produced domestically then

$$e\phi = \beta w + (1+r)\, e\,(f\mu)$$

and, with $\pi \equiv e\phi$,

$$1 = \beta(w/\pi) + (1+r)(f\mu/\phi) \tag{13}$$

If only the jth means of production is produced domestically then

$$ef_j = b_j w + (1+r)\, e\,(fm_j)$$

and

$$(f_j/\phi) = b_j(w/\pi) + (1+r)(fm_j/\phi) \tag{14}$$

(The appropriate weak inequalities naturally obtain for all commodities not produced domestically.) The various linear 'w/π versus r' frontiers defined by (13) and (14) can now be compared and, if either w/π or r is given, the pattern of production specialisation be determined. In a stationary economy, at least, every possible specialisation 'produces' the same commodity–the consumption commodity–as the sole net product.

The above argument, based on our constant returns to scale assumption, has been simplicity itself. But what *could* it have meant to say that, here, the analysis could and should be based on 'given outputs'? In the present context, the question of what production method to operate is *indistinguishable* from the question of *what* to produce, at least when there is (in the literal sense) only one available method of production for each commodity. Suppose now, then, that there are (in the literal sense) two available methods for one commodity and just one method for each of the others. Does it thereby become more clear what it could mean to base the analysis of choice of method/choice of specialisation on 'given outputs'? Surely not. Here 'Chapter XII' must become 'Chapter I'!

(Before moving on, we may use (13) and (14) to emphasise a point already made above. If $w=0$ then the various specialisations imply values of $\phi/f\mu$ and fj/fmj for $1+R$; once again the production conditions of the pure consumption commodity are seen to enter into the determination of R.)

Since our discussion is by now broadening out somewhat, we may abandon the device of moving through *Production of Commodities* section by section in order to consider some further issues. In the closed economy and single products context, Sraffa observed that (other than at a switchpoint) there will be one price equation for each produced commodity and that 'the number of [variables] exceeds the number of equations by one and the system can move with one degree of freedom' (1960a:11). In the small open economy (with single products) there will of course still be one equation for each *domestically produced* commodity (but not for each produced commodity, since imported ones have exogenously given world prices–notice, however, that they do not have exogenously given *domestic* prices since, as we have noted above, the exchange rate is endogeneous). When the exchange rate is included as a variable, it has been true in each of the price equation systems considered above that the number of variables has exceeded the number of equations by one. But we should now notice a simple fact about real capitalist economies; they not only export commodities but each one exports *many* commodities. (So-called 'monocrop' economies are not an exception to this rule, first because 'mono-crop' means only that one commodity dominates (not exhausts) the country's exports and, second, because coffee, bananas, petroleum, etc. each come in different qualities, at different times of the year, and so on.) But this fact has been suppressed in each of our above systems, for there was always a single exported commodity. Serious trade theory, however, must account for multiple-export economies, i.e. real world

economies. And such accounting does not come naturally to any constant returns to scale analysis, with a uniform wage rate, a uniform rate of profits and no natural resource limitation to output levels. (Giammanco (1998) has recently considered the effect of scarce resources.) The reason is obvious within the small open economy framework; the presence of two export-industry price equations will suffice to determine relative prices, the exchange rate, the real wage rate and the rate of profits. And the presence of three or more such equations will lead to 'overdetermination'. Indeed, that problem will naturally arise even with just two exports if there is also, say, an internationally given rate of profits.

The 'difficulty' just referred to is not peculiar to a Sraffa-inspired trade analysis, of course, but that hardly means that it can be ignored in such an analysis. It would take us much too far afield to argue for any particular resolution of this 'difficulty' (and in any case the best resolution might well differ from case to case), but merely as an illustration we may consider the following. Let there be a single non-tradeable machine which can produce either itself or *various* tradeable consumption commodities. Take the profit rate to be uniform but the wage rate to be variable. In an obvious extension to case of sub-section 1.2 we may write:

$$p = bw + (1 + r)\,[a\,p + e\,(f\,m)]$$

$$\pi_j = b\omega_j + (1 + r)\,[\alpha_j\,p + e\,(f\,\mu_j)]$$

$$\pi_j = e\phi_j$$

Let there be n exports and set n=1. Then e is determined and hence (π_2,…, π_n) are determined. The remaining equations are (n+1) in number and involve as still-unknown variables $(p, r, w, w_1,\ldots w_n)$. Provided that (10) holds for *each* consumption commodity, there exist alternative positive solutions to our equations–*both* w and r may be taken as given, for example–and the multiplicity of exports is sustainable provided that the appropriate differential wage rates are sustainable. (It will be clear that we are here moving towards the 'factor price equalization' literature.) Whilst we are not anxious to defend this particular solution to the 'difficulty', we most certainly do wish to insist that any worthwhile theory of trade must allow for multiple-export economies. (Salvatore Baldone, commenting helpfully on this paper, is more positive about the solution in question).

3 Beyond the small economy analysis

Any reader immersed in closed economy versions of Sraffian analysis will be liable to feel that, while international trade is of course an important phenomenon that cannot properly be ignored, the above discussion has 'bent the stick' too far away from the closed economy framework. After all,

the critique of marginal theory *turns on* interdependence and the variation of relative prices with changes in distribution, whilst the small open economy analysis takes many relative prices as fixed and thereby approaches, at least, the 'partial equilibrium' form of analysis. Should not a trade theory be more ambitious, it may be asked, and would its results not then become more similar to those of closed economy theory? We naturally cannot anticipate every form of analysis that might be proposed but can only offer a few thoughts on two obvious possibilities, that of the single 'large' open economy and that of 'the world as a whole'. Just before doing that, however, we may note that the small open economy analysis should not be waived aside *too* quickly; are there not many real capitalist economies for which the 'small economy' analysis is, at the very least, often highly appropriate?

We began this chapter with the simple descriptive fact that capitalist economies are open, a fact hardly open to serious discussion. It is probably far more difficult to judge, in a serious manner, to what extent actual economies are 'large' in the relevant, technical sense. Let us assume however–and the assumption is not ridiculous–that a significant number of real capitalist economies are 'small' with respect to many commodities but 'large' with respect to one or more commodities. By definition, the analysis of such an economy must recognise that, for one or more commodities (on either the export or the import 'side'), the relation between price and quantity is crucially important. Now the discussion of such a relation lies entirely outside the formal framework of *Production of Commodities by Means of Commodities,* so that there is little or no scope for making straightforward comparison of its closed economy theory and large, open economy theory. We thus leave this type of analysis aside here, even while insisting that any worthwhile trade theory must be able to deal with it.

We turn finally to the suggestion that trade theory ought to deal, primarily at least, with the 'world as a whole.' It goes without saying that the world as a whole is a closed economy–but it does not go without saying that the theory of the capitalist, trading world will simply be 'closed economy theory writ large'. We only have space here to point to a few considerations and, in doing so, shall ignore transport costs, tariffs and many other phenomena; not because they are unimportant (they are not) but in order to emphasise that what we shall refer to is independent of such 'complications'. Moreover, in the interest of brevity we shall simply ask a number of questions, rather than attempt a full discussion.

a What may reasonably be assumed about the availability of alternative methods of production in different countries? This has at least two aspects. In the first place, some production processes are not footloose, for simple climatic or resource endowment reasons. Second, complex questions arise relating to the diffusion of knowledge about production processes, even when they are, in principle, footloose. The assumption

that there are given, alternative methods of production available is less easily interpreted in the real, world economy than in a fictitious, closed economy.

b The simple truth that labour is not homogeneous in any one economy holds *a fortiori* in the world economy; and while international labour mobility is certainly not zero in all respects, it is either zero or very low in many respects. How far would it be reasonable to take *all* real wage rates as given? (And recall the need to explain multiple-export economies.) The answer would no doubt need to give serious consideration to various institutional matters. But if the answer is, 'Not far', then how many–and which–rates could be so taken?

c How strong are the forces towards an international uniformity of the rate of profit? Are there perhaps stronger forces towards an international uniformity of (various kinds of) interest rates? And, if so, how different can the various national 'profit rate–interest rate margins' become without provoking international flows of *real* investment designed to change productive capacity and output? (Institutional considerations will again have to be taken into account.) How many–and which–rates of profit could it be appropriate to take as given?

d Our earlier consideration of a single, small open economy showed that the concept of a basic commodity became somewhat elusive and that, correspondingly, the (purely auxiliary) construct of a Standard commodity could even involve pure consumer commodities. Is there any reason to think that the solidity of these concepts would be restored in a world economy analysis?

e Underlying, and more fundamental than, question d) is the question what it could possibly mean to base one's analysis on 'given outputs' when the world economy is at issue. Could such an assumption have *any* useful sense if stated in terms of 'world gross outputs'? If taken to be an assumption about outputs *in each country,* then it would be an assumption ruling out most of the questions needing to be both asked and answered. It is incumbent on anyone who proposes that the analysis of the world economy can and should be based on a 'given outputs' assumption to explain in some detail exactly how it is to be interpreted and that, so interpreted, it is sensible. And if no such assumption can be made, then the question *will* arise 'as to the variation or constancy of returns' (Sraffa 1960a:v).

Within closed economy theory it has indeed proved useful, for certain purposes, to suppose the existence of known methods of production, given levels of physical inputs and outputs, and either a given real wage or a given rate of profit. It is not clear that matters can be so simple when the 'world economy' is considered.

4 Concluding remarks

Real capitalist economies are open economies, facing major flows of exports, of imports and of both short and long-term financial investment. They are multi-export and multi-import economies, in which domestic use does not always entail domestic production; though there are non-tradeable commodities, of course. These and other simple facts about capitalist economies must be faced head on in any economic analysis which is to be useful–the only exception to this being an analysis intended simply to criticise some other 'closed economy' theory. A consideration of Sraffa's seminal *Production of Commodities by Means of Commodities* has shown that the critique of the marginal theory of value and distribution, which it inspired, can readily be extended to the small open economy setting (other than under the most restrictive conditions, which no sensible defender of the marginal theory would wish to rely upon). It has also been seen, however, that a number of ideas familiar from Sraffa's book do not appear to retain their full value when one turns to the constructive task of analysing open economies–the only kind worth analysing constructively. This is not to deny, of course, that the economist engaged in such a task may well draw great inspiration from Sraffa's example of tireless dedication to clarity and precision in economic theorising.

Notes

1 Reprinted from *Metroeconomica* 50, 1999, pp. 260–76.
2 Cristina Marcuzzo has kindly informed me that in his Cambridge lectures for 1930 and 1931, Sraffa indicated that he would deal with foreign trade but that, in the event, he in fact said little about it.

17 Sraffa and the mathematicians

Frank Ramsey and Alister Watson

Heinz D.Kurz and Neri Salvadori[1]

1 Introduction

In the Preface of *Production of Commodities by Means of Commodities* Sraffa mentions John Maynard Keynes, pointing out that in 1928 he had shown him 'a draft of the opening propositions of this paper' (Sraffa 1960a:vi). Yet, there is no expression of gratitude to any of his fellow economists for comments, suggestions or assistance during the long period over which the book had been in preparation. There is no mention of Maurice Dobb, Richard Kahn, Nicholas Kaldor, Joan Robinson or of any other economist, whether Cantabrigian or not. The only people Sraffa thanks are three mathematicians: 'My greatest debt is to Professor A.S.Besicovitch for invaluable mathematical help over many years. I am also indebted for similar help at different periods to the late Mr Frank Ramsey and to Mr Alister Watson' (ibid.: vi–vii). In a provisional draft of the book's preface, written in Rapallo on 3 January 1959, Sraffa had also thanked David Champernowne amongst his 'mathematical friends' (*Sraffa Papers (SP)* D3/12/46:49).[2] However, at a later stage his name was dropped from the list. We can only speculate why Sraffa did this. Perhaps the presence of the name of Champernowne, who was a mathematician by training, but then had become a statistician and economist, would have rendered the absence of the names of other economists even more glaring. This Sraffa may have wanted to avoid. Sraffa's papers also show that he benefited from Champernowne, the mathematician, not Champernowne the economist. To avoid a possible irritation on the part of his other fellow economists Sraffa then may have decided to mention only pure mathematicians.

In this chapter we shall ask what was the role of some of Sraffa's 'mathematical friends' in the genesis of the propositions of his book. This question could not sensibly be approached, let alone answered, prior to the opening of Sraffa's unpublished papers and correspondence in the Wren Library of Trinity College, Cambridge. The available material provides evidence as to the kinds of problem Sraffa was concerned with and when, and which of these problems he would communicate to his mathematical

colleagues, seeking their assistance to solve them. It is hardly an exaggeration to say that without the help of Ramsey, Watson and especially Besicovitch Sraffa could not have accomplished his task.

In the various drafts of the Preface of his 1960 book Sraffa composed, he consistently singled out Besicovitch as the mathematician whom he owed the greatest intellectual debt. In fact, Besicovitch can be said to have taken a crucial part in the development of Sraffa's thought especially in the second and third phase of his work on *Production of Commodities,* that is, basically in the first half of the 1940s and in the second half of the 1950s. Sraffa consulted Besicovitch on virtually all problems of a mathematical nature he was confronted with. There are numerous documents in his unpublished papers reflecting their close collaboration. A proper treatment of it is beyond the scope of this chapter: the material is too huge and complex and ought to be dealt with separately. Confronted with the alternative of entirely setting aside Sraffa's collaboration with Besicovitch or of providing just a few illustrations of it, we opted for the former solution. This is a serious limitation of the chapter, which we hope to be able to make good in another work. Hence, apart from a few remarks in this chapter attention will exclusively focus on Sraffa's collaboration with Frank Ramsey and Alister Watson.

The composition of the chapter is as follows. Section 2 provides some hints as to Sraffa's training in mathematics. Section 3 gives information about his meetings with his mathematical friends, our main source being his diaries. The diaries are also used in section 4 in order to give an idea about the community of scholars involved in reading and commenting on the manuscript of his book. After the stage has been set we enter, beginning with section 5, into a discussion of Sraffa's collaboration with the mathematicians. Section 5 reconstructs Frank Ramsey's contribution. Sections 6 and 7 turn to Sraffa's collaboration with Alister Watson during the period when Sraffa was writing the book and at the time of the correction of the galley-proofs, respectively. Section 8 is an excursus to the main argument. Its starting point is the correction of a slip in Sraffa's book by Harry Johnson and Sraffa's response to it. The reconstruction of this story is here reported because it sheds additional light on the relationship between Sraffa, David Champernowne and Alister Watson. Section 9 contains some conclusions.

2 Sraffa's training in mathematics

Sraffa had no special training in mathematics: he had been exposed to the ordinary dose of mathematics common in Italian secondary schools, but no more, and during his studies at Turin University the classes he attended were mathematically not demanding. When Sraffa moved to Cambridge he apparently brought with him two books by Pradella (1915a and 1915b) on the mathematics which were then used in secondary schools. Sraffa's annotations in the first of the two document that he must have studied the

volume carefully. Pradella's book on algebra and arithmetic is mentioned a few times in his notes. For instance, he refers to it in a document titled 'First equations: on linear homogeneous equations' (*SP* D3/12/10:33). Another book to which Sraffa referred in his first papers on systems of production equations is Chini (1923). In particular there are two documents dated 'End of Nov. 1927' in which Sraffa calculated two numerical examples relative to equations without a surplus and with a surplus (see *SP* D3/12/2:33). In the example with a surplus he found that there was no solution (since the two equations were contradictory). There is a big question mark added on the document, but then follows the remark:

> V. Chini p. 41 (le equazioni sono contraddittorie quindi non esiste alcuna soluzione)
>
> Le equaz. devono essere { non contradditorie / indipendenti
>
> [See Chini p. 41 (the equations are contradictory and as a consequence there is no solution) non
>
> The equations must be { non contradictory / independent]

A copy of the book by Chini (1923) is in Sraffa's Library (No. 3204), but there are only a few annotations, mainly on pages 41 and 42, where the mentioned property is dealt with.

Another book to which Sraffa refers sometimes is Vivanti *(Complementi di Matematica):* see, for instance, *SP* D1/11:79, where with the help of this book Sraffa calculates some simple derivatives and the maximum of a simple function. Vivanti is referred to in another document in which Sraffa expressed some concern about the possibility that his system of equations has '*infinite* soluz. proporzionali' (*SP* D3/12/11:86). However, Vivanti's book is not in Sraffa's Library (in all probability Sraffa referred to Vivanti 1903). In his papers there are also references to G.Chrystal's book on *Algebra,* part I, published in 1889, which Sraffa consulted on the solution of systems of equations (see *SP* D3/12/6:23; see also *SP* D3/12/8:1 and 30); there is no copy of the book in Sraffa's library.[3]

3 Sraffa's meetings with his 'mathematical friends'

In his *Cambridge Pocket Diaries* Sraffa used to note his appointments and the meetings he attended. The diaries provide a useful skeleton of his activities over time. They also provide useful information about his meetings and collaboration with his mathematical friends which gets some confirmation from the material contained in his unpublished papers. There is no presumption, of course, that this information is complete, nor can we be sure

that the meetings were mainly or at least partly devoted to discussing the problems Sraffa encountered in his attempt to reformulate the Classical approach to the theory of value and distribution. However, cross-checking the dates listed and the dates of some of his unpublished manuscripts in which he refers to the discussions he had with Frank Ramsey, Alister Watson and A.S.Besicovitch reveals that there are close connections between the two. Therefore it might be of some interest to begin by providing the details of the respective information available in Sraffa's diaries.

As is well known, Sraffa's work on what was eventually to become his 1960 book fell in three periods: the first broadly comprised the years from (late) 1927 to 1930,[4] the second the 1940s, with the main activities in the first half of the decade,[5] and the third the second half of the 1950s.

In the first of the aforementioned periods the following meetings with Frank Ramsey are listed in Sraffa's diaries; during this period there is no information about meetings with other mathematicians. The first appointment with the young mathematician is dated 28 June 1928. The two meet again on 11 November 1928, on 10 and 30 May, and on 29 November 1929. There are no other appointments listed with Ramsey, who died from an attack of jaundice on 19 January 1930 in a London hospital.

In the second period there are four meetings with David Champernowne noted in Sraffa's diaries, two at the beginning of the 1940s, 27 October 1940 and 1 February 1942, and two in the second half, that is, 26 November and 11 December 1947. However, Sraffa's writings in that period do not seem to reflect an impact of Champernowne on the progress of his project. Things are different with regard to Besicovitch. The following meetings with him are listed in the diaries: 29 October and 7 and 11 November 1942, 13 May 1943, 5 June 1944. Besocovitch's collaboration with Sraffa is also vividly reflected in the latter's unpublished papers.[6] From 1945 onward Sraffa also met with Alister Watson. The diaries list the following appointments: 1 May and 30 July 1945, 19 January 1947, 31 January 1948, 4 and 7 January 1949.

The 1950s show these appointments. Both before and after his completion of the main body of the Ricardo edition Sraffa met with Alister Watson and David Champernowne. According to his diary Watson visited Sraffa in Cambridge from 25–7 July 1952 and from 13–14 January 1953. He had an appointment with Champernowne on 15 February 1953. Watson visited Sraffa again from 29–30 April 1955. The date of this latter visit is significant, because it took place only a few days after Sraffa's return from Majorca and Spain, where he had begun, in Majorca, to resume his constructive project and to draft parts of his book. Apparently, he was keen to discuss with Watson some of the difficulties he encountered. On 14 June of the same year Sraffa noted in his diary: 'Besicovitch returned from America.' Obviously, he was also eager to get Besicovitch's assistance. A few days later, on 18 June, he wrote: 'Trovato il trick per ridurre il sistema a linearità (utilizzando relaz. lineare fra w e r) con

soluzione lineare generale di R' [Found the trick to reduce the system to linearity (using a linear relation between w and r) with a general linear solution for R]. His meeting with Besicovitch had to be postponed, however, because on 5 June Sraffa left for continental Europe, where he stayed until 4 October.[7] His diary notes 'passegg. Besicovi[t]ch' [walk with Besicovitch] on 18 November of the same year. In mid-December Sraffa had to undergo an operation because of a hernia and spent several weeks in the Evelyn Nursing Home. Besicovitch visited him twice, on 21 December 1955 and on 4 January 1956. On 21 April Sraffa's diary notes 'walk Besicovitch'; then there are meetings listed on 25 July, 6 August and 19 October. In the second half of 1957 Sraffa had several meetings with David Champernowne, who was then still affiliated with Oxford University. The first meeting of the two in that year is dated 20 July. On 19 August Sraffa noted in his diary: 'written to Champernowne & booked room', and an entry on 24 August says: 'Champernowne arrives.' Apparently Champernowne stayed until 28 August and had every day long discussions with Sraffa. Most importantly, as Sraffa noted on 26 August: 'Champernowne (legge il mio lavoro. Tutto Part I, §1–47' [Champernowne (reads my work. The whole Part I, §1–47], and on the following day: 'e 2 Appendices' [and two appendices]. Champernowne's reading continued the following days. On 28 August Sraffa noted in his diary: 'Champernowne ritorno a Oxford' [Champernowne back to Oxford]. Three days later, on 31 August, he had every reason to be happy because he could note in his diary: 'Besicovitch offre di aiutarmi nei miei problemi matematici' [Besicovitch offers to help me with my mathematical problems]. Yet the following day, on 1 September, we find the sober observation: 'Besicovitch (pochino!)' [Besicovitch (not much!)]. Two other meetings appear to have taken place that month, one on 7 September, about which we find the remark: 'Besicovitch risponde a domanda' [Besicovitch answers to question], and one on 13 September.

It must have come as a shock to Sraffa when around the turn of the month Besicovitch told him that he could not help him any more. On 1 October 1957 an understandably depressed Sraffa noted in his diary: 'Besicovitch non ce la fa' [Besicovitch cannot do it]. Yet, the pending tragedy did not unfold: just one day later we find the relieving message: 'Bes. si ri-interessa' [Besicovitch gets interested again]. One can only wonder what has made the mathematician radically change his mind twice in so short a time. Then the speed at which Sraffa's work progressed accelerated tremendously. He had another meeting with Besicovitch on 5 October. On the following day Sraffa jotted in his diary that the mathematician 'Swinnerton-Dyer guarda il mio problema' [Swinnerton-Dyer looks at my problem].[8] On 17 October he noted: 'Besicovitch manda il mio problema a Todd' [Besicovitch sends my problem to Todd].[9] On 22 October, we read: 'Bes. mi da una soluz. dei non-basic' [Besicovitch gives me a solution for non-basics], and on 1 November: 'Besicovitch (ultime prove)' [Besicovitch (last proofs)]. Sraffa had further meetings with his elder friend on 2 November and on 8 December. (On 17

December, in a session that lasted for five hours, he discussed with Nicholas Kaldor 'Capital theory–depreciation'.) On 25 December we read: 'Besicovitch (prova non-basics in multiple syst.)' [Besicovitch (proves non-basics in multiple system)]. After another meeting with his mathematical friend on 26 January 1958 and some hard work we find on the 29th of that month the triumphant exclamation: 'Filled last gap in my work (Rent) FINIS.'

4 Reading the manuscript and the proofs

On 5 February 1958 Sraffa wrote to Alister Watson inviting him to see his work in Cambridge. Before that visit took place, other people were involved in reading the manuscript. On 7 February he had lunch with Kaldor in College, who afterwards 'legge 17 pp. mio lavoro' [reads 17 pages of my work]. On 12 February he reported: 'Maurice [Dobb] legge 10 p. mio lavoro' [Maurice reads 10 pages of my work]. On the same day Sraffa wrote again to Watson, anxious to get his younger friend's reaction. Watson came to Cambridge for the weekend from 15–17 February. On 15 February Sraffa noted in his diary: '12 [o'clock] Alister Watson arrives for week-end to read my work'; on the 16th: '10.30–1 Watson reads[;] 2–4 walk to Coton[;] 5–8 reads on[;] 8 Watson in hall (Master's lodge Besicovitch)'; and on the 17th: '1 Watson lunch, poi riparte' [1 o'clock Watson lunch, then he leaves]. On 19, 21 and 27 February Maurice Dobb continued his reading to arrive at p. 75 of the manuscript. On 6 March Sraffa noted: '4.30–6 Maurice (discussion, no reading)'. On 11 March Sraffa reported the receipt of a letter by Watson announcing his coming on Wednesday of that week. On 11 and 12 March there were altogether four meetings between Sraffa and Watson dedicated to 'mio lavoro' [my work]. On 21 and 22 March Champernowne was involved in reading and discussing the manuscript. On 25 March Sraffa left for Paris and then Milan, where Sergio Steve read the work between 9 and 12 April; on 12 April Sraffa noted in his diary in brackets: 'S.consiglia pubblicare con prefaz. che spieghi attaches storici' [Steve advises to publish with a preface explaining historical backgrounds]. After his return to Cambridge on 15 April there were two further meetings with Champernowne on 18 and 19 April. On 3 May Sraffa went on a walk with Besicovitch. On the 23rd of that month he wrote to Champernowne. Apparently, Sraffa had doubts about whether to publish the work. These were effectively dispelled, it seems, by his Trinity College mathematical friend; on 31 May we find the following entry in Sraffa's diary: 'Besicovitch insiste che io pubblichi[;] il fatto che ho potuto prevedere risultati matematici interessanti mostra che c'è qualcosa nella teoria' ['Besicovitch insists that I publish; the fact that I was able to forsee interesting mathematical results shows that there must be something in the theory].

Later that year Sraffa attended (together with Champernowne and Kaldor) the famous conference on capital theory in Corfu, 4–11 September 1958 (see Lutz and Hague 1961), where he met, among others, John Richard

Hicks, Edmond Malinvaud, Paul A.Samuelson and Robert Solow. There are no indications in his diary that he spoke to his fellow economists about his book. However, from private conversations with Paul A. Samuelson we know that in Corfu Sraffa had told him that he was about to publish a book on capital theory.[10]

On the occasion of a visit to Italy during Christmas vacation of 1958 Sraffa prepared drafts of the Preface of his book, but was unsatisfied; in addition he carried out corrections of Part III. He sent copies to his friend Mattioli in Rome. Back in Cambridge he gave Pierangelo Garegnani the opportunity to read the manuscript between 14 and 19 January 1959. On 1 March he noted in his diary: '1 Birch–rimette a posto il mio esempio numerico' [Birch–fixes my numerical example].[11] The following day we read: 'dato a Dobb da leggere Part I del mio MS' [given to Dobb to read Part I of my MS]. On 16 March Sraffa had a 'seduta con Dobb: sue osservazioni dopo letto tutto il mio lavoro' [session with Dobb: his remarks after having read the whole work]. On 31 March he reported, in brackets: 'Consegnato MS per estimate' [manuscript has been presented for the estimate], and on 3 April an appointment with Burbridge, the man at CUP in charge of his book: 'accepted estimate U.P.' On 22 April he noted with some irritation, in brackets: 'Champernowne riparte senza avermi visto' [Champernowne leaves without having seen me]. (Champernowne, who had applied for a position in Cambridge, had visited the Faculty.) In a letter dated 2 May 1959 he was informed that people in the Department of Applied Economics of Cambridge University would check the calculations for the numerical examples contained in the book (see *SP* D3/12/112:78). On 9 May Sraffa wrote a letter to Roy F.Harrod and on 10 May one to Champernowne. On 19 May he was informed by the Press: 'Burbridge: "Prod, of Com." comincia in settimana: bozze fra un mese o 6 sett.' [Burbridge: "Prod, of Com." starts within a week: proofs in a month or six weeks]. On 22 May we read: '1.45 phoned Champ. (Council has appointed him).' In the period from 29 May to 3 June Garegnani is reported to have read the entire manuscript. On 28 July Maurice Dobb is said to have provided 'correz. al mio MS' [corrections to my manuscript].

On 7 September 1959 Sraffa received from Burbridge, 'in segreto' [in secret], a set of proofs before they were corrected inhouse (this set seems to correspond to item No. 3371 of Sraffa's books). Next day a meeting with Champernowne is reported. The following day Sraffa left for Paris and then Milan, where on the 26th of the month he could happily note in his diary: 'Ricevuto 1ª bozza corretta completa di "P. of C. by C."' [Received the first corrected complete proofs of "P. of C. by C."]. On the following day he wrote: 'rivisto bozze in albergo' [checked proofs in the hotel].

Back in Cambridge he had a 'seduta con Champernowne' [session with Champernowne] during the afternoon of 29 September. On 2 October he received four additional copies of the proofs from the binders. On the 9th of

that month there is a note '9 Maurice (mie bozze)' [9 o'clock: Dobb (my proofs)]. Amartya Sen read the proofs on 22, 24, 25 and 28 October. On the following day Sraffa went on a walk with Carlo Brunner and noted in his diary: 'ridatogli bozze P. of C. by C.' [I gave him again the proofs of P. of C. by C.].[12]

On 3 November Sraffa reported to have 'phoned to Alister Watson & sent him proof to read'. In brackets he added: 'rec^{d}. 18th', which replicates the information given on the 18th of that month: 'received proofs with comments from A.Watson.' In the meantime Sraffa had another meeting with C.Brunner, on 8 November, concerning a 'report su [on] P.C.C.'; and two days later he reports 'p. 16 correction to Watson Brunner e Matt. [Mattioli]'– the reference being apparently a correction sent to the people mentioned. On 20 November Sraffa met Robert Neild at 7.30 p.m.; in his diary he noted: '9.30–12.30 Robert legge mie bozze' [9.30–12.30 Robert reads my proofs], an activity that is continued on the following two days: on the 21st between 11 a.m. and 1 p.m. and between 2.45 and 6 p.m.; and on the 22nd between 10.30 and 12 a.m. and between 3 and 6 p.m., where, as Sraffa did not fail to notice in brackets, Neild 'Salta i 3 cap. J.-P' [Skipped chapters VII–IX]. On 13 December he noted: 'sent [my]self proofs Milan', that is, to Mattioli.

On 16 December Sraffa left for Paris and then Milan, where on the morning of the 19th he began to dictate an Italian translation of his book to a secretary in Mattioli's office. This work and the correction of the text, which was carried out in long sessions, assisted by Mattioli, was finished at 5 a.m. on 12 January 1960. To celebrate the event, Sraffa, Mattioli and Giulio Einaudi (the publisher of the Italian edition of Sraffa's book, a son of Sraffa's former teacher Luigi Einaudi) had champagne.

Back in Cambridge Sraffa noted in his diary on 17 January: 'bozze' [proofs], referring presumably to the second set of proofs. On the 20th of that month he sent the English proofs to his friend and fellow economist Sergio Steve 'per confrontare con le ital.' [for a comparison with the Italian proofs]. On the same day he received the blurb for his book which he showed to Dobb and sent by express mail to Mattioli. On 24 January he noted: 'mandate 2^{0} bozze ingl. a Steve (espresso) e lettera id[;] scritto Matt, (con bozze indice)' [I sent second proofs of the English version to Steve by express with a letter; I wrote to Mattioli (with proofs of the index)]. Two days later we read: '9 Maurice (queries on last doubts)'. On 18 February Sraffa noted: 'Handed in final proofs *for press*!' However, an entry of 20 March reads: 'espresso a [express to] Burbridge con [with] stoppress corrections.' Back in Italy (Rapallo) Sraffa received on 7 April '2 copie mio libro in fogli (di 32 pagine)' [two copies of my book in folio (of 32 pages)]. The following day he got from Einaudi the second set of proofs of the Italian version of his book and started working on them, assisted by Steve and Mattioli.

On 13 April he met Rosenstein-Rodan in Milan. Sraffa noted in his diary: 'Rosenstein has seen, in Boston, 2 or 3 weeks ago, my proofs: disagrees on

"marginal" in Preface but not with the rest. Also Solow and Samuelson have seen and approved.'[13]

Back in Cambridge, Sraffa on 16 April noted in his diary: 'A Dennis [Robertson], a sua richiesta, le bozze del mio libro' [To Dennis, at his request, the proofs of my book]. The following day carries the entry: 'Dennis has read my Ch. I, will read no more. "A wicked book, ought to be burnt".' And on 18 April we read: 'Dennis: "Not ashamed of yourself! an immoral book. Neo-ricardian & Neo-marxist".' On 12 May Sraffa noted: 'To Joan, advance copy of my book.' While Sraffa, Joan Robinson and Richard Kahn ('Joan & Kahn', as Sraffa used to refer to them in his diary) had had numerous walks together during the last couple of years, the evidence suggests that Sraffa did not inform the two about the precise content and progress of his work. It was only after he had completed the book that he would break his silence.[14]

On 27 May 1960 we read: 'Pubblicaz. ediz. inglese "Production of Commodities".' And on 6 June: 'Produzione Merci a ½ Merci pubblicato in Italia'.[15] On the front cover of Sraffa's copy of the 1959–60 *Cambridge Pocket Diary* these two important events are abbreviated as: 'P.C. x C 27/5 e P.M. ½ M. 6/6 pubblicato.'

5 Frank Ramsey

Sraffa began to formulate what he was later to call the 'conditions of production' or the 'production system' in terms of systems of simultaneous equations in the second half of the 1920s. Sraffa's 'first equations' refer to systems of production without a surplus, whereas his 'second equations' refer to systems with a surplus. At the end of November 1927 he put down equations representing two industries without and with a surplus (see *SP* D3/12/2:32–5). One of the systems with a surplus is given by

$$\begin{aligned} 11A &= 3A + 9B \\ 13B &= 7A + 3B \\ S &= 1A + 1B, \end{aligned}$$

where A and B indicate the prices of the two commodities and S the volume of the surplus product of the system as a whole. Sraffa observes that these equations are 'contradictory' (ibid.) and that 'the problem is overdetermined' (*SP* D3/12/11:17).

On 26 June 1928 Sraffa summarised what Ramsey appears to have told him on the occasion of their meeting earlier that day:

(1) Equations with surplus: Exact solutions can be found for up to 4 equations. Approximate solutions can probably be found for any number of equations.
(2) It can probably be proved that, whatever the number of equations only *one* set of solutions is significant.

(3) Equations without surplus: each quantity must be expressed by *two* letters, one being the number of units, the other the unit of the commodity. Otherwise, if I use only one letter, this would stand for heterogeneous things and the sum would be meaningless.

(*SP* D3/12/2:28)

This note should probably be seen in conjunction with another note in the same folder, which, however, has no date on it, but appears to have come out of the same meeting (ibid.: 29). The first three lines of the second document are in Sraffa's hand in pencil and the rest is in Ramsey's hand in ink. Sraffa put down the following system of equations:

$$v_a A = \left(v_a a_1 + v_b b_1 + c_1\right) r$$

$$v_b B = \left(v_a a_2 + v_b b_2 + c_2\right) r$$

$$C = \left(v_a a_3 + v_b b_3 + c_3\right) r$$

The interpretation is obvious: *A, B* and *C* are the gross outputs of commodities *a, b* and *c,* respectively, the a_is, b_is and c_is are the inputs of the three commodities in the production of commodity *i* (*i*=1, 2, 3; where, obviously, 1 stands for *a,* 2 for *b,* and 3 for *c*), and v_j is the value of commodity *j* (*j*=*a, b*), commodity *c* serving as numeraire; *r* is the interest factor. The part written by Ramsey is the following:

$$v_a\left(a_1 - \frac{A}{r}\right) + v_b b_1 + c_1 = 0$$

$$v_a a_2 + v_b\left(b_2 - \frac{B}{r}\right) + c_2 = 0$$

$$v_a a_3 + v_b b_3 + \left(c_3 - \frac{C}{r}\right) = 0$$

$$\begin{vmatrix} \left(a_1 - \frac{A}{r}\right), & b_1, & c_1 \\ a_2, & \left(b_2 - \frac{B}{r}\right), & c_2 \\ a_3, & b_3, & \left(c_3 - \frac{C}{r}\right) \end{vmatrix} = 0$$

As already stated, in our interpretation the two documents refer to the same meeting, but chronologically the order is to be reversed: the one containing the three remarks was a memo prepared by Sraffa after the meeting summarising its results, whereas the other document was produced during the meeting. At first Sraffa appears not to have explicitly distinguished between the quantity and the price or value of a commodity, a fact to which Ramsey immediately seems to have objected. Sraffa then appears to have introduced the distinction during the conversation with Ramsey, as is shown by the second document. Ramsey then reformulated the system first by putting the system of homogeneous linear equations in its canonical form, then by setting the determinant of coefficients equal to zero in order to get a non-trivial solution. This was enough for him to recognise what became the first remark in Sraffa's memo of the meeting. This remark, in fact, says that although there are solutions for any number of equations (that is, processes and therefore produced commodities), their computation is possible only for a number of commodities smaller than or equal to 4. There is no doubt that this refers to the fact that algebraic equations of a degree larger than 4 are not solvable in terms of radicals and, as a consequence, with the exception of some special cases, only approximate solutions can be found. Ramsey, in fact, calculated what in the spectral analysis of a matrix is called the characteristic equation (whether he knew this literature or not) and found that it is an algebraic equation whose degree is equal to the number of commodities involved.

As regards the second remark, as reported by Sraffa, we do not know, of course, what was at the back of Ramsey's mind. However, had the starting point of his remark been the Perron-Frobenius Theorem, then things would have been crystal clear. Yet in this case he could have been expected to draw Sraffa's attention to the existence of this theorem, which is a most powerful tool to solve the kind of problems Sraffa was interested in. There is no evidence to this effect; on the contrary, Sraffa's papers would seem to imply that none of his mathematical friends referred him to this theorem.[16]

The reference in the third remark to 'Equations without a surplus' was perhaps meant as a reminder that back home Sraffa had to carry out the change with regard to equations with a surplus also with regard to the equations without a surplus.[17]

6 Alister Watson's help during the writing of *Production of Commodities*

Before we enter into a discussion of the details of how Watson assisted Sraffa in solving some of his mathematical problems, it is perhaps worth mentioning that Watson felt honoured by being asked to lend a helping hand. This is neatly expressed in the following letter by Watson dated 13 February 1958 (*SP* C 333):

Dear Sraffa,
Many thanks for your letter. I hope to get up to Cambridge on Saturday by midday. It will be very good to see you & I am very grateful to you for asking me.

Yours sincerely,
Alister Watson

Unfortunately, there is no record of the meeting (which is also mentioned in the diary).

The first note we came across referring to Alister Watson is entitled 'Alister Watson's visit to Cambridge' on 19 January 1947.[18] It is a memo by Sraffa about the content of the discussions they had (*SP* D3/12/44:4, 6). Apparently, the main question was the uniqueness of the maximum rate of profits *R:*

> I. Q-system: several all-positive solutions. The only solution I have considered gives a value 0 to the *qs* of all *non-basic* processes.
>
> However, suppose that one (or more) of the non-basic commodities [wheat] uses itself in its own production in a proportion greater than that of the basics taken as a whole (in other words, its own internal *R* is *smaller* than the *R* of the basic group), then there is another solution: for this non-basic commodity uses in its own production some basic ones, thus diminishing the ratio of basic means of production to basic products.
>
> If, on the other hand, the internal *R* of the non-basic is larger than the *R* of the basic group, there is only one all-positive solution, with the *q*'s for non-basics=0.
>
> N.B. This has its symmetrical case in the P-system. If some of the non-basics have an own internal *R larger* than the basics group, there are alternative solutions with all the basic *p*'s=0, and bigger values of R.
>
> (ibid.: 4)

The note continues on page 6, whereas page 5 includes a note added by Sraffa on 23 February 1955. Let us report first the end of the note of 1947:

> We can thus sum up:
> There are several non-negative systems of roots of the Q-system. The system with the largest value for *R* has all zero values for the *qs* of the non-basic processes.
>
> There are several non-negative systems of roots of the P-system. All these systems, except the one with the smallest value for *R*, have all zero values for the *ps* of the basic commodities.
>
> [N.B. (1) The largest R of the Q-system is equal to the smallest *R* of the P-system.–(2) The proposition referring to the Q-system assumed that non basics have a *smaller* internal own ratio than *r*-basic; that referring to P-system assumes it *larger*]
>
> (ibid.: 6)

The Q-system mentioned in this memo is certainly the system of equations which determine the Standard commodity

$$\mathbf{q}^{\mathrm{T}}[\mathbf{I} - (1+R)\mathbf{A}] = \mathbf{0}$$

where $\mathbf{q}$ is the vector of multipliers, $\mathbf{A}$ is the square matrix of material inputs, $\mathbf{I}$ is the identity matrix, and R is the maximum rate of profit. If there are non-basic commodities and if matrix $\mathbf{A}$ is in the canonical form, then the above equation can be stated as

$$\mathbf{q}_1^{\mathrm{T}}[\mathbf{I} - (1+R)\mathbf{A}_{11}] = (1+R)[\mathbf{q}_2^{\mathrm{T}}\mathbf{A}_{21}+\mathbf{q}_3^{\mathrm{T}}\mathbf{A}_{31}+ \ldots + \mathbf{q}_3^{\mathrm{T}}\mathbf{A}_{s1}]$$

$$\mathbf{q}_2^{\mathrm{T}}[\mathbf{I} - (1+R)\,\mathbf{A}_{22}] = (1+R)[\mathbf{q}_3^{\mathrm{T}}\mathbf{A}_{32} +\ldots + \mathbf{q}_s^{\mathrm{T}}\mathbf{A}_{s2}]$$

. . . . . .

$$\mathbf{q}_s^{\mathrm{T}}[\mathbf{I} - (1+R)\mathbf{A}_{ss}] = \mathbf{0}^{\mathrm{T}}.$$

It is clear from Sraffa's memo that he had arrived at the solution obtained by setting $\mathbf{q}_i = \mathbf{0}$ $(i = 2, 3, \ldots, s)$, $R = (1 - \lambda_1)/\lambda_1$ (where λi is the Perron-Frobenius eigenvalue of matrix $\mathbf{A}_{ii}$), and $\mathbf{q}_1=\mathbf{x}_1$ (where $\mathbf{x}_i$, is the left eigenvector of matrix $\mathbf{A}_{ii}$ corresponding to λ_i). But Watson showed him that if the Perron-Frobenius eigenvalue of submatrix $\mathbf{A}_{jj}$, λ_j, is larger than the Perron-Frobenius eigenvalues of matrices $\mathbf{A}_{11}$, $\mathbf{A}_{22}$,..., $\mathbf{A}_{j-1,\ j-1}$, then another non-negative solution is found by setting $\mathbf{q}_i = \mathbf{0}$ $(i = j + 1, j + 2, \ldots, s)$, $R=(1-\lambda_j)/\lambda_j$, $\mathbf{q}_j=\mathbf{x}_j$, and

$$\mathbf{q}_i^{\mathrm{T}}=(1+R)[\mathbf{q}_{i+1}^{\mathrm{T}}\mathbf{A}_{i+1,i} + \mathbf{q}_{i+2}^{\mathrm{T}}\mathbf{A}_{i+2,i} + \ldots + \mathbf{q}_j^{\mathrm{T}}\mathbf{A}_{ji}][\mathbf{I} - (1+R)\mathbf{A}_{ii}]^{-1}$$

$(i = 1, 2, \ldots, j - 1)$. Of course, several of these solutions may exist and to each of them corresponds an R smaller than that found by Sraffa and none of these solutions exists if 'the internal R of the non-basic is larger than the R of the basic group', that is, if $\lambda_1 > \lambda_j$ $(j = 2, 3, \ldots, s)$. These are the results summarised in Sraffa's memo with respect to the 'Q-system'.

Let us turn now to the 'P-system'. This is clearly the price system when the wage rate equals zero and, as a consequence, the rate of profits equals the maximum rate of profits:

$$\mathbf{p} = (1 + R)\mathbf{A}\mathbf{p}$$

where $\mathbf{p}$ gives the price vector. If there are non-basic commodities and if matrix $\mathbf{A}$ is in the canonical form, then the price equation can be stated as

$$\mathbf{p}_1 = (1+R)\mathbf{A}_{11}\mathbf{p}_1$$

$$\mathbf{p}_2 = (1+R)(\mathbf{A}_{21}\mathbf{p}_1 + \mathbf{A}_{22}\mathbf{p}_2)$$

. . . . . .

$$\mathbf{p}_s = (1+R)(\mathbf{A}_{s1}\mathbf{p}_1 + \mathbf{A}_{s1}\mathbf{p}_2 + \ldots + \mathbf{A}_{ss}\mathbf{p}_s)$$

It seems that the solution Sraffa had in mind prior to Watson's visit in January 1947 was $\mathbf{R} = (1 - \lambda_1)/\lambda_1$, $\mathbf{p}_1 = \mathbf{y}_1$ (where $\mathbf{y}_i$ is the right eigenvector of matrix $\mathbf{A}_{ii}$ corresponding to λ_i), and

$$\mathbf{p}_i = (1+R)[\mathbf{I} - (1+R)\mathbf{A}_{ii}]^{-1}[\mathbf{A}_{i1}\mathbf{p}_1 + \mathbf{A}_{i2}\mathbf{p}_2 + \ldots + \mathbf{A}_{i,i-1}\mathbf{p}_{i-1}]$$

($i = 2, 3, \ldots, s$). But this solution is semipositive (actually positive) *if and only if* $\lambda_1 > \lambda_i$ ($i = 2, 3, \ldots, s$). The memo *does not* notice this fact. It *does* notice another fact, i.e., that if , $\lambda_j > \lambda_i$ ($i = j + 1, j + 2, \ldots, s$), then a non-negative solution can be found by setting , $\mathbf{p}_i = \mathbf{0}$ ($i = 1, 2, \ldots, j - 1$), $R = (1 - \lambda_j)/\lambda_j$, $\mathbf{p}_j = \mathbf{y}_j$ and

$$\mathbf{p}_i = (1+R)[\mathbf{I} - (1+R)\mathbf{A}_{ii}]^{-1}[\mathbf{A}_{ij}\mathbf{p}_j + \mathbf{A}_{i,j+1}\mathbf{p}_{j+1} + \ldots + \mathbf{A}_{i,i-1}\mathbf{p}_{i-1}]$$

($i = j + 1, j + 2, \ldots, s$). Note that in all these solutions the prices of basics are zero and, if $\lambda_1 > \lambda_i$ ($i = 2, 3, \ldots, s$), the R so determined is larger than $(1 - \lambda_1)/\lambda_1$.

The set of assumptions implicit in the memo is not entirely clear. Certainly it is assumed that there is at least one basic. But all the remarks on the 'P-system' are correct only if $\lambda_1 > \lambda_i$ ($i = 2, 3, \ldots, s$), whereas in this case the remarks on the 'Q-system' become uninteresting.

Let us now turn to the supplement of 1955. Sraffa wrote:

> We can avoid all these complications by, from the start, removing "manually" all the non-basic equations and dealing with a system composed exclusively of basic commodities [these to be defined, before the removal, as comm.s which directly or indirectly enter all the others] (*) and then we can say that there is only one all-positive [and not merely non-negative] solution for the *ps* and the *qs*.
>
> [N.B. One point which needs clearing about the Watson alternative solution is this: does it remain true that if we multiply the equations by any pair of solutions of the *ps* and *qs,* which is not the all-positive pair of solutions, the sum of all the equations is null?]
>
> (*) For practical application this good enough. But discuss in a note the abstract possibilities of this not being so, e.g. of the system falling into two or more self-contained (self-basic) groups of commodities–as if one lumped together the equations of two countries which have no commercial relations (& treating, of course, iron in country *A* as a different commodity from iron in *B*).
>
> The more 'elegant' system of solving the complete system (with *qs* of non-basics=0) can be discussed here with the Watson difficulties (query, did he derive them from von Neumann?).
>
> (ibid.: 5)

In this note we find what was to become the expository strategy of *Production of Commodities*. In §35 (the last section of Chapter IV), whose title in the table of contents is 'Non-basics excluded', Sraffa argues that 'We may in

consequence simplify the discussion by assuming that all non-basic equations are eliminated at the outset so that only basic industries come under consideration' (Sraffa 1960a:25). This is essentially the idea expressed in the note of 1955 of 'removing "manually" all the non-basic equations and dealing with a system composed exclusively of basic commodities' (D3/12/44:5). In the book the argument justifying this choice is completed with a footnote referring to the 'freak case of the type referred to in Appendix B' (Sraffa 1960a:25, fn.), that is, to what in the note above is called 'the Watson alternative solution'.

In the 1955 note, the question in brackets refers to the reason why the proof of the uniqueness of the solution of the 'Q-system' provided in §41 of the book for the case in which the non-basics are excluded does not apply when they are *not* excluded. The point is that now both some *p*'s and some *q*'s can be zero and the zeros can be distributed in such a way that the scalar product of the price vector with a solution of the 'Q-system' different from the Standard one can be zero even if both vectors are semipositive. This possibility can be excluded when one of the two vectors is positive, as is the case in which non-basics are excluded.

On 29 April 1955 Watson visited Sraffa again. In February of that year, apparently in order to prepare for the visit, Sraffa annotated his previous notes. After the visit he produced a memo of their discussions (*SP* D3/12/58:8–9):

> Points discussed: (Told him the proof of uniqueness of all-positive solution of *q*'s and *p*'s)
>
> (1) The value of *R* which corresponds to the all-positive *p*'s and *q*'s is the smallest of the values of *R*. This is proved by the same method by which solution is sought by approximation through successive substitutions. (By the same method Watson proves existence of an all positive solution of *ps,* which I prove by continuity from $r=0$.)
>
> (2) All the values of *R* in p-system are equal to the corresponding values in the q-system. (This is proved by means of the determinant of the coefficients, which is the same in the two systems).
>
> (3) Discussed the possibility of proving uniqueness in case of joint products, when there may be negative *qs* and *ps*. Does not regard it as likely.
>
> [Subsequently I have concluded that *if* there is an all-positive solution of the qs (as there *must* be for fixed cap. and there *may* be for joint products) then uniqueness can be proved for any really existent system: for at *some* value of *r* this must have all positive *ps* (i.e. at the *actual* value of *r*); now this *r* can be represented in terms of *R,* & then the proof can be applied.]
>
> [With ref. to N. 2 above. My proof of positive prices in the one-process-one-product system is as follows. At $r=0$ values are proportional to quantities of labour, & these being positive, values must be positive. Now

increase gradually r until wages fall to 0. Can any price turn negative as a result? in order to do so, the change being continuous, it must first become 0; but to do so, wages being positive, the price of one of the commod. used in its production must become negative. So no price can become negative first.–This does not apply to joint prod, or Fix. Cap. For the price of a joint prod, can become 0 without need that any other price is negative; it suffices that the price of the other joint product becomes large enough.]

This memo clearly refers to some of the proofs included in *Production of Commodities,* expecially those of Chapter V. An echo of the argument (2) in the memo is in §29 where it is proved, among other things, that the maximum rate of profits coincides with the 'Standard ratio'. Interestingly, the proof in the book does not use the determinant argument, but follows from an economic reasoning. In §37 (which is the first paragraph of Chapter V, apart from the summary of the Chapter presented in §36) Sraffa proves the existence of the Standard system, following a procedure which seems to correspond to that described in the memo as 'by approximation through successive substitutions'.[19] Further, in §39 the positivity of prices for each rate of profits between zero and the maximum rate of profits is proved following the procedure illustrated above, based on the fact that no price can become negative before any other. In the book, but not in the memo, the proof is completed with a reference to the fact that the 'prices of basic products cannot become negative through becoming infinite' (Sraffa 1960a:28 fn.). The proof of the uniqueness of the Standard system which is sketched in the memo for some possible case of joint production is actually the proof used for single production in §41. Finally, in §42 we find the proof that 'The value of R which corresponds to the all-positive p's and q's is the smallest of the values of R' mentioned above. In §72 of *Production of Commodities* we find also the reason why the proof of the positivity of prices provided in §39 does not apply when there is joint production. Finally, an echo of the above reference to fixed capital is found in §84: 'a system which contained no other element of joint production besides what is implied in the presence of fixed capital would in general have an all-positive Standard commodity, thus reproducing in this respect the simplicity of the system of single-product industries' (Sraffa 1960a:73).

7 Alister Watson's help at the time of the corrections of the galley-proofs

Watson was of great help to Sraffa when the galley-proofs of the book manuscript had to be corrected. As mentioned above, on 3 November 1959 Sraffa phoned Alister Watson and 'sent him proofs to read'. In his letter dated 17 November 1959 (*SP* D3/12/112:71–72), which Sraffa received the following day, Watson wrote:

Dear Piero,

I must start with the most abject apologies for having kept your proofs so long. I found it much more difficult than I had expected to give the necessary time to it, and ended up by going sick.

I have no doubt that it should be published.

I have marked a few corrections in the proofs, in ink. Some of these are points where I suspect that the error was in your copying corrections onto this copy. Other suggestions are on separate sheets enclosed.

There are two general points. First, I think that the general treatment of Multiple-Product Industries, in Chapters VII, VIII and IX, is much the most difficult part of the book, and I fear the reader's interest may flag at this stage. Would it be worth while to explain that in the applications that are to follow many of the points are clearer and that these are merely necessary preliminaries?

Secondly, it seems to me so important that you take the rate of profits as variable from the outside that it should be given even more emphasis and explanation (at the end of Chap. V) than it now has. Otherwise, it might be asked, for example, why in §§50 and 57, we should not assume that the number of processes is one more than the number of products, so that everything, including the rate of profits, is fixed. The answer is given only by the rest of the book, but the dynamic role of the rate of profits might be foreshadowed. Many congratulations on finishing the job!

Yours ever

Alister

Enclosed in the letter was a note containing eleven queries (*SP* D3/12/112:74–5). These queries concerned, as we will see, all parts of the book. Sraffa replied on 22 November, as we can see from a minute of his reply (*SP* D3/12/112:77):

Dear Alister,

Thank you so much for your letter & note. I have now gone through the thing again and have adopted your suggestions whenever they could be fitted in easily. One or two are left over, although I entirely accept them, as involving rather more work & more energy than I can muster at the moment.

On looking over this book once more I find it most unsatisfactory, & at the moment I am inclined to suppress it: however, this is a subject on which I have so often fluctuated that I may well change my mind once more & let the printer to go ahead with it.

I am really most grateful for all the trouble you have taken about: whether this thing is to be born or mummified it will be much less bad because of your intervention.

But, when will you come for visit to Cambridge?

The Press received the marked first proofs on 26 November (*SP* D3/12/106). In the Sraffa papers and library there are three sets of first proofs (*SP* D3/12/106–7 and No. 3753; the first is the marked set sent to the publisher, the other two are bound), but in none of them did we find the marks in ink mentioned in Watson's letter. So we cannot compare what Watson really received with the second proofs. Nor can we evaluate the suggested corrections that Watson put directly on the proofs. However, we can analyse the note by Watson and compare the first proofs (*SP* D3/12/106) with the second proofs (*SP* D3/12/108), and, when necessary, with the published book. Sraffa added, in pencil, a question mark to the third and seventh of the queries, a 'no' on the margin of the fourth query, and a typical checking sign to all other queries.

Before we discuss Watson's queries, let us first address the two 'general points' mentioned in his letter. Sraffa took the first one very seriously and, in fact, added a footnote appended to the title of Chapter VII:

> The next three chapters on Joint Production are in the main a preliminary to the discussion of Fixed Capital and Land in chs. x and xi. Readers who find them too abstract may like to move on to chs. x and xi and to refer back when necessary.

This footnote was first inserted as a note 'in not too small type' under the title of Part II in the title page (p. 41, see *SP* D3/12/107), but then, with a letter sent on 4 January 1960, Sraffa decided to have it as a footnote appended to the title of Chapter VII (p. 43, p. 42 being blind). On the contrary, the second general point raised by Watson does not seem to have prompted Sraffa to change the text.

Let us now address Watson's queries. The first query by Watson reads: 'p. 10. §9. Should there not be more discussion of this point?' The published version of section 9 is very brief:

> We shall also hereafter assume that the wage is paid post factum as a share of the annual product, thus abandoning the classical economists' idea of a wage 'advanced' from capital. We retain however the supposition of an annual cycle of production with an annual market.

The only difference with respect to the first proofs is that in here the adjective 'annual' to 'product' is missing: it was only added at this stage. This change does not seem to have been prompted by Watson's query; it only implied bringing the expression in line with the expressions 'annual cycle of production' and 'annual market' used in the second sentence of the section.

The second query refers to section 12. The preceding section introduces the equations of production with the amounts of labour explicitly represented, and in section 12 the 'national income' is taken as numeraire.

The section ends with an observation which, in the first proofs, reads: 'The result of adding the wage as one of the variables is that the number of these now exceeds the number of equations by one and the system is free to move along one of the axes.' Watson commented on this: '"along one of the axes" is inaccurate. Suggest "with one degree of freedom", with perhaps an explanation that if any one of the unknowns is fixed the others will be fixed too.' Sraffa followed the suggestion. In the second proofs as well as in the printed book we read the following sentence, and the appropriate changes are pencilled on the first proofs:

> The result of adding the wage as one of the variables is that the number of these now exceeds the number of equations by one and the system can move with one degree of freedom: and if one of the variables is fixed the others will be fixed too.

The third query refers to the end of section 34, but it is not clear; Sraffa in fact added a question mark. The only change we find from the first to the second proofs is a correction of a misprint.

The fourth query refers to section 37, that is, the section devoted to prove the existence of the Standard system. As the reader will recall, this proof uses an algorithm which consists of the repetition of two steps until the solution is found. (The algorithm may require an infinite number of steps in order to converge.) The first step consists 'in changing the proportions of the industries', the second 'in reducing in the same ratio the quantities produced by all industries, while leaving unchanged the quantities used as means of production' (p. 26). Watson observed: 'It isn't quite obvious that the first type of step can always be carried out.' There are several changes between the first and the second proofs and between the latter and the printed book, but they do not seem to be related to Watson's query. If we look at Sraffa's description of the algorithm, it is clear that the first step can be carried out, but there are many (actually infinitely many) ways to perform it, and from a mathematical point of view the description of an algorithm needs to be uniquely defined.

The problem is to prove that there is a scalar λ and a semipositive vector $\mathbf{q}$ such that for a given semipositive indecomposable square matrix $\mathbf{A}$

$$\mathbf{q}^{\mathrm{T}}[\lambda\mathbf{I} - \mathbf{A}] = \mathbf{0}^{\mathrm{T}}.$$

Sraffa's algorithm can be descibed in the following way:

[i.0] There are $\mathbf{q}_{i-1}^{\mathrm{T}} \geq \mathbf{0}^{\mathrm{T}}$ and $\lambda_{i-1} \geq 0$ such that $\mathbf{q}_{i-1}^{\mathrm{T}}[\lambda_{i-1}\mathbf{I} - \mathbf{A}] \geq \mathbf{0}^{\mathrm{T}}$.

[i.1] Find $\mathbf{q}_i^{\mathrm{T}} \geq \mathbf{0}^{\mathrm{T}}$ such that $\mathbf{q}_i^{\mathrm{T}}[\lambda_{i-1}\mathbf{I} - \mathbf{A}] > \mathbf{0}^{\mathrm{T}}$ and $\mathbf{q}_i^{\mathrm{T}}\mathbf{1} = \mathbf{q}_{i-1}^{\mathrm{T}}\mathbf{1}$.

[i.2] Find $\lambda_i\ (<\lambda_{i-1})$ such that $\mathbf{q}_i^{\mathrm{T}}[\lambda_i\mathbf{I} - \mathbf{A}] \ngtr \mathbf{0}^{\mathrm{T}}$ and $\mathbf{q}_i^{\mathrm{T}}[\lambda_i\mathbf{I} - \mathbf{A}] \geqq \mathbf{0}^{\mathrm{T}}$.

[i.3'] If $\mathbf{q}_i^{\mathrm{T}}[\lambda_i\mathbf{I} - \mathbf{A}] = \mathbf{0}^{\mathrm{T}}$, then end of the algorithm: λ_i and $\mathbf{q}_i$ are the required scalar λ and vector $\mathbf{q}$.

[i.3''] If $\mathbf{q}_i^{\mathrm{T}}[\lambda_i\mathbf{I} - \mathbf{A}] \geq \mathbf{0}^{\mathrm{T}}$, then the algorithm can re-start.

Since the sequence $\{\lambda_i\}$ is decreasing and bounded from below $(\lambda_i > 0)$, it converges to the requested solution.

The steps [i.1] and [i.2] are the two steps mentioned by Sraffa. The second is well defined since

$$\lambda_i = \max_h \frac{\mathbf{q}_i^{\mathrm{T}}\mathbf{A}\mathbf{e}_h}{\mathbf{q}_i^{\mathrm{T}}\mathbf{e}_h},$$

whereas the first step is not well defined: there are infinitely many ways to perform it. Being a mathematician, Watson was understandably concerned about this fact. It is not clear whether Sraffa undestood Watson's concern.[20]

The fifth query by Watson refers to what is now footnote 2 on page 43 of the book. (As we have seen, in order to take account of the first general point raised by Watson, Sraffa added at the stage of the first proofs what is now footnote 1 on page 43.) The footnote in the first proofs reads:

> Incidentally, since the proportions in which the two commodities are produced by any one method will in general be different from those in which they are required for use, the existence of two methods of producing them in different proportions will make it possible to obtain the required proportion of the two products by an appropriate combination of the two methods.

Watson commented on this: 'For "will make it possible…" perhaps "may make it possible…" would be clearer (since negative multipliers may be needed).'

Sraffa changed the text, but in the opposite direction, rendering the meaning less ambiguous and more determinate. Of course, he was aware that negative multipliers may be needed (see §53), but negative multipliers, while permissible in a fictitious construction such as the Standard system, are not so with regard to the actual requirements for use. In fact, in the second proofs as well as in the printed book we read the following sentence, and the appropriate changes are pencilled on the first proofs:

> Incidentally, considering the proportions in which the two commodities are produced by any one method will in general be different from those in which they are required for use, the existence of two methods of producing them in different proportions will be necessary for obtaining the required proportion of the two products through an appropriate combination of the two methods.

With respect to the sixth query, Sraffa again followed Watson's suggestion. This refers to section 63, devoted to the construction of the Standard system in the case of joint production. In the first proofs the comment by Sraffa on the equations defining the multipliers of the Standard system is: 'These equations are of the gth degree, so that there may be up to g possible sets of values or roots for R and the q's; and each set will represent a Standard commodity of different composition.' Watson commented on this: 'Substitute: "These equations give an equation for R of the jth degree, so that there may be up to j possible values of R and corresponding sets of values of the q's; and each set…".'

Sraffa carried out the suggested change (which seems to include a change from g and G to j and J also in the equation, unless these corrections were not pencilled by Sraffa in the set of proofs that Watson received) on the first proofs, but both in the second proofs and in the printed book the passage begins with 'The' instead of 'These'.

The seventh query refers to what is now §79 (it was §78 in the first proofs because Sraffa at that stage divided section 75 into sections 75 and 76). It is devoted to the fact that with fixed capital the reduction to dated quantities of labour is generally impossible. Like the third query also the seventh appears to have been unclear to Sraffa. At any rate, there is no change from the first to the second proofs, and none of the changes from the latter to the book appear to be due to Watson's suggestion.

The eighth query refers to what is now §86 devoted to extensive differential rent. In the first proofs the text was:

> There will therefore be n production-equations, to which must be added the condition that the least productive land pays no rent; and to these equations there will correspond an equal number of variables representing the rents of the n qualities of land and the price of corn.

Watson commented on this:

> Whence does the definition of 'the least productive land' arise, if the order of fertilities is not defined independently of the rents? The answer is perhaps contained in §87, but, if so, a forward reference should be given.

Sraffa followed the suggestion by changing the text and adding a footnote. In the second proofs as well as in the book we read the following sentence, and the appropriate changes are pencilled on the first proofs:

> There will therefore be n production-equations, to which must be added the condition that one of the lands pays no rent;[1] and to these equations there will correspond an equal number of variables representing the rents of the n qualities of land and the price of corn.
>
> [1]By this token only can it be identified as the least productive land in use (cf. p. 75).

This elicits two remarks. First, an answer to Watson's question was not to be found in section 87 (section 88 of the printed book), devoted to an explanation of the relation of rent to 'extensive' and 'intensive' diminishing returns. In this section, in fact, the case of lands of different qualities is considered 'readily recognised' and not really dealt with. Second, although Sraffa's wording was misleading and required some change, the formal exposition was correct. In fact, in the first proofs, as well as in the second proofs and in the book, we read in the same section:

> the condition that one of the rents should be zero can be written
>
> $$\rho_1\rho_2 \ldots \rho_n = 0$$
>
> the relevant solution being always the one in which the ρ's are ≥ 0.

The ninth query by Watson refers to section 89 of the printed book (§88 of the first proofs), devoted to the complication introduced by a multiplicity of agricultural products. In the first proofs we read: 'It may however be noticed that only *one* of the crops could be raised by two separate methods; apart from that, the number of processes would have to be equal to the number of products.' Watson commented: 'There is something wrong with the sense of this as corrected.' Sraffa responded by changing the wording, but not the meaning. In fact, there does not seem to be anything wrong with the passage. However, the adjoint 'as corrected' appears to indicate that what was 'wrong' was a correction pencilled by Sraffa on the set of proofs received by Watson (which we have not found, as mentioned). In the second proofs, as well as in the book, we read the following sentence, and the appropriate changes are pencilled on the first proofs: 'It may however be noted that only for *one* of the crops would two separate methods of production be compatible; for the rest, the number of processes would have to be equal to the number of products.'

The last two queries by Watson refer to section 95 of the first proofs (§96 of the published book), devoted to the choice of technique in joint production. In this case when an additional method is introduced, it is not clear what is the method which is superseded (in single production it is that which produces the same commodity as the additional method). The sentence in the first proofs is: 'And the problem is how to identify among the pre-existing methods the one to which the new method is an alternative.' The comment is: '"And the problem…" It is not made clear enough why this is the problem. (For example, several methods might be superseded together.)'

In response to the problem raised by Watson Sraffa substituted 'is' with 'arises of, without analysing the question more deeply. The last query concerns the footnote on page 87 of the book (page 86 of the first proofs). In the first proofs the footnote reads: 'We assume here that no commodity's price behaves in the peculiar way described in §§71–2.' The 'peculiar way'

consists of the possibility that in joint production a price may fall faster than the wage as a consequence of a change in the rate of profits. Watson commented: 'Why do we have to assume this, and how much of a restriction is it?' Sraffa makes only a small change by adding after 'here' the parenthetical sentence '(and it is essential for the conclusion)'. However, things are much more complex than both Sraffa and Watson were able to recognise at that time (as the following literature has proved; see for instance Salvadori (1985)). It cannot be excluded, of course, that had Sraffa been given the opportunity to pay greater attention to Watson's ultimate two queries, he could have grasped the complexity of the problems involved and found a solution, but this would certainly have been 'involving rather more work & more energy than I can muster at the moment' (*SP* D3/12/ 112:77).

The historical reconstruction provided above shows how Sraffa at the stage of the correction of the galley-proofs went about the comments he got from his mathematical friends. He carefully scrutinised their concerns and suggestions, but he did not always follow their advice. There is a set of cases in which he interpreted the suggestions as indicative of the fact that his presentation needed to be changed in order to avoid possible misunderstandings. The remaining cases are those where he either had difficulty in understanding the concerns of his mathematical friends or considered these concerns as uninteresting from the point of view of an economist. In the latter cases he simply set the problems aside.

8 Excursus: Harry Johnson's correction of a slip

In a letter dated 15 May 1961 Harry G.Johnson wrote to Sraffa: 'I have been working over your book with a class of graduates here. We have come across two places in which we think your argument wrong.' Here we are interested only in his first criticism, which relates to a slip in two of Sraffa's mathematical expressions in the book and with regard to which Sraffa, before replying to Johnson, contacted Watson and Champernowne. (The other criticism refers to the problem of reduction in the case of fixed capital in §79 and derives from a misunderstanding on Johnson's part; it need not concern us.) As regards the slip, Johnson pointed out:

The two formulas in §47 at the top of p. 37 are wrong.

$$\frac{\mathrm{d}}{\mathrm{d}r}\left(1-\frac{r}{R}\right)(1+r)^n = \frac{(1+r)^n}{R}\left[\frac{(R-r)n}{1+r}-1\right]$$

This is zero when $n = \dfrac{1+r}{R-r}$ or $r = \dfrac{nR-1}{n+1}$, as contrasted with your two

formulas. Just as a check, I computed the value of *r* according to my and your formula at which the curves in Fig. 2 should reach their maxima. The results of my formula checked with the figure, whereas your formula gave too high an *r*.

(*SP* D3/12/111:223)

To this Johnson added the numerical calculations referred to.

Sraffa answered Johnson on 21 June (see draft of letter, *SP* D3/12/111:225–6; see also Johnson's reply of 27 June in which he refers to Sraffa's letter dated 21 June, *SP* D3/12/111:227–8). Between 15 May and 21 June the only note in Sraffa's diary concerning this question is dated 22 May: 'written Watson.' On 21 June he listed several people in his diary to whom he had written letters, but Johnson's name is absent. Yet there is some further material in Sraffa's papers related to the issue under consideration. There are three communications by Champernowne and a letter by Alister Watson. Besicovitch does not seem to have been involved in this. The reason for this is probably that during most of the period he was in the United States and, as Sraffa noted in his diary, came back only on 18 June.

As stated, the suggested correction concerns section 47, which is devoted to the pattern of movement of the individual terms of the reduction to dated quantities of labour as distribution changes, when the Standard commodity is used as numeraire. The reader will recall that section 46 introduces the reduction to dated quantities of labour, whereas section 48 uses the results of the preceding section to show that the movements of prices are complex (Sraffa provides the example of the 'old wine' and the 'oak chest'). Let us consider section 47 more closely. The general form of any nth individual term of the reduction, when the Standard commodity is used as numeraire, is:

$$L_n\left(\frac{R-r}{R}\right)(1+r)^n .$$

It is shown in the section that if $n \leq 1/R$, then such a term is a decreasing function of r (in the relevant range $0 \leq r \leq R$), otherwise it is first increasing and then decreasing. The maximum is obtained for the values of r and n that satisfy the equation obtained by setting the derivative with respect to r equal to zero:

$$\frac{L_n}{R}\left[-(1+r)^n + n(R-r)(1+r)^{n-1}\right] = 0 .$$

The corrected values in the relevant range are those determined by Harry Johnson and his students. Instead the values we find in the 1960 book are:

$$n = \frac{1}{R-r}$$

$$r = R - \frac{1}{n}.$$

There appears to be only one way to obtain these wrong expressions, that is, by failing to reduce the power of the second term of the derivative from n to n-1. And Alister Watson thought this could have been the origin of the slip. Confronted with the riddle, he apologised to Sraffa for having overlooked the latter when reading the proofs. In a letter dated 9 June 1961 he wrote:

> Dear Piero,
> I am sorry I have taken so very long in answering your letter–which is not due to the difficulty of the questions, but only to my delay in getting round to have a proper look at them.
>
> Johnson is quite right about the first point. I find the formula $r=\frac{nR-1}{n+1}$ in my own notes, but tucked away so that I obviously hadn't thought of drawing your attention to it & it never occurred to me to check the passage in your book. The slip is made rather less important by the fact that the last sentence of the paragraph is, in any case, correct.
>
> I haven't been able to think of any particularly plausible way in which the slip occurred–it does amount in a way to replacing n+1 by n & this could have been done I suppose by Besicovitch in a hurry.
>
> As for the last point, I suppose you are right in your interpreatation of Johnson's meaning. I don't know if it would be of interest, either to you or to him, but I have recently come across a paper giving a brief statement and bibliography of the theorems of the type you prove and use that have been dealt with mathematically.[21] This might perhaps help to make clear to him that others besides yourself have thought it necessary to prove such things and that they are distinct from the simpler result he quotes.
>
> It was good to hear about the reviews: it certainly seems as if some interest is being taken in your work, in particular, that the market hasn't been spoilt by the 'games theory' type of attack that is so fashionable.
>
> Yours ever
> Alister Watson
> (*SP* D3/12/111:456–7)

Watson's interpretation is not implausible per se: it could have been Besicovitch who, 'in a hurry', had blundered. Watson was willing to assume some responsability for the fact that the slip had crept into the published text.

Yet in the light of the further material available to us, Watson's interterpretation cannot be sustained. Let us first turn to Champernowne's reaction when confronted with the problem by Sraffa.

On 31 May Champernowne sent Sraffa the following note (*SP* D3/12/111:462):

> Dear Sraffa,
> The formula still doesn't seem to come out to $n = \frac{1}{R-r}$ but to $n = \frac{1+R}{R-r}$ when wages are advanced: conversely the formula for r becomes
>
> $$r = R - \frac{1+R}{n}.$$

Apparently, Sraffa was not of the opinion that this answer settled the question. On 2 June Champernowne sent another note (*SP* D3/12/111:460), writing, among other things:

> I return HGJ's letter. He is right on the first point. I can't follow your argument which he attacks in his second point–but I gather from you that you could cope with that one.
>
> I keep trying to get your answer relating to the first point by assuming labour paid in advance but although I keep getting contradictory answers I never seem to get yours.
>
> Tomorrow I get my examination scripts so I would like to stop thinking about the production of commodities by commodities.
>
> Yours sincerely
> D.G.Champernowne

A card from Champernowne to Sraffa dated 20 June 1961 is again on this problem: 'A possible explanation of the *R* not appearing as denominator in Besicovitch's expression would be that he took as unit of value the total capital or (same thing) total input: where as you took as unit of value the net national income' (*SP* D3/12/111:230). The reference is not directly to Johnson's letter, but seemingly to an old note by Besicovitch. The idea is close at hand that in that note Besicovitch had put forward a calculation using a different amount of the Standard commodity (i.e., 'total capital') as unit of value. However, also this explanation does not settle the case, because a change of the kind indicated affects the derivative in the sense that it is now multiplied by a positive constant, but this change does not affect the relationship between r and n obtained by the condition that the derivative equals zero.

Champernowne's communications to Sraffa reflect that he and Sraffa took pains to understand the origin of the slip. They first checked what happens if wages are paid *ante factum*. Although the answer is different from the one

when wages are paid *post factum,* it is also different from the published one. Then Sraffa, scrutinising his papers, appears to have found an old note in the hand of Besicovitch where R is missing in the denominator. This fact was interpreted, but it did not disclose the origin of the slip.

Folder D3/12/62 contains the material Sraffa had grouped under the heading 'Fluctuations of price with variations of r'. The first part of D3/12/62:2 is in Besicovitch's hand and provides the calculation starting from the *non-constant* part of the nth individual term of the reduction, which in the document is indicated with letter *'I':*

$$I = (R - r)(1+r)^{n-1}.$$

This is clearly the document at which Champernowne hinted: the wages are supposed to be paid *ante factum* and there is no R in the denominator. (This however does not seem to be a consequence of a different amount of the Standard commodity being used as numeraire, but just of setting aside the constants which do not affect the result.) The findings obtained are correct and it was certainly not Besicovitch who 'in a hurry' blundered. This document has no date, but there is an insertion in it dated 1/12/42 whose first part is also in Besicovitch's hand. In the same folder we find two notes written by Sraffa on 28 and 29 December 1956, respectively, which refer to the issue and here we find the origin of the blunder. The first note (28 December 1956) reads:

> The relation of r to w was different in 1942 from what it is in 1956. (r was a linear function of $w(1+r)$ in 1942 and it is…[a linear function] of w in 1956).
>
> In 1942 the formula
>
> $$Lw(1+r)^n = \frac{(R-r)(1+r)^{n-1}}{R} = \frac{(1+R)(1+r)^{n-1} - (1+r)^n}{R}$$
>
> because not w, $w(1+r)$ was a linear function of r, and therefore was replaced by
>
> $$\left(1 - \frac{r}{R}\right).\square$$
>
> In 1956 the formula of the relation is
>
> $$r = R(1-w),\ w = 1 - \frac{r}{R}\square$$
>
> so that […] $Lw(1 + r)^n$ becomes $(1 - \frac{r}{R})(1 + r)^n$ [. . .]
>
> (*SP* D3/12/62:5)

Hence Sraffa was clear that a change of assumption from an *ante factum* to a *post factum* paid wage implied a change in the formula, but apparently he did not ask one of his mathematical friends to obtain the new formula for him. Probably he thought that it would be enough to substitute 'n+1' for 'n' in the original formula. (If that had indeed been the case he was not consistent in applying that rule.) The second note (29 December 1956) reads:

> The (1942 Besicovitch) rules becomes (1956 form):
>
> 1) In general $Lw(1 + r)^n$ has its maximum value when $r = R - \frac{1}{n}$:
>
> 2) Therefore, when $R - \frac{1}{n} \leq 0$, then $Lw(1+r)^n$ has its maximum value for $r = 0$ and decreases steadlily as r increases (i.e. when the 'age' of the labour term is equal to, or smaller than, the number of years purchase of the maximum rate of profits) i.e. where $n \leq \frac{1}{R}$
>
> 3) The term which is at its maximum value when r is a given value, say r_0, is that whose 'age' is
>
> $$n = \frac{1}{R - r_0}$$
>
> 4) The maximum value of my is $Lw(1+r)^n$
>
> $$\frac{1}{Rn}\left(1 + R - \frac{1}{n}\right)^n.$$
>
> 5) The rate of profit at which any n-th term reaches its maximum value is equal to the difference between R and the rate of profits of which its own period n is the purchase period, viz. $R - \frac{1}{n}$.
>
> (*SP* D3/12/62:1)

Now knowing what happened, let us turn to the correspondence with Harry Johnson. In his reply Sraffa left no doubt who was to be blamed for the slip:

> Of course you are right about the formulas in §47, p. 37.I have looked up my notes to see how it came about (it is the digging up of the old notes that has delayed my reply): I find that the correct formula was worked out for me by Besicovitch twenty years ago, but in preparing the book I made a minor change of assumption & in adapting the formula to this I blundered. Fortunately the diagram, as you say, was based on the correct formula; & so is the conclusion in the last sentence of §47.

Sraffa followed essentially the same route when amending the text on the occasion of the 1963 reprint of the book. Here we find the correct formulas on page 37:

$$n = \frac{1+r}{R-r}$$

$$r = R - \frac{1+R}{n-1}$$

plus a note appended to the preface (p. vii):

> The only change made in the present reprint (1963) has been to correct the expressions for n and r at the end of §47, p. 37, which went wrong in a last-moment change of notation. No alteration has been necessary in the corresponding text (p. 37) and diagram (fig. 2, p. 36) which were based on the correct formulas.

Obviously, one must not interpret Sraffa's remark as meaning that there is a 'notation' for which the formulas in his book would be correct. The meaning rather appears to be that in adapting the correct formulas to a change in a premiss regarding the payment of wages, Sraffa had slipped.[22] According to our reconstruction Sraffa correctly described what has happened.

9 Conclusion

The chapter has dealt with Sraffa's collaboration with his mathematical friends Frank Ramsey and Alister Watson. The assistance of these mathematicians was of great importance to him. The material presented from Sraffa's hitherto unpublished papers and correspondence testifies to the independence of Sraffa's mind and his scepticism as regards all propositions he could not master in his own way. Although he sought the help of mathematicians, he did not put his lot in their hands, so to speak. He would carefully listen to them when they talked and jot down summary accounts of the discussions he had with them; he would ponder over their notes and proofs, their statements about whether a problem he had put to them was solvable or not, and what the solution was, if there was one; but he would remain sceptical until he had finally understood the correctness or otherwise of the answer given or the fruitfulness of the avenue indicated by them, thinking through the problem himself and applying his own mental tools and ways of reasoning. He did not use, or trust per se, abstract mathematical reasoning and would not himself employ mathematical tools other than elementary ones. Sraffa's fastidiousness, it seems, was certainly an obstacle to the progress of his work but probably also a precondition of the latter's excellence.

Notes

1 We should like to thank Pierangelo Garegnani and Ian Steedman for valuable comments on an earlier draft of this chapter. We are also grateful to Jonathan Smith, archivist at the Wren Library of Trinity College, Cambridge, who catalogued Sraffa's papers, for his assistance throughout our work on this project.

2 We are grateful to Pierangelo Garegnani, literary executor of Sraffa's papers and correspondence, for granting us permission to quote from them. References to the papers follow the catalogue prepared by Jonathan Smith. Unless otherwise stated, all emphases are in the original.

3 Nerio Naldi kindly informed us that in Sraffa's former flat in Rapallo there are several mathematical exercise and high school books. We still have to check this material.

4 In February 1930 Sraffa was assigned by the Royal Economic Society the task of editing David's Ricardo works and correspondence. As we know, Sraffa immediately took up the work and put a lot of effort into it. However, for a while he appears to have been of the opinion that he could carry on with his constructive work, albeit at a much reduced speed. Therefore, we find documents also after February 1930. Yet soon Sraffa appears to have been overwhelmed with the new task, which absorbed all his energy and forced him to interrupt his constructive work. It goes without saying that his editorial work generated noticeable positive externalities to his constructive work, both conceptually and analytically.

5 The discovery of Ricardo's letters to James Mill in 1943 and their full availability in 1945 directed Sraffa's attention away from his constructive work and toward his editorial work, with the main body of *The Works and Correspondence of David Ricardo* being published between 1951 and 1955.

6 On 24 January 1950 Sraffa noted in his diary: 'Besicovitch elected prof, (on his birthday.'

7 On 3 October 1955, 10.30–13, he met Togliatti in Rome. In his diary Sraffa noted in brackets: 'dettagli del mio libro: con. Marx restato all' 800' [details of my book: with Marx left in the XIX century].

8 Peter Swinnerton-Dyer (born in 1927) was a Research Fellow in Mathematics in Trinity College, 1950–4, and later became a Professor of Mathematics at Cambridge University.

9 John Arthur Todd (1908–98) was a Lecturer in Mathematics in the University of Cambridge, 1937–60, and a Reader in Geometry; he was a Fellow and then the Master of Downing College.

10 See also Samuelson's recollection of the event in Kurz (2000:113).

11 Bryan Birch was a fellow of Trinity between 1956 and 1960 and had the set of rooms above Sraffa's in Neville's Court. He is presently Professor at the Mathematical Institute, Oxford.

12 Unfortunately, we have not yet been able to identify Carlo Brunner.

13 We know from Paul Samuelson that in the spring of 1960 he received from Cambridge University Press page proofs of Sraffa's book. In the letter accompanying the proofs he was asked: 'Shall we bring out a separate American publication?', to which he replied 'in enthusiastic affirmation'. We also know from Samuelson that he showed the proofs to Solow, who, however, did not really study the book at that time. See again Samuelson's recollection in Kurz (2000:113).

14 On 29 May he and Joan went for a long walk. Sraffa's diary notes: '2–7. Joan walk Hardwick e discusso, ahimè, il mio lavoro! [and talk about, alas, my work].' However, all's well that ends well: as Sraffa added, later that day they had 'champagne in hall'.

15 The correct Italian title is *Produzione di merci a mezzo di, merci.* The Italian word 'mezzo' has both the meaning of 'means' and that of 'half: Sraffa has taken advantage of this.

16 One is inclined to say that Sraffa was forced to develop that theorem himself. As we have argued elsewhere, Sraffa's demonstration of the existence and uniqueness of the 'Standard commodity' in the case of single production can be considered a (not fully complete) proof of this theorem (see Kurz and Salvadori 1993).

17 In *SP* D3/12/2 there are three small sheets with symbols and figures in Ramsey's hand, but they seem to be of no use in the present context.

18 In Sraffa's papers there do not seem to be records of the meetings between the two in 1945, 1948 and 1949.

19 We will come back to this procedure in the next section.

20 A simple way to find a well defined algorithm is to set

$$\mathbf{q}_i^{\mathrm{T}} = \frac{\mathbf{q}_{i-1}^{\mathrm{T}}\mathbf{l}}{\mathbf{q}_{i-1}^{\mathrm{T}}\left(\mathbf{I}-\mathbf{A}\right)^{-1}\mathbf{l}}\,\mathbf{q}_{i-1}^{\mathrm{T}}\left(\mathbf{I}-\mathbf{A}\right)^{-1}.$$

21 On the top of the letter Sraffa wrote in pencil: 'yes, send bibliography', but we were not able to trace it in his papers. It is quite possible that the 'bibliography of the type of theorems' Sraffa is said to have proved refers to the Perron-Frobenius Theorem. If so, then Watson's hint may thus be interpreted as rendering some additional support to our above claim that Sraffa did not know that this theorem existed because his mathematical friends had not drawn his attention to it.

22 It should be noted that in later reprints of the book the note does not reappear.

18 Monetary analysis in Sraffa's writings

Carlo Panico[1]

Introduction

According to Samuelson (1987), Sraffa's work on Ricardo's writings and on the analysis of prices and distribution deserves an outstanding place in the history of economics. He worked on these subjects for most of his life after 1923 when, hoping to be appointed as lecturer, which he was in October at the University of Perugia, in Italy, he began a study of Marshall's partial equilibrium, which led to the publication of two articles in *Annali di Economia* and in the *Economic Journal* (Sraffa 1925a; 1926a).[2]

Before 1923, Sraffa's attention had been attracted by some applied monetary problems.[3] His interest in monetary analysis remained vivid in the subsequent years,[4] when he published in the *Economic Journal* a critical review of Hayek's book, *Prices and Production,* and participated intensively in discussions on Keynes' *Treatise on Money* and *General Theory*.[5]

The links between his earlier work on money and his later writings on Ricardo and on prices and distribution have already been pointed out by the literature.[6] This has clarified the origins of Sraffa's theoretical interests, the homogeneity over time of his views on the factors affecting income distribution and his fruitful intellectual relation with Keynes.[7] Moreover, this literature has shown the error of considering Sraffa's contributions as 'merely abstract exercise in pure theory'.[8]

The aim of this chapter is to verify the validity of the conclusions reached by the literature in the face of the evidence, known as the *Sraffa Papers (SP),* which have recently become available at the Wren Library of Trinity College in Cambridge. It will be argued that this evidence confirms the conclusions reached by the literature, providing a more complete and articulated picture of the positions held by Sraffa and of his intellectual itinerary.

1920: the honours thesis

Sraffa's first publication, *L'inflazione monetaria in Italia,* was the honours thesis he defended in 1920 at the University of Turin (Italy) for his graduation. The work, supervised by one of the most prominent Italian economists, Luigi

Einaudi,[9] was published the same year by the *Premiata Scuola Tipografica Salesiana* of Milan. In order to clarify some problems related to monetary policy measures that Italy was taking in that period, the thesis deals with some of the most discussed themes in the monetary literature of the time: the causes and the consequences of inflation, the stabilisation of internal prices and exchange rates within an unstable international financial system, and the arguments for restoring the gold standard and revaluing the currency to the pre-war gold parity. These were the central themes of the International Financial Conference, organised in 1920 in Brussels by the League of Nations,[10] and of the International Economic Conference, called by the Supreme Council of the Allies and held in Genoa in April 1922. During the latter conference, which he covered for the *Manchester Guardian Commercial,* Keynes worked out on this matter a position that he presented first in the same journal and then in *A Tract On Monetary Reform* in 1923.[11]

Sraffa's thesis is an entirely applied work which does not contain any theoretical sections. Yet, a theoretical position can be recognised in its pages. This position, which has similarities to and differences from that proposed by Keynes in *A Tract on Monetary Reform,*[12] clarifies the views held by Sraffa at the time on the working of the monetary system and on the factors affecting income distribution.

Like Keynes in *A Tract,* Sraffa adopted, without doubting its validity, the quantity theory of money in the form dominant at the time.[13] The role of money as a 'reserve of value' was recognised; yet no reference was made to the speculative motive in the sense used in *A Treatise on Money* and in *The General Theory*. In *A Tract,* as in Sraffa's thesis, speculation was conceived as forecasting future changes in commodity prices and in exchange rates, rather than forecasting future changes in the value of securities.

Unlike Keynes, who thought that monetary factors has temporary and exhaustible effects on income distribution,[14] Sraffa implicitly suggested that monetary phenomena, like inflation and deflation, have permanent effects on the social conflicts which regulate the *equilibrium* or *normal* real wage rate.[15] This view, whose theoretical foundations Sraffa had not yet explored, was closer to those of the Classical political economists and of Marx, which Sraffa studied in the subsequent years, than to the tradition which was dominant at the time. According to the Classical political economists and Marx, income distribution, and in particular the determination of the real wage rate, is influenced by economic, historical and institutional factors, including the conflictual relations between social classes. The complexity of the phenomena linked to income distribution, it was thought, is better studied by separating this analysis from that of the relative prices of commodities. As a consequence, following these authors, it is possible to describe how social conflicts affect state intervention and to attribute to the latter a permanent influence on the *equilibrium* or *permanent* level of distributive variables.

In Sraffa's thesis the presence of this approach can be seen in the claim that monetary policy can have a permanent influence on income distribution. It testifies to the adoption of a standpoint, which will be here called 'conventionalist', according to which the *level* of the economic variables under examination is not determined by natural or material forces, such as the availability of the factors of production in the neoclassical theory of distribution, but can establish itself at any level considered *normal* by the common opinion[16] and can be affected by the decisions taken by the monetary and other authorities.

In the thesis Sraffa also presented the view, close to that held at the time by Fisher and Cassel, that the *equilibrium* value of money is determined by what common opinion considers normal.[17] In opposition to those who were in favour of a return to the pre-war gold parity, Sraffa claimed that it is not possible to consider the negative consequences of a revaluation of the currency on the economy and on income distribution 'as a necessary evil to re-establish "normal' conditions" (Sraffa 1920:42. English translation 1993:24). The pre-war gold parity, he claimed, could be considered 'no more "normal" than any other value of money' (Sraffa 1920:42. English translation 1993:24). In general, he concluded by making reference to both deflation and inflation:

> the value of money is normal when forces which tend to make it change are absent. Thus, had the required time to let prices adjust to circulation elapsed, a new equilibrium would have established and the value reached by money would be perfectly normal.
>
> (Sraffa, 1920:42. English translation 1993:24)

At the time of the thesis, the distance from neoclassical theory and the affinity with Classical and Marxian positions on income distribution were not the result of a profound knowledge of the analyses proposed by these schools of thought. Only in subsequent years did Sraffa develop a systematic study of the different theories of value and distribution and become fully aware of the differences among them.

1922: the essays on the Italian banking crisis

The interest in applied monetary problems and in the effects of policy measures on income distribution attracted the attention of Sraffa after graduation too. To improve his knowledge of the working of the banking system, he first spent a few week in a bank as an apprentice.[18] Subsequently (spring 1921) he went to London to study at the London School of Economics, where he further studied monetary economics, as some documents of the *Sraffa Papers* (D1/18) show. At the same time, he collected information on the conditions of life of the English and American working

classes, writing on this theme three articles for the newspaper of the Italian Communist Party *Ordine Nuovo*.[19] Finally, in August 1921, he went to Cambridge to meet Keynes.[20] One result of their meetings was that Keynes asked Sraffa to write an article on the crisis that was striking the Italian banking system for the Weekly Supplement of the *Manchester Guardian Commercial,* which dealt with the monetary and financial problems of the post-war reconstruction in Europe.[21] The article Sraffa wrote was too long for the *Manchester Guardian Commercial,* but suitable for the *Economic Journal,* where it was published in June 1922, while a shorter article, subsequently written by Sraffa, took its place in the 7 December issue of the *Manchester Guardian Commercial.*

The articles contain a detailed account of the mechanisms through which one of the four major Italian banks, the *Banca Italiana di Sconto,* was led to close its doors at the end of 1921. They also describe the measures taken by the authorities to avoid the closure of other major banks and the way in which the former replaced the latter in financing the industrial sector during the years of the crisis. The work is descriptive and applied. Yet the final part of the article in the *Economic Journal* presents some general propositions referring to the features of the different banking systems and proposing the view that the operation of a mixed banking system is more suitable than that of a specialised one for the development of industry, since it is more oriented towards the channelling of funds to industry. Moreover, Sraffa argued that the major problem of a mixed banking system is not the high risk of a liquidity crisis, due to the assets-liabilities structure of the banks' balance sheets,[22] but the embroiled relations between banks and industries and the consequent formation of large groups or 'concentrations', strongly intermingled through the reciprocal holding of shares and the appointment of directors, able to control relevant sections of the economy, of the media and of the political world, as the events related to the *Banca Italiana di Sconto* showed. As Sraffa noticed, the operations leading to the banking crisis were set in motion by the economic problems of the *Società Ansaldo* of Genoa, a leading metallurgical firm who had control of the *Banca Italiana di Sconto*. Through the detailed description of these events, Sraffa provided a reconstruction of the complex interactions among the industrial sector, the financial sector and the political world. He underlined how government intervention is part of the conflict among different economic and social groups and suggested to the reader some general considerations on the formation of monetary policy measures and on their effects on income distribution.

These parts of the articles also clarify the links and the differences between Sraffa's position and that of the Marxian tradition that was then prevailing. With respect to this tradition, Sraffa's analysis is not based on a rigid class division, but acknowledges the existence of conflicts within the capitalist class and claims that these conflicts, like the autonomous interests of political and administrative bodies, can affect economic policy decisions. Moreover, it

underlines the danger for democracy coming from the formation of large groups or 'concentrations', able to disguise from the majority of the population the aims of their initiatives and to influence the exertion of power.

> The general tendency seems to be towards the…formation of large 'groups' of companies of the most varied kinds concentrated around one or more banks, mutually related by the exchange of shares and by the appointments of Directors common to them. Within these "groups" the various interests are all equally subject to the interests of a few individuals who control the whole group…. Very little is known…about these groups…. What the public knows and feels…is the enormous financial and political power which they have and the frequent use they make of it to influence both the foreign and home policy of the government in favour of their own interests. Each group keeps several press organs which support its policy, and some of the accusations made against certain Ministries of being actuated by the interests not of a class, but of private concerns, and of favouring one financial group against another, have no doubt a basis of truth.
>
> (Sraffa 1922a:196)

In the description of the events leading to the banking crisis, Sraffa (1922a: 191–2) pointed out in which cases the Italian government, 'blackmailed by a band of gunmen or a group of bold financiers' (Sraffa 1922a:197), acted to protect powerful pressure groups at the expense of society as a whole. Moreover, he disclosed that the *Banca Italiana di Sconto* had funded fascist groups (Sraffa 1922a:191) and the fact that two lawsuits for illegal financial operations and unlawful speculation had been initiated against the managers of some large Italian banks, who had been acting in such a way as to avoid the court's sentence:[23]

> In consequence of these facts, two lawsuits were begun: one against the directors of the Banca Commerciale and the Credito for illegal purchase of their own shares, the other against the directors of the Banca Commerciale, Banca di Sconto and Ansaldo for the *aggiotaggio* (illegal speculation) carried out in the operations on the Bourse in March 1920. As all these companies had been provident enough to appoint some Senator on their Board of Directors, these lawsuits had to take place, as prescribed by the Constitution of the Kingdom of Italy, before the Senate constituted into a High Court of Justice, and on account of the slowness of these proceedings the lawsuits are still dragging on.
>
> (Sraffa 1922a:184)

The two 1922 articles, particularly that in the *Manchester Guardian Commercial,* caused irritation in Italian financial and political circles, and resulted in some

quarrelling with one of the major banks,[24] and a series of troubles with the fascist regime, which forced Sraffa to leave the country temporarily. Keynes, informed by Sraffa himself, invited him to come to England until things had calmed down. The invitation, however, could not be exploited because the English authorities, when he arrived in Dover, did not give him leave to land.[25]

1923–7: other writings on monetary policy

The study of the formation of economic policies and of the effects of these policies on income distribution continued to attract Sraffa's attention in the subsequent years, as his published writings[26] and some documents of the *Sraffa Papers* show. In a 1923 manuscript,[27] Sraffa described the Italian monetary policy arguing that the autonomous interests of the Fascist Party had played a role in its formation. In the same document he further examined, as he had done in the 1922 articles, the measures taken to deal with the financial crisis of the *Banca Italiana di Sconto* and of some industrial sectors. In the final part of the document, Sraffa examined the exchange rate, as he had done in his honour thesis, writing some notes on the trend of the Lira.

The evolution of the Italian exchange rate was considered in three other papers of the 1920s, which were addressed to readers with a different cultural and political background. The first, addressed to readers with a social-liberal background, was published in 1923 in the magazine *La Rivoluzione Liberale,* founded by Gobetti.[28] It is a comment, short but very critical, on a note coming from Fascist sources and published in the newspaper *Il Popolo d'Italia.*

The second is an exchange of correspondence between Sraffa and Tasca,[29] published in the monthly magazine of the Italian Communist Party *Stato Operaio* in the issue of November-December 1927. Sraffa's letters, which comment on Tasca's position on the causes leading the Italian government to the revaluation of the Lira, warn the readers of the danger of over-simplified interpretations of economic events, showing some differences between Sraffa's views and those of the Marxian tradition, differences which were already evident in the 1922 articles.

Tasca's interpretation of the events related to the trend of the Italian Lira argued that the revaluation policy, adopted in August 1926, was introduced to favour the *immediate* interests of the dominant economic groups. Sraffa disagreed with this position and wrote:

> In general, it seems a mistake to me–and a very dangerous one–to believe that every single action of the fascist government (and of any capitalist government) is *directly* dictated by the *immediate* interests of the banks and of the big industrialists.
>
> (Sraffa 1927b:1089; italics in the original; our translation)[30]

For him, the independent interests of political and administrative bodies can be able to have a relevant role in the formation of economic policy. Thus, his interpretation of the events related to the trend of the Lira was that in 1926 the fascist government had turned in favour of the revaluation to gain the support of the middle classes and of part of the working classes and so avoid the danger of the social isolation that it was facing. This plan, however, was contrasted by the financial and industrial groups damaged by the revaluation.[31]

The third essay is the text of a conference held in England on 3 November 1927 for the Emmanuel Society (see *SP* D2/3). The conference shows Sraffa's interest in the attempts of the monetary authorities to stop through administrative measures the devaluation of the Lira of March 1926. According to the authorities, these measures had to stabilise the exchange without generating the negative consequences of a deflationary policy. Unfortunately, Sraffa argued, in August 1926 the fascist government declared its intention to revalue the currency.[32] This declaration defeated the efforts of the monetary authorities and from August 1926 to March 1927 the Lira increased its value by 90 per cent with devastating consequences for the economy and on income distribution. These consequences were examined by Sraffa in the final part of the conference. On that occasion he claimed that, given the intensity of the depression occurring, the revaluation policy had benefited nobody, not even those groups, like the rentiers, who normally take advantage of the rise in the value of the currency. Thus, he concluded the conference by inviting the audience to inquire, without however answering these questions, on the motives that had led the fascist government to follow such a damaging road.

Thus, the problems relative to the formation of the economic policy and to its effects on income distribution were still at the centre of Sraffa's writings of the period 1923–7. In these writings the conventionalist idea, close to the Classical and Marxian approach, that government intervention has permanent effects on income distribution is still present.[33] In those years too, however, Sraffa was not yet fully aware of the differences among the theories of distribution of the different schools of thought, nor was he conscious of the implications of the neoclassical theory and of the critiques that he would later propose in *Production of Commodities*. This is confirmed by a document of the *Sraffa Papers* (D1/15), which criticises the positions held by Cole and Dobb[34] by using the neoclassical idea that the relative scarcity of the factors of production capital and labour determines income distribution:

> They [Cole and Dobb] imagine that a state of things in which capital is scarce (which is the same as saying that labour is abundant) has in itself a sort of permanency or necessity. They fail to see that it is quite as possible to have a state in which abundance of capital and scarcity of labour

> prevails. In fact, it is the capitalistic method of accumulation that prevents the formation of sufficient capital. The only really final objection to capitalism must be found in its inability to achieve its own goal–Capitalism fails to capitalise. The very mechanism through which saving is brought about is so framed that it actually prevents saving to reach the socially desirable level: as it is the interest from old savings that provides the source for new savings, so soon as accumulation increases and the total of savings begins to approach the optimum amount, interest falls and thus sets back saving to the old amount. The optimum accumulation can never be reached.
>
> (*SP* D1/15)

1927–32: discussions on the *Treatise* and the review of *Prices and Production*

When Sraffa moved to Cambridge at the end of 1927, Keynes was working on *A Treatise on Money,* and the possibility of discussing with him the problems raised in this book furthered Sraffa's interest in money and banking. As a result of these discussions, Sraffa worked more on monetary theory than on monetary policy. In those years, however, he became more and more involved in other areas of research, like the history of economic thought and the analysis of prices and distribution.[35]

The participation of the Italian economist in discussions on *A Treatise,* before and after its publication, is documented by the literature[36] and is confirmed by the *Sraffa Papers*. In them, beside some writings on the discussions held between January and May 1931 during the meetings of the Circus and the preparatory writings of the review of Hayek's *Prices and Production* (*SP* D1/70–85 and D3/9), is significant what J.Robinson wrote–'To the Circus Master–May Term 1931'–on the cover of the abstract that she presented to Sraffa of her 'A parable on savings and investment' published in 1933 in *Economica.*[37]

The discussions on *A Treatise* gave Sraffa the opportunity to deepen his knowledge of the most debated themes of those years: the quantity theory, the theory of the trade cycle, the theory of interest. The *Treatise* moved within the neoclassical tradition.[38] It had not rejected the separation between a 'real' and a 'monetary' department of economics, with 'real' forces determining the equilibrium position of the economy and the 'monetary' department studying its cyclical movements. In its treatment of the quantity theory, whose validity was not denied, the *Treatise* took as given the equilibrium levels of the distributive variables, including the 'natural' interest rate, assuming that they were determined in the 'real' department of economics on the basis of the relative scarcity of the factors of production. None the less, Keynes' work introduced some new elements. Beside integrating the notion of liquidity preference, it extended the influence of monetary factors within the dominant

approach by recognising that, through 'forced savings', monetary factors could have some influence on the 'real' factors determining the equilibrium position of the economy and that, owing to the complex operations of financial markets, the market interest rate could diverge from its natural or equilibrium level for long periods of time.

In Sraffa's writings of those years there is no explicit critique of this approach. His monetary analyses do not differ significantly from those of Keynes; his analyses on prices and distribution, on the other hand, do not state clearly his position, which, as in previous years, does not seem yet aware of the critique of the neoclassical theory that he will propose in *Production of Commodities*.

In the review of Hayek's *Prices and Production,* the most important of Sraffa's essays of those years, the Italian economist follows closely Keynes' approach to monetary questions. He adopts the quantity theory in the version presented in *A Treatise* (Sraffa 1932a:42 and 46–9) and, like Keynes, he acknowledges the difficulties faced by the monetary authorities in identifying, in the complex operation of financial markets, the equilibrium rate of interest, representing 'the ideal maxim for monetary policy' (Sraffa 1932a:49–51 and 1932b:251). Finally, always in line with the English economist, he criticises Hayek's conclusions on the effects that a credit inflation, with its consequent formation of 'forced saving', can have on the equilibrium position of the economy.

According to Hayek, 'forced saving', unlike voluntary saving, does not produce any permanent effect on the economy. For him, the intervention of the banking system, that sets inflation in motion, introduces an 'artificial' element in the 'natural' operation of market processes.[39] As soon as this artificial disturbance ceases, inflation stops and the behaviour of the agents, no longer influenced by 'unnatural' elements, reproduces the initial situation.

To criticise this conclusion, Sraffa claimed that it is in contrast with common sense. During a period of inflation, he said,

> one class has, for a time, robbed another class of a part of their incomes; and has saved the plunder. When the robbery comes to an end, it is clear that their victims cannot possibly consume the capital which is now well out of their reach.
>
> (Sraffa 1932a:48; see also 1932b:249)

In this passage it is possible to recognise the idea, already expressed by Sraffa in his honour thesis and in the subsequent writings, that monetary policy and inflation are part of the social conflicts which contribute to the determination of income distribution. Yet, in spite of the relevance of this point for the subject under discussion, the review does not clarify which theory of distribution is adopted, nor is any critique advanced to the neoclassical

theory. Moreover, there is not even a critique of Hayek's analysis, recalled by Sraffa (1932a:45), of the relationship between the amount of capital employed in the economy and the length of the production period. The lack of this critique suggests that in those years Sraffa had not yet acquired awareness of the logical inconsistencies of the neoclassical theory of capital, that he will describe in *Production of Commodities*.

The documents of the *Sraffa Papers* related to the preparation of the review also underline that the Italian economist noticed that Hayek's book did not follow the traditional approach of the textbooks on monetary economics.

> According to Dr H. the task of monetary theory 'is nothing less than to cover a second time the whole field which is treated by pure theory [i.e. what is usually called the theory of value and distribution] under the assumption of barter, and to investigate what changes in the conclusions of pure theory are made necessary by the introduction of indirect exchange' (p. 110). This is not the common opinion, and it does not correspond to the line along which in fact the division of labour between monetary and non-monetary economists takes place. It is sufficient to refer to a standard treatise on value and distribution, e.g. to Marshall's *Principles,* to see that these theories are expounded directly in terms of a monetary economy: and this is true even in the case of one like Marshall who rightly or wrongly thought that the consideration of money was essential to the truth of his conclusions (see *Principles,* Appendix on Barter). And it is sufficient to refer to a standard book on money, e.g. Keynes' *Treatise,* to see that it is the subject matter, or the field covered, that is almost entirely different.
>
> The division line, which appears to be asserted more and more definitely, is another one. The non monetary theory studies a state of equilibrium, and the conditions which determine it: it goes as far as comparing two or more states of equilibrium, and measuring the differences in their conditions–but goes no further. Here begins the field of monetary theory: or rather, jumping over the study of the path followed in the transition from one position to another, it sets to study states of disequilibrium. I suppose that every monetary economist today regards trade fluctuations as his exclusive subject.
>
> (*SP* D3/9:181–2)

Hayek's book was linked to the literature of the time that introduced some changes in the notions of equilibrium used in economics.[40] Yet neither the documents of the *Sraffa Papers* nor his published works show that he had perceived the existence of these changes. In examining the problems related to the treatment of the 'own interest rates', that Sraffa (1932a: 49–50) called 'natural rates',[41] he noticed that *in any moment* there can be as many own rates as there are commodities. For Sraffa (1932a:49) none of these rates can be the

equilibrium one. Hayek (1932a:245), instead, claimed that in equilibrium can exist several own interest rates,[42] a claim to which Sraffa found it difficult to attribute a coherent theoretical meaning.[43] By referring to the traditional notion of equilibrium used in economic theory, he thought that competitive market forces tend to equilibrate the own interest rates, as they tend to eliminate the divergence of market prices from the normal or equilibrium prices. As a matter of fact, Sraffa (1932a: 50; 1932b:251) said, these tendencies are two aspects of the same process: when demand equals supply of each commodity, and saving decisions are equal to investment decisions, equilibrium–that is a situation in which there are no more forces acting to change relative prices–prevails and all own interest rates are equal to each other.

> In equilibrium the spot and the forward price coincide, for cotton as for any other commodity; and all the 'natural' or commodity rates are equal to one another, and to the money rates.
>
> (Sraffa 1932a:44)

The review of Hayek's book, as the other discussions on *A Treatise,* thus led Sraffa to reconsider the links between monetary theory and the theory of prices and distribution. We will see in the next section that Keynes showed his intention in the preparatory works of *The General Theory* to modify the way in which the neoclassical tradition linked monetary theory and the theory of prices and distribution. By doing so, he may have induced Sraffa to reconsider this problem.

1929–31: lectures on *Continental Banking*

During his first years in Cambridge Sraffa lectured on three subjects.[44] He gave a course on *Continental Banking,* which compared different banking systems, a theme that he had already examined in the 1922 articles on the Italian banking crisis.

The text of these lectures (*SP* D2/5) is full of bibliographical, historical, statistical and technical information. The aim of the course was to compare the features of the specialised banking system prevailing in England with those of the mixed system prevailing in Germany. The former excels in the creation of means of payment, the latter in the provision of loans to the firms: in some cases, it can also promote and control the activity of the firms with which it has financial relations. According to Sraffa, mixed banks can be not only creditors of firms, but also their more or less permanent partners.

He noticed that specialised banks have a longer historical existence and that it is their activity which characterises the notion of banking traditionally accepted. Mixed banks started to operate only in the second half of the nineteenth century, showing immediately their effectiveness in channelling financial resources to productive sectors.[45]

The latter point had already been recalled by Sraffa in his 1922 articles, where he had also claimed that a mixed system is not necessarily more risky than a specialised one as far as liquidity and solvability are concerned. This position too was developed in the lectures (see *SP* D2/5:14), where Sraffa dealt at length with the notion of liquidity. He argued that the degree of liquidity of financial activities depends more on the difficulty of selling them in the market at the present price than on their maturity. What is important is the existence of a large market:[46] in particular, the availability of a big purchaser, as the central bank can be, to buy these securities at a given price increases the degree of liquidity of these assets and the solvability of the financial institutions that use them. Central banks thus have a relevant role in determining the degree of solvability of a banking system. In the countries where a mixed system operates, Sraffa noticed, central banks have developed closer co-operative relations with the credit institutions than those existing in England, precisely to avoid problems of solvability.

Beside elaborating further the problems already considered in the 1922 articles, in the lectures Sraffa examined other themes related to the working of the banking systems. Here we only mention three of them.[47] In the first place, when he described the historical evolution of the mixed and specialised systems, Sraffa dealt with a subject that has become relevant in modern post-Keynesian monetary theory, that is the relationship between the behaviour of the monetary authorities and financial innovation. Anticipating some post-Keynesian positions on endogenous money, Sraffa claimed that the tendency of the English system to be effective in the creation of means of payment is a result of the introduction of the Peel Act of 1844, which imposed on the central bank rigid rules in the issue of money. The scarcity of the means of payment issued by the central bank induced commercial banks to be innovative in the use of cheques. In Continental Europe, instead, the lack of rigid rules in the issue of central bank money has brought about systems that make more limited use of cheques.

In the second place, in describing the origin of the mixed systems in Europe, Sraffa recalled that it can be brought back to Saint-Simon's Utopian theories of social reform. The first mixed bank, the *Crédit Mobilier,* was founded in 1852 by two followers of the great French Utopian, the Pereire brothers.[48] In the lectures Sraffa dealt at length with Saint-Simon's theories[49] and with the role that they had in the foundation of *Crédit Mobilier,* beside presenting a historical reconstruction of its setting, of its working, of its rapid success,[50] of its conflicts with the monetary authorities, and of its failure in 1867.

Finally, the lectures deal with the issue of the autonomy of the German central bank before and after the hyper-inflation of the 1920s. In particular, Sraffa described the conclusions reached in 1924 by the Dawes Commission on the issue of monetary base and on the proceedings relative to the

appointment of managers, proceedings that had to guarantee the independence of the German central bank (see *SP* D2/5:38–46).

1932–7: discussions on *The General Theory*

In the 1930s the preparation of the *Works and Correspondence of David Ricardo* became Sraffa's prevailing activity. None the less, he continued to follow Keynes' work, which, during the period here considered, parted with the dominant neoclassical approach in order to propose a 'conventionalist' theory of the level of production and of the interest rate. Sraffa's participation in the discussions on *The General Theory* is documented by the literature[51] and is confirmed by the *Sraffa Papers*. Sraffa was therefore an aware witness of the changes occurring in Keynes' work in those years.

The interpretations of the evolution of Keynes' thought presented in the literature are many.[52] In general, the shift from the analytical approach of *A Treatise* to that of *The General Theory* is associated with the introduction of the concept of a 'monetary theory of production' in his writings, which occurred towards the end of 1932 (see Keynes 1979:49–57). This concept was introduced to elaborate a theoretical approach which abandoned the traditional separation between a 'real' and a 'monetary' department of economics, arguing that the *equilibrium* position of the economy is directly influenced by monetary factors. In particular, he claimed, monetary policy is relevant in the determination of the *equilibrium* level of the income produced and of the interest rate (see Keynes 1979:54–7).

A major area of work for the development of this approach, as Keynes himself pointed out (1973a:410), was the establishment of three points in the theory of interest: (1) the rejection of the concept of a 'natural' interest rate representing an 'ideal maxim for monetary policy'; (2) the critique of the internal consistency of the neoclassical theory of the interest rate; (3) the proposal of an alternative theory of the interest rate.

The rejection of the concept of a 'natural' interest rate representing an 'ideal maxim for monetary policy' was linked to that of the separation between a 'real' and a 'monetary' department of economics. As to the critique of the internal consistency of the neoclassical theory of interest, Keynes (1973a:485–92) attributed great importance to it and made several attempts in different directions in order to work it out.[53] Yet, he was not able to put it on solid grounds and, in the final version of *The General Theory,* he ended up by accepting Harrod's advice to play down his claims against it.[54]

Finally, the proposal of an alternative theory took two steps of elaboration. In the first step, Keynes examined the determination of the *market* interest rate, by reformulating the analysis of liquidity preference, which was already present in *A Treatise*. In this analysis the determination of the interest rate was described as 'a highly psychological phenomenon' (Keynes 1936:202), which reflected the liquidity premium of holding money. In *The General Theory,*

Keynes examined the determination of the *normal* or *equilibrium* interest rate, that he also called 'durable', that represented the average value around which the *market* rate of interest oscillates. In opposition to what he had done in *A Treatise,* where he referred to the 'natural' rate, Keynes claimed that the *equilibrium* interest rate is determined in a 'conventional' way: it is not constrained at a natural level determined by real forces; it can establish itself at any level considered normal by common opinion; and it can be influenced by the decisions of the monetary authorities.

> It might be more accurate, perhaps, to say that the rate of interest is a highly conventional, rather than a highly psychological, phenomenon. For its actual value is largely governed by the prevailing view as to what its value is expected to be. *Any* level of interest which is accepted with sufficient conviction as *likely* to be durable *will* be durable; subject, of course, in a changing society to fluctuations for all kinds of reasons round the expected normal.
>
> (Keynes 1936:203)

According to Keynes (1936:202–4), historical and institutional factors can play a relevant role in the determination of the *equilibrium* interest rate. The common opinion is significantly influenced by the decisions of the monetary authorities, although it is necessary to refer to the existing historical situation to evaluate the strength of this influence:

> a monetary policy which strikes public opinion as being experimental in character or easily liable to change may fail in its objective of greatly reducing the long-term rate of interest…. The same policy, on the other hand, may prove easily successful if it appeals to public opinion as being reasonable and practicable and in the public interest, rooted in strong conviction, and promoted by an authority unlikely to be superseded.
>
> (Keynes 1936:203)

In the *Sraffa Papers* it is possible to read Sraffa's comments on Keynes' attempt to present an alternative theory of the interest rate. In his notes on Chapter 17 (*Sraffa Papers,* I 100) and in his annotations to the text,[55] we can notice that he was puzzled by some specific points of the analysis of liquidity preference, that is by the first step of Keynes' analytical elaboration.[56] Moreover, the passages of *The General Theory* relative to the formulation of a conventionalist theory, which belong to the second step of Keynes' analytical elaboration, were commented on as follows. On the one hand, Sraffa highlighted the first of the two passages quoted above on the conventional character of the interest rate. On the other hand, he annotated the second passage quoted above by adding two separate exclamation marks on the right hand side and by writing on the left hand side: 'This is the way of making a theory.[57]

The documents of the *Sraffa Papers* thus confirm that in those years the Italian economist intensively participated in the discussions on Keynes' work. At the same time, they confirm that Sraffa could appreciate that Keynes wanted to attribute a conventional character to the theory of the interest rate and the existence of some elements of disagreement on the way the latter was trying to establish this point.

As we will see in the next section, the documents relative to *Production of Commodities* show that in the subsequent years Sraffa too proposed a conventionalist theory, based on the idea that monetary policy is relevant in the determination of the money interest rates. Moreover, he reached important results on another subject that Keynes' work of that period unsuccessfully analysed, that is the search for logical inconsistencies in the neoclassical theory.

Production of Commodities: preparation and subsequent discussions

Several documents of the *Sraffa Papers* written after 1937 are relative to *The Works and Correspondence of David Ricardo* and to *Production of Commodities*.[58] In them there is no sign of further reflections on the monetary questions dealt with in previous years. Yet in the documents relative to *Production of Commodities* there are some elements which can contribute to clarifying Sraffa's positions on the relations between monetary theory and the theory of distribution.

In a preliminary draft of the Introduction to *Production of Commodities* Sraffa wrote that one of his aims was to verify up to what point income distribution can be considered independent of the material conditions of production:

> One of the objects of this enquiry is to serve as preliminary to an investigation of the theory of distribution. In particular to verify whether distribution between classes of society is determined by the supply of factors of production and their marginal productivity. The way in which we are proceeding is to see how far we can go in assuming that the distribution is indeterminate with respect to the internal conditions of the system; and at what point, if any, do we find it necessary to acknowledge that the technical conditions of production determine the distribution.
>
> (*SP* D3/12/42:78; dated 23/3/1957)

This would allow one to verify in which degree it is possible to consider income distribution as a conventional phenomenon.

In pursuing this objective it is possible to trace some common points between Sraffa's work and that of Keynes' *General Theory*. These are the attempts to criticise the logical consistency of the neoclassical theory and the

acceptance of a conventionalist theory of the interest rate, which Sraffa used to determine the rate of profits and to take this as the independent variable in the analysis of prices and distribution.

The earliest documents of the *Sraffa Papers* in which the critique of the logical inconsistency of the neoclassical theory of capital is clearly spelt out are dated 1942.[59] They refer first to the problem of measuring capital within the neoclassical theory of distribution and then to other aspects of this critique.[60] In the debate that followed the publication of *Production of Commodities,* the validity of this critique was recognised by some outstanding neoclassical economists.[61]

As to the use of a conventionalist theory of the interest rate to determine the rate of profits and take this as the independent variable in the analysis of production prices, the documents of the *Sraffa Papers* allow one to reconstruct how the Italian economist formulated this proposal. He noticed that the English classical economists took the real wage rate as exogenously given. They considered that the real wage rate was determined by forces which are external to the production process, like the physiological or historical necessities of the working class. This practice, Sraffa said, is not suitable for the study of modern economies, in which wages appear to contain, beyond the element of subsistence, a part of the surplus value produced.[62] Sraffa also noticed that, since it is difficult to distinguish the element of subsistence from the part of the surplus value produced, it is better to consider the wage rate as a unique variable, even if this can make it difficult to distinguish the 'basic' and the 'non basic' elements of an economic system.[63] In this way, the formation of the *whole* real wage comes to be considered as part of the processes through which the surplus value produced is shared between the different social classes. It is consequently put on the same level as the formation of the rate of profits, so that this variable too can be taken as independent in the analysis of prices of production.[64] As a matter of fact, Sraffa concluded, when the wage comes to be measured in terms of an abstract standard, it is preferable to take the rate of profits as the independent variable, assuming that the latter is determined by the money interest rates, that is again by forces which are external to the system of production under consideration:

> The last steps of the preceding argument have led us to reverse the practice followed from the outset of treating the wage, rather than the rate of profits, as the independent variable or 'given' quantity. The choice of the wage in the preliminary stages depended on its being assumed to consist of specified necessaries, determined by circumstances which (whether natural or social) were outside the system of production under examination. But the moment the possibility of variations in the division of the product was admitted, this consideration lost much of its force. And now that the wage comes to be 'given' in terms of a more or less abstract

> standard and does not acquire a definite meaning until the prices of commodities are determined, the position is reversed. The rate of profit, as a ratio, has a significance which is independent of any prices, and can well be 'given' before the prices are fixed. It is accordingly susceptible of being determined from outside of production in particular by the level of the money rates of interest. In the following sections the rate of profits will therefore be treated as the independent variable.
>
> (*SP* D3/12/80:8–9; dated 29/3/57)

On the determination of the interest rate the *Sraffa Papers* provide some new elements with respect to what is contained in paragraph 44 of *Production of Commodities*. The forces external to the production process, to which Sraffa refers in the passage quoted above, are the policy of the monetary authorities and the way in which this policy is viewed by financial markets:

> The point of view, however, of variation depending on the pull and push for the distribution of the surplus is more suited to regarding the rate of profit as being the independent variable and as being itself a reflection of the rate of interest which is determined by the operation of the Stock Exchange or regulated by banking policy.
>
> If however, as is more convenient with the sharing of surplus, we regard r as determined by the money rate, and this by Bank or Stock Exchange, the problem is much simplified.
>
> (*SP* D3/12/68:2)

The reference to monetary policy and to the prevailing views of financial markets is frequent in the preparatory documents of *Production of Commodities* and in those subsequent to its publication.[65] In a correspondence with Garegnani after the publication of his book, Sraffa further clarified the meaning of the hint he gave in paragraph 44. In the first place, he underlined the link between this hint and the objective of his work, that is the elaboration of a theory of distribution independent of the relative scarcity of the factors of production. In the second place, he clarified his intention to pursue this objective by using a conventionalist theory of the interest rate. Finally, he underlined the provisional character of his proposal, which was susceptible of being further elaborated and modified:

> I am convinced that the maintainance of the interest rate by the bank and (or) the stock exchange has had its part in the detemination of income distribution among social classes…. I did not want to commit myself much, and in general I only wanted to signal something in order to avoid the belief that the system is presented as 'foundation' for a theory of the relative supplies of capital and labour! It is *what is denied* that seems important to me: as to what is *affermatively claimed,* I have no intention to

> put forward another mechanical theory which, in one form or another, states again that income distribution is determined by natural, or technical or even accidental, circumstances, which in any case are such that they make any action taken by either part, in order to modify it, futile…. I do not see any difficulty in the determination of the rate of profit through a controlled or conventional interest rate, provided that the rate of profit will not be assumed to be determined by external unchangeable circumstances.
> (*SP* D3/12/111; stress in the original; our translation)[66]

In conclusion, on the basis of the documents of the *Sraffa Papers* it seems possible to claim that Sraffa argued for the development of a conventionalist theory of distribution. In line with Keynes' position in *The General Theory,* he wanted to give an active role to monetary policy in the determination of the equilibrium interest rate, thus attributing to the monetary authorities an active role in the determination of distributive variables and making their decisions part of the historical and institutional factors affecting the division of the surplus value produced.

Conclusions

The examination of the *Sraffa Papers* confirms the results reached by the literature on the links between Sraffa's earlier papers on monetary questions and his later work on the theory of value and distribution. Moreover, it provides a more complete picture of his positions and his intellectual itinerary, pointing out that, since the beginning, he had put at the centre of the stage the problem of income distribution as a conventional phenomenon, even if it was only in his later work that he verified analytically whether distributive variables can be considered independent of the material conditions of production.

His honour thesis had already implicitly suggested that monetary events, like inflation and deflation, have a permanent influence on social conflicts and contribute to the determination of the *normal* or *equilibrium* level of distributive variables. The real wage rate, he claimed, can be influenced by monetary and exchange policies, whose formation is, in turn, influenced by the counteracting interests of different social groups. This position was closer to those of the classical and marxian traditions than to the neoclassical theory which then prevailed. At the time it was not the result of a systematic study of the analyses proposed by these schools of thought, a study that Sraffa began in the subsequent years.

In the 1922 articles on the Italian banking crisis, Sraffa again examined the formation of economic policy and its effects on income distribution, showing a detailed knowledge of the working of the banking systems and an analytical approach which also differed from the Marxian tradition then prevailing. These features are also present in his 1925–7 papers, in which

again we find the idea that government intervention has permanent effects on income distribution and some signs of Sraffa's limited awareness of the implications of the neoclassical theory and of the critique of this theory which he proposed in *Production of Commodities*.

After he moved to Cambridge in 1927, Sraffa's interests in monetary studies were further stimulated by the fruitful relations with Keynes, who was at the time engaged in the publication of *A Treatise on Money* and *The General Theory*. By participating in the discussions on these books, Sraffa was led to move his attention from monetary policy to monetary theory, and in particular to the study of the relation between monetary theory and the theory of prices and distribution. He was an aware witness of the changes occurring in Keynes' thought in 1932, when he introduced in his writings the concept of a 'monetary theory of production', which implied a departure from neoclassical theory and the possibility of proposing a conventionalist theory of the interest rate. Like Keynes in *The General Theory,* Sraffa wanted to criticise the logical consistency of the neoclassical theory and accepted a conventionalist theory of the rate of interest. An examination of the *Sraffa Papers* shows that the earliest documents which present the critique of the neoclassical theory of capital and distribution proposed in *Production of Commodities* are dated 1942. It also shows that Sraffa used a conventionalist theory of the rate of interest to determine the rate of profits and to assume this variable as independent in the analysis of prices and distribution. Finally, this examination underlines the provisional character of his conclusions on the determination of the rate of profit and the possibility of further analysis on the influence of government intervention on the equilibrium level of distributive variables.[67]

It thus seems possible to conclude, in the face of what has been here examined, that Sraffa's intellectual itinerary was characterised by continuity on the factors affecting income distribution and that, as time passed, he became more and more aware of the limits of the positions expressed by the different traditions of thought and of the possibility of putting the foundations of economic theory on a conventionalist basis.

Notes

1 The author wants to thank MURST and CNR for the financial support given to the research projects from which this chapter draws. He also wants to thank Dr J.Smith of Trinity College, Cambridge, for kindly guiding him through the *Sraffa Papers (SP)*.

2 On Sraffa's appointment as lecturer at the University of Perugia and on his initial study of Marshall's partial equilibrium, see Naldi (1998a).

3 Sraffa (1922a) was his major contribution to this field.

4 It should, however, be remembered that in the preparatory writings of the review of Hayek's book *Prices and Production,* Sraffa stated: 'A long review of a short book requires some apology: the more so that the book is a monetary one, and the reviewer is a non-monetary economist' (*SP* D3/9:181).

5 Sraffa's interest in monetary economics might be explained by a document in the *Sraffa Papers,* written, according to the catalogue, for the preparation of a lecture. There Sraffa writes: 'Among the different parts of economics, monetary theory is the most precise: it perhaps overlooks the lowest number of essential facts. This is probably due partly to the relative simplicity of the subject, partly to the fact that for its practical relevance, it has been studied for the longest period of time and has been elaborated to the largest extent. As a consequence, monetary theory (like financial theory) is perhaps the only economic theory which is taken into account by practitioners when actions are taken' (*Sraffa Papers,* D3/1; our translation). The original text is in Italian and reads as follows: 'Fra le diverse parti dell'economia, quella monetaria è fra le meno inesatte: la teoria della moneta è quella forse che trascura il minore numero di fatti essenziali. Ciò è dovuto probabilmente in parte alla relativa semplicità della materia, in parte al fatto che, per la sua importanza pratica è stata più anti-camente studiata ed ha subito una maggiore elaborazione. In conseguenza di ciò, la teoria monetaria (insieme con la finanziaria) è forse la sola teoria economica di cui i pratici, nell'agire, devono tenere conto.'

6 See Pasinetti (1979; 1985b), Roncaglia (1983; 1984), Ginzburg (1986), Eatwell and Panico (1987), Potier (1991), Panico (1988a), De Cecco (1993; 1994), Ranchetti (1996), Davis (1998a).

7 See Roncaglia (1983, 1984, 1999), Ginzburg (1986), Eatwell and Panico (1987), Panico (1988a), De Cecco (1993, 1994), Ciocca and Rinaldi (1997), Davis (1998a) and Bellofiore and Potier (1998). A different position is held by Skidelsky 1986), who describes the relation between Sraffa and Keynes as a case of non-communication. For a critique of Skidelsky's arguments, see Panico (1988a).

8 This expression is used by Pasinetti to criticise the attitude of 'many economists, especially in the United States' (1979:738) who regard Sraffa's contributions in this way.

9 According to Pasinetti (1985b:319) and De Cecco (1993:1; 1994:155), Einaudi, who held at the time different views from those presented in Sraffa's thesis, was led to revise his positions. For more information on Sraffa's university studies, see Pasinetti (1979; 1985), Roncaglia (1983; 1984) and Faucci (1986), who points out that Sraffa was trained by economists who preferred Ricardo, Mill and the pragmatism of Marshall's school to the formalism of Walras and Pareto.

10 According to De Cecco, in writing his thesis, Sraffa used several documents from this conference. De Cecco (1993:3; 1994:158) states that if we read the documents of the conference, we notice that Sraffa's thesis contains some brilliant comments, some in agreement, others in disagreement, on the conclusions reached in Brussels and on the arguments proposed by the major economists attending it.

11 Howson (1985:153–9) argues that during the conference Keynes changed his view on this matter. Before the opening of the conference he had argued for an immediate return to a gold bullion standard, at existing currency value, as shown in a plan for 'Stabilisation of the European Exchanges', that Keynes made public (see Howson 1985:157). In *A Tract,* instead, Keynes 'argued not just for devaluation as against deflation but for domestic price stability over exchange rate stability' (Howson 1985:158). The new position, which appears to emerge already in an article in the *Manchester Guardian Commercial* of 7 December 1922 (in the *Collected Writings of J.M.Keynes,* vol. XVIII:70–84), is closer to that proposed by Sraffa in his thesis (see Sraffa 1920:46. English translation 1993:26) than the previous one.

12 For an analysis of the content of Sraffa's thesis on monetary policy and for a comparison with that of *A Tract,* see Roncaglia (1983; 1984), Ginzburg (1986), Panico (1988a) and De Cecco (1993; 1994).

13 See Panico (1988a:9) and De Cecco (1993:2; 1994:157).
14 For an analysis of this point see Panico (1988a:9–14), where it is noticed that Keynes (1923:26–8 and 123–4) acknowledges the influence of monetary factors on income distribution, stating, however, in line with the dominant view of the time, that this influence is temporary: 'But we cannot estimate the *stability* of this state of affairs, as contrasted with its desirability, unless we know the source from which the increased reward for the working class was drawn. Was it due to a permanent modification of the economic factors which determine the distribution of the national product between different classes? Or was it due to some temporary and exhaustible influence connected with inflation and with the resultant disturbance in the standard of value?' (Keynes 1923:27).
15 See Sraffa (1920:25 and 40–2. English translation 1993:16 and 23–4).
16 This does not imply that natural or material factors do not affect income distribution or that the equilibrium levels of the variables conventionally determined are not subject to constraints. Thus, within the classical theory of distribution, the laws regarding the cost of production of commodities set some constraints, not to the levels of the individual distributive variables, but to the relationships among these variables, like for instance the inverse relationship existing between the real wage rate and the rate of profits. At the same time, in a situation of free international capital movements, a gold parity or an interest rate determined without taking account of the purchasing power of the currencies or of the conditions prevailing in foreign financial markets could not be considered durable by the common opinion. For an analysis of the features of a theory of distribution where distributive variables are determined in a conventional way, see Panico (1988b).
17 Fisher's theory of distribution did not follow a conventionalist approach, but the neoclassical tradition then prevailing. None the less, in analysing the equilibrium value of money he made it depend on the evolution of monetary policy.
18 See Roncaglia (1984:108) and Panico (1988a:15).
19 See Sraffa (1921a, b, c). For further information on these articles see Potier (1991).
20 As Pasinetti (1985b:320) recalls, Sraffa carried to Keynes a letter of introduction written by Mary Berenson, wife of the American art critic Bernhard Berenson. The Berensons had played host in their house 'I Tatti' near Florence to Keynes and other members of the Bloomsbury Group a few years before. On this point see also Potier (1991) and Panico (1988a:15, n. 2).
21 According to Potier (1991), Keynes asked Sraffa to write the article during their second meeting which occurred probably in fall 1921.
22 Mixed banks can make both short and long-term intermediation, so that in their balance sheets there can be very liquid liabilities and locked up assets.
23 In the *Manchester Guardian Commercial* article Sraffa described how two major Italian banks had hidden to the public the poor state of their accounts, and how the monetary authorities, circumventing the existing laws, enabled 'the unfortunate operations of the ordinary banks to continue to be systematically saddled in the future on the banks of issue, and in the end on the State' (Sraffa, 1922b: 676).
24 Sraffa had to protect himself against the attempts of the *Banca Commerciale Italiana* to sue him. These attempts, which fortunately were unsuccessful, did not constitute Sraffa's only preoccupation in those days. Besides the events reported in the next footnote, just a few weeks before, Sraffa had been forced to resign from his job as Director of the Labour Statistics of the Province of Milan, owing to the violent assaults and attacks by some fascist groups on this institution, which had been founded by the previous socialist administration of that province. For more information on these events, see Roncaglia (1983:140; 1984:111) and Naldi (1998b).

25 Mussolini sent two telegrams to Sraffa's father. In the first, dated 20 December 1922, he stated that the article in the *Manchester Guardian Commercial* was an act of 'banking defeatism' and a 'sabotage of the Italian finance' made by a socialist who will have to give 'strict account' of his behaviour. In the second, dated 21 December, he asked Sraffa's father to persuade his son to retract. Sraffa refused and to protect himself moved temporarily to Lugano in Switzerland. A few days later, as Pasinetti (1979:737 and 1985b:320) and Roncaglia (1984:111) also point out, Sraffa was invited by Keynes to come to England. Sraffa left a few weeks later, but on the 26 January 1923, at Dover, he was detained, questioned for three hours by an Inspector and then told that he could not be given leave to land by special order of the Secretary of State. Whether he was stopped because of pressure from the Italian fascist government on the British Foreign Office or on account of contacts with some British communists established during his journey in England in 1921, is not known. Sraffa went to Paris and subsequently back to Italy, where fortunately the situation had calmed down. None the less, Keynes tried to obtain the cancellation of Sraffa's name from the list of the 'undesired' people, and was able to get it in summer 1924, after the change of the British government. See also Panico (1988a) and Naldi (1998b).

26 On monetary analysis, during the period 1923–7, beside the writings mentioned in this section, Sraffa translated into Italian Keynes' *A Tract of Monetary Reform,* published by Treves in 1925, and published four reviews (see Sraffa 1925b; 1926b, c; 1927a).

27 See *Sraffa Papers,* D3/3. The Papers' catalogue gives as its title 'Untitled article on the economics of fascism' and its date as 1923; in the text, however, there is no element related to the date.

28 See Sraffa (1923). The article is signed S. and was attributed to Sraffa by Potier (1991).

29 Angelo Tasca was, with Gramsci, Terracini and Togliatti, one of the founders of *Ordine Nuovo* in 1919. With them he played a relevant role in Socialist Party Congress in 1921, which led to the birth of the Italian Communist Party, of which he was a major figure until 1929, when he was expelled for 'rightism'. The two letters of Tasca to Sraffa, published in *Stato Operaio,* can be found in *Sraffa Papers* (C 309). Potier (1991) has also clarified that there was another exchange of correspondence between Sraffa and Tasca, dated 1930, on the Italian economic crisis and its financial aspects. This correspondence can be found at Fondazione Feltrinelli in Milan.

30 The original text is in Italian and reads as follows: 'In linea generale mi sembra un errore–e molto pericoloso–quello di credere che ogni singolo atto del governo fascista (e di ogni governo capitalists) sia direttamente dettato dagli interessi immediati delle banche e dei grossi industriali.'

31 According to Sraffa (1927b:1089–90), by strengthening the fascist government, the revaluation would have been useful, in the last instance, to the most powerful economic groups. Yet it did not give them an immediate benefit, as confirmed by the pressures of these groups on the government to avoid it.

32 One can recall on this point the speech given by Mussolini in Pesaro on 18 August 1926.

33 In another conference given by Sraffa in Cambridge at the Keynes Club in 1927 (*SP* D2/2), this idea is mentioned again dealing with the objectives of the fascist proposal to create a Corporate State in Italy. The Fascist Party, Sraffa argues, having refused the nationalisation of the means of production, wants to nationalise the mechanisms through which income distribution is determined by imposing strict government's controls on trade unions.

34 These positions were presented by Cole, in a lecture given at the Marshall

Society in Cambridge on 26 October 1927, and by Dobb, in a book on Russia's economic development.

35 In November 1927, soon after his arrival in Cambridge, Sraffa discussed with Keynes some first elaborations of the analyses that he published in *Production of Commodities* (see Panico and Salvadori 1994:339 fn. 8). Subsequently, he intervened in the Symposium of the *Economic Journal* on Marshallian supply curves (Sraffa 1930a). Moreover, as Potier (1991) points out, already in 1930 he had acquired a reputation as a good connoisseur of Ricardo's writings, by publishing in the *Quarterly Journal of Economics* (Sraffa 1930b) a critical note of an essay by Luigi Einaudi (1929) on a mistake attributed to Ricardo. Finally, in 1930, and again thanks to Keynes' intervention, Sraffa was appointed editor of Ricardo' writings.

36 See Potier (1991) and Panico (1991).

37 The abstract can be found at the Wren Library of Trinity College in Cambridge under 'Sraffa's books 4612'. J.Robinson's recollections on Sraffa's intense participation in the discussions of the Circus are presented in Robinson (1978: XII). On this point see also Potier (1991).

38 For further information on the interpretation of the evolution of Keynes' monetary thought here followed, see Panico (1988b).

39 In the documents of the *Sraffa Papers* (D3/9) relative to the preparation of the review, Sraffa criticised Hayek's distinction among the actions of individual agents, which represent 'natural' actions, those of the government, which represent 'artificial' actions, and those of the banking sector, which represent something which is closer to the actions of the government and are consequently bound to produce damage as do all things which are not natural. See *SP* (D3/9:132; but also D3/9:15 and D3/9:34–43).

40 On these changes in the notions of equilibrium used by economic theory, see Garegnani (1976) and Milgate (1979). The link between Hayek's work and the literature which at the time introduced these changes in the notion of equilibrium is however controversial. According to Kurz (1995:12–13 and 41), in *Prices and Production* the notion of intertemporal equilibrium does not play any constructive role.

41 The term natural rate of interest was then used by Sraffa in a different way from Wicksell and Keynes, who defined it as the real rate of return which prevails in an equilibrium situation and which is uniform in all sectors of the economy. Sraffa, instead, following Hayek, defined it as the real rate of interest measured in terms of any one of the existing commodities. Both in the preparatory writings (*SP* D3/9:7; 10; 34–43; 56–61; 174–6; 177) and in the review (Sraffa 1932a:49) Sraffa often detained himself on the misunderstandings that these terminological problems could produce, taking Hayek as responsible for them.

42 On Hayek's claim see also Fritz Machlup-Wolf's letter to Sraffa, dated Vienna 29 July 1932, which can be found in the *Sraffa Papers* (C 180).

43 'Dr. Hayek now acknowledges the multiplicity of the "natural" rates, but, he has nothing more to say on this specific point than they "all would be equilibrium rates". The only meaning (if it be a meaning) I can attach to this is that his maxim of policy now requires that the money rate should be equal to all these divergent natural rates' (Sraffa 1932b:251). In the preparatory writings (*SP* D3/9:128) Sraffa put at the end of this sentence an exclamation mark, perhaps to underline the puzzle it caused.

44 During his first academic year in Cambridge (1927/8), Sraffa was excused from lecturing. In the subsequent three years he gave two courses to tripos students, one titled *Advanced Theory of Value,* held in the Michaelmas Terms of 1928/9 and 1929/30 and in Lent Term of 1930/31 (see *SP* D2/4), and one titled *Continental*

Banking, held for three years after 1929. Finally, during Lent Term 1941 (8 lectures), Easter Term 1941 (2 lectures), and Lent Term 1942 (8 lectures) and Lent Term 1943, he lectured to tripos students on *Industry,* dealing with some financial problems related to the separation of property from control in corporate firms (see *SP* D2/8).

45 In document D2/5:15 Sraffa underlined the contribution given by mixed banks in the second half of the nineteenth century to German industrial development.

46 In document D2/5:14 Sraffa clarifies that the same applies to commodity markets. Some commodities, for instance gold and some raw materials, have a high degree of liquidity on account of the way their markets are organised.

47 Other themes dealt with in the lectures are: the formation of large concentrations in the German and English economy; the operative costs of the two systems, with special reference to those related to the German hyper-inflation of the 1920s; the degree of control of the German central bank on the money market.

48 According to Sraffa, the origin of the mixed bank is anomalous. In general, new institutions emerge from the previous state of things, without a prearranged plan: their origins and features are understood after they have become established. The large use of cheques in England, Sraffa noticed, is a typical case. It did not come from a prearranged plan. If Sir Robert Peel could have anticipated that the increased use of cheques would have undermined his attempt to achieve rigid rules in the creation of means of payments, he would have probably introduced in his 1844 Act measures to avoid it. The birth of the mixed bank, instead, can be considered as the result of the will of the Pereire brothers to affect in a prearranged way the working of market forces and realise a Saint-Simonian plan for social reforms.

49 On the relevance that Sraffa attributed to Saint-Simon as a precursor of Marx's political theory, see Meldolesi (1982) and Potier (1991).

50 This success, Sraffa recalled, had inspired Emil Zola's novel *L'Argent.*

51 See Panico (1988a:23–4).

52 For an analysis of this literature and of Keynes' writings of that period, see Panico (1988b), from which the interpretation here followed is derived.

53 See Keynes (1973a:489, 492, 530–6, 540, 547–8, 551, 553–5 and 590; and 1973b:462, 470–1 and 477).

54 On the exchange between Harrod and Keynes on this point, see Milgate (1977).

55 Sraffa's copy of *The General Theory* can be found in the Wren Library of Trinity College in Cambridge under 'Sraffa's books 2644'.

56 See Sraffa's annotations to pp. 200–1 of the original edition of *The General Theory*.

57 The original text is in Italian and reads as follows: 'così si fa una teoria.' See Sraffa's annotations to p. 203 of the original edition of *The General Theory*.

58 See *Sraffa Papers,* D3/11, D3/12 and D3/13. The largest part of the documents relative to *Production of Commodities* was written in the 1950s and contains different drafts of this important book. A large number of documents was written in the 1940s. Few of them were written in previous years.

59 See *Sraffa Papers* (D3/12/16:41; dated 2 July 1942) and (D3/12/16:14; dated August 1942). Another document (D3/12/15:10–11), dated 1942, contains some notes on an article published by Kaldor in *Economica* in February 1939. In them Sraffa points out that the capital/labour ratio can vary in the opposite direction to the composition of capital, if a variation of the interest rate simultaneously occurs. It is, however, worth noticing that Sraffa's annotations to Part III, chapters I and II, of Lindhal's 1939 book *Studies in the Theory of Money and Capital* (see Sraffa's books, n. 2092) show that he was aware, when he read this book, of the logical inconsistencies of the neoclassical theory of capital, since he

systematically highlighted all passages where these inconsistencies are present. The date of these annotations is not specified. Yet some notes on this book are added in document D1/91 which is dated post-1941.

60 A reconstruction of how Sraffa worked out the different aspects of his critique of neoclassical theory of capital and distribution is not yet available in the literature. In order to stimulate this study, it can be useful to notice here that in the documents of *Sraffa Papers* written in the 1940s there is one (*SP* D3/12/42:84; dated 15 November 1945) on switches of techniques and another (*SP* D3/12/42:23; dated March 1947) which refers to a critical note written by Irving Fisher as an Appendix to *The Rate of Interest* where the American economist notices with surprise the occurring of the phenomenon known as reverse capital deepening.

61 See the Symposium organised by the *Quarterly Journal of Economics,* particularly the *Summing up* by Samuelson (1966).

62 Already in the 1940s, Sraffa noticed that a theory of prices and distribution which took the real wage rate as determined by the concept of subsistence could not considered satisfactory if referred to modern economies. See *Sraffa Papers* (D3/12/16:18). The document has no date, but it belongs to a folder having the following inscription 'Notes mostly 1942 on (Crosscap) (transferred 1955 into a black cover)'. Subsequently, he reconsidered this point several times. See *Sraffa Papers* (D3/12/35:26; D3/12/50:10; D3/12/50:16–24; D3/12/52; D3/12/68:1–2; D3/12/71:8; D3/12/71:10; D3/12/78:6; D3/12/78:11; D3/12/80:8–9).

63 See *Sraffa Papers* (D3/12/50:10; D3/12/50:16–24; D3/12/71:10).

64 Sraffa had noticed already in 1942 that within his analysis it is possible either to take as exogenously given one of the two distributive variables, or to identify the pairs of the values of the real wage rate and the rate of profits which are compatible with the prevailing conditions of production: 'So far, we have required knowledge of one kind–i.e. of existing things. Thus we determine prices: we can also determine wages, if purely objective necessity, like raw materials for cloth or fuel for machine–and then also the rate of profit. But, now, theories of r and w assume knowledge of possibilities–i.e. of what would happen in certain hypothetical circumstances–knowledge not of points but of curves' (*SP* D3/12/29:25; dated 27/12/42).

65 In document D3/12/68:2 Sraffa claims: 'And, with the problem that of distrib. the surplus, the rate of profit as independent variable seems more consonant (germane). The latter being in turn determined by the rate of interest, as determined by the Bank.' In document D3/12/78:6 he instead wrote: 'It is possible…to conceive of it [the rate of profit] as being «given» from outside the system of production, much as conforming with the pattern of money rates of interest determined independently by the banking system or the Stock Exchange' (dated March 1957). In document D3/12/78:13 he talks again of money interest rates determined 'by the banking system or the Stock Exchange'. See also *Sraffa Papers* (D3/12/111).

66 The original text is in Italian and reads as follows: 'Sono convinto che il mantenimento del saggio di interesse da parte della banca e (o) della borsa abbia avuto la sua parte nel determinare la distribuzione del reddito fra le classi sociali…io non ho inteso dir niente di molto impegnativo, e in generale ho solo voluto metter fuori qualche segnale per evitare che si creda che il sistema viene presentato come "fondamenta" per una teoria delle offerte relative di capitale e di lavoro! E' la *negazione* che mi sembra importante: quanto alla *affermativa* non ho nessuna intenzione di mettere avanti un'altra teoria meccanica che, in una forma o nell'altra, ribadisca l'idea che la distribuzione sia deteminata da circostanze naturali, o tecniche o magari accidentali ma comunque tali da rendere futile

qualsiasi azione, da una parte e dall'altra, per modificarla.... Non vedo la difficoltà alla determinazione del saggio del profitto mediante un saggio dell'interesse controllato o convenzionale, a condizione che non si presupponga il saggio del profitto determinate da circostanze ineluttabili esterne.'

67 As to this point we can recall that some recent works (see Panico 1993, 1997 and 1999) have shown that, when we introduce in post-Keynesian models of steady growth the existence of an accommodating monetary policy and of a fiscal policy with a unbalanced government budget, it is possible to reconcile the post - Keynesian theory of growth and distribution, proposed by Kaldor and Pasinetti, and that theory of distribution, derived from Sraffa's hint in paragraph 44 of *Production of Commodities,* in which variations in the interest rates affect the rate of profits. This result contradicts the view, proposed by Moss (1978:306), Nell (1988), Pasinetti (1988), Pivetti (1988), Wray (1988) and Abraham-Frois (1991:197 and 202), that these two post-Keynesian approaches to income distribution are alternative.

19 On the relationship between Sraffa and Keynes

Fabio Ranchetti[1]

1. According to a well-known opinion, the intellectual relationship between Sraffa and Keynes was 'a case of non-communication':[2] the two Cambridge revolutions, the one associated with Sraffa and imperfect competition, the other with Keynes and effective demand, 'never converged in Keynes' lifetime' (Skidelsky 1992:290). In spite of the fact that everybody, not only in Cambridge,[3] acknowledged the relevance and the strength of his criticism, and the powerful influence on their own writings, Sraffa remained 'an isolated intellectual figure, feared and admired, rather than actually understood'.[4] Relying upon new sources and materials now available (mainly the *Sraffa* and *Keynes Papers,* but the *Kahn Papers* and the *Kaldor Papers* are also relevant), it has now become possible to understand the intellectual relationship between Sraffa and Keynes better, and therefore to reconsider that opinion. I contend that it is both right and wrong. It is right because, due to his extraordinary integrity and independence of mind, Sraffa followed a highly original path of thought, quite distant from the Cambridge Marshallian tradition, which culminated in 1960 in *Production of Commodities by Means of Commodities.* It is wrong, however, because the intellectual exchange between Sraffa and Keynes was constantly strong and deep, and was interrupted only by Keynes' illness and then by his death. Without Sraffa's passionate discussions and contributions, both the *Treatise on Money* and *The General Theory* would have been different and, probably, without Keynes' influence and encouragement *Production of Commodities* would have never seen the light. In order to examine the relationship between Sraffa and Keynes, I think it is convenient to distinguish five fundamental stages. The first one–which dates from 1921 (the date of their first encounter)–mainly concerns the economic issues examined by Keynes in the *Tract on Monetary Reform,* which Sraffa translated and published into Italian in 1925. I here recall that what struck Keynes in his first encounter with Piero Sraffa (at that time a very young man, just 23 years old), in August 1921, was his exceptional sharpness of mind in discussing speculation and hedging on futures markets (see Chapter 3 of the *Tract* in which there is a section on this topic).[5] The second stage focuses on the *Treatise on Money* and is linked to the activities of

the Cambridge 'Circus', in which Sraffa took a pre-eminent part. Again, the main topics at issue were speculation and interest rates: 'Piero will be troublesome about speculators. He says that relative yields are the only things that matter–it is not a question of bearishness', as Kahn put in a letter to Keynes (7 May 1931). The third stage is the triangular, theoretical controversy between Keynes, Hayek and Sraffa which culminated in Sraffa's famous review-article–'Dr. Hayek on Money and Capital'–in the March 1932 issue of the *Economic Journal*. As is well known, it is in this article that Sraffa put forward a theory of the own-interest rates which Keynes took up in the *General Theory*. The fourth stage regards the *General Theory* and Sraffa's critique. As we will see, it is, to a great extent, a development of the questions raised and discussed in the controversy with Hayek. The fifth and final stage of their relationship deals with Hume's *Abstract* of the *Treatise on Human Nature,* which Keynes and Sraffa published, with a joint introduction, in 1938. Looking at their research programme from a general perspective, it is easy to identify a unifying theme which links the five different stages we have distinguished together. That is the analysis of the relationship between the rate of money interest and the rate of profit, within the context of an 'entrepreneur economy'.[6] In fact, the discussions between the two economists centred mainly on the 'causal nexus' between these two economic variables, which are important in order to explain 'the change in the existing economic system' (Keynes, *Treatise*). (The energies spent by Keynes and Sraffa in studying and publishing Hume's *Abstract* are part and parcel of that research project, contributing as they did to clarifing the relevance of the notions of cause, convention and belief in the construction of a rigorous economic theory.) Starting from a brief analysis of paragraph 44 of *Production of Commodities,* in what follows I will concentrate only on what I defined as the third and fourth stages of their relationship, that is on Sraffa's critique of Hayek's theory of money and capital and on Sraffa's critique of Keynes' theory of money and interest as presented in the *General Theory*.[7]

2. The relationship between Sraffa's and Keynes' economic theories has often been considered starting from paragraph 44 of *Production of Commodities,* in which Sraffa states that the rate of profits is 'susceptible of being determined from outside the system of production, *in particular by the level of the money rates of interest*' (Sraffa 1960a:33; emphasis added). Notwithstanding Sraffa's silence on the matter, some economists have interpreted this passage as an implicit reference to Keynes' theory, and in particular to Chapter 17 of the *General Theory,* where, as is well known, Keynes refers explicitly to Sraffa's notion of 'natural or commodity rate of interest' and puts forward a theory of the relationship between the money rate of interest and the rate of profit. The interesting thing is that in the *Sraffa Papers,* one can now find a detailed examination and criticism of Chapter 17, and also other materials which greatly help to understand the way Sraffa (and Keynes) thought the rate of

interest and the rate of profits should have been determined, and therefore to clarify the precise link between *Production of Commodities* and the *General Theory*. In order to examine Sraffa's critique of Chapter 17, it is however necessary to recall Sraffa's critique of Hayek's theory of money and capital first.

3. In his critique of Hayek's theory of money and capital, presented in the March 1932 issue of the *Economic Journal* edited by Keynes, Sraffa put forward the following argument, which is based on the notion of a 'natural or commodity rate of interest'.[8]

For each commodity that has a forward market it is possible to define a 'commodity' or 'natural' rate of interest as the ratio between the quantities exchanged of the commodity at two different dates, minus one. In a monetary economy, i.e. an economy in which transactions (including loans) are made in terms of money, the natural or commodity rate of interest is then defined as the ratio between the spot and the forward price of the commodity (the latter discounted at the relevant money rate of interest), minus one. Approximately, the natural rate is equal to the money interest rate minus the percentage difference between the forward and the spot price of the commodity.[9] According to Sraffa, a divergence between the natural or commodity rate and the money rate of interest, and between the various (one for each different) commodity rates, implies a divergence between the spot price and the forward price of the commodities. In particular, the forward prices will be lower than the spot prices for those commodities whose output is expected to increase (and *vice versa* the forward prices will be higher than the spot prices for those commodities whose output is expected to decrease). Fundamentally, the divergence is due, according to Sraffa, to a difference between the market price of the commodities and their costs of production. Since in a perfectly competitive equilibrium all rates must be uniform (and spot and forward prices coincide for all commodities), such divergence defines a state of disequilibrium of the economy. On the other hand, as Sraffa states,

> under free competition, this divergence of rate is as essential to the effecting of the transition [towards equilibrium, and therefore towards a uniform rate of interest] as is the divergence of prices from the costs of production; it is, in fact, another aspect of the same thing.
>
> (Sraffa 1932a:50)

For our purposes, three points of Sraffa's argument are worth noting.

First, as one can see from its definition, the commodity rate of interest depends on the money rate of interest, which is given.[10] Therefore, it is the changes in the spot prices which bring the different commodity rates to uniformity, or to equilibrium as it is defined by the equality between the *given* money rate and the commodity rates. This means that, in Sraffa's analysis, the money interest rate is not simply a *numeraire*. On the other hand, and this

is my second point, in the whole argument Sraffa never states or hints at an explicit theory of the determination of the level of the money rate of interest which is taken as given. As we know, in Sraffa's articles in the *Economic Journal,* the argument is directed against the 'Classical' theory of capital and interest as represented in this instance by Hayek.[11] However, Sraffa confined himself to pointing out, by means of a well-constructed example, some logical defects of the Classical theory of interest without presenting, at least explicitly, a new, alternative theory of the rate of interest.

Third, in the whole argument Sraffa never uses marginalist concepts or tools. In particular, Sraffa's notion of natural or commodity rate of interest does not imply at all any marginalist reasoning, and has nothing to do with the Classical and Hayekian notion of a natural rate. In fact, within this context, according to Sraffa, 'natural' rate simply means 'in physical terms'; whereas, according to Hayek (and the Classical theory), 'natural' means, in a much more complicated and theoretically compromising way, to be determined by the symmetrical 'real' forces of thrift and productivity.[12]

4. In the *General Theory,* and in some of the writings which followed it, Keynes intended to propose a new theory of interest and money. Keynes' argument should therefore be confronted with the Classical theory of interest and money, of which it meant to be a critique. The Classical theory of interest, *as it was understood and interpreted by Keynes,* could be summed up in three propositions.[13]

C1. Interest is the premium for saving, that is for the abstaining from consumption. Consequently, the rate of interest is 'the "price" which brings into equilibrium the demand for resources to invest with the readiness to abstain from consumption' (Keynes 1936:167).

C2. The rate of interest–where by rate of interest here one should mean the 'natural' rate according to the Classical theory–is determined by the simultaneous working of two *real* factors or forces: the propensity to consume, on the one hand, and the marginal efficiency of capital, on the other hand. For example, in a very clear passage, Keynes summarises the Classical theory of interest:

> the amounts of savings depends on the propensity to consume and the rate of interest taken in conjunction, and the amount of investment on the marginal efficiency of capital and the rate of interest taken in conjunction, so that the rate of interest is fixed at that point at which the amount of savings will be equal to the amount of investment.'
>
> (Keynes, 1973b, vol. XIV:15)

C3. The *money* rate of interest depends on the marginal efficiency of capital assets *other than money*. In fact, as Keynes states in another very clear passage,

> the orthodox theory maintains that the forces which determine the common value of the marginal efficiency of various assets are independent of money, which has, so to speak, no autonomous influence, and that *prices move until the marginal efficiency of money, i.e. the rate of interest, falls into line with the common value of the marginal efficiency of other assets as determined by other forces.*
>
> (Keynes, 1973b, vol. XIV:103; emphasis added)

As a result, the position of equilibrium is given by that 'common value of the rate of interest and of the marginal efficiency of capital at which the saving determined by the former is equal to the investment determined by the latter' (Keynes, 1973b, vol. XIV:104).

To the 'Classical' theory, Keynes contrasts his own new theory of interest. For our purposes, Keynes' theory may be summarised in the following three propositions.

K1. The rate of interest is 'the reward for parting with liquidity' (Keynes 1936:167) for a specified period of time, and it measures how 'unwilling' are those who possess money to part with it. The rate of interest is therefore an essentially *monetary* phenomenon: 'it is the "price" which equilibrates the desire to hold wealth in the form of cash with the available quantity of cash' (ibid.).

It is a characteristic feature of Keynes' notion that the interest rate depends upon a set of decisions which are distinct and subsequent with respect to the decisions regarding consumption and saving: in Keynes' theory, the rate of interest could thus be conceived as 'susceptible of being determined from outside the system of production' (as Sraffa will later say in his *Production of Commodities*), and therefore independently of the real forces, and particularly independently of the marginal productivity of capital. As to its determination, in the *General Theory* there are two different lines of research, which are not perfectly integrated (and may be not integrable): the first one (K2A) is a Classical analysis in terms of demand and supply, the second one (K2B) is a much more heterodox analysis in terms of conventional and institutional factors.[14]

K2A. Given the available quantity of money, the rate of interest is determined by the liquidity-preference of wealth holders, that is by the quantity of money they decide to keep relative to each different level of the rate of interest.

To understand Sraffa's critique, it is essential to underline one aspect of Keynes' proposition. Keynes conceives liquidity-preference as a specified functional relationship between the quantity of money[15] and the rate of interest. This relationship can be represented by a continuous, descending curve: 'As a rule, we can suppose that the schedule of liquidity-preference relating the quantity of money to the rate of interest is given by a smooth curve which shows the rate of interest falling as the quantity of money is increased' (Keynes

1936:171). In another important passage, Keynes confirms this point: 'there is a continuous curve relating changes in the demand for money to satisfy the speculative motive and changes in the rate of interest as given by changes in the prices of bonds and debts of various maturities' (Keynes 1936:197). To explain that particular shape of the liquidity-preference curve, Keynes proposes three different reasons. The first is that, as a consequence of a decrease in the rate of interest (and therefore an increase in the national income), it is likely, *ceteris paribus,* that the demand for money due to the transactions-motive will increase (cf. Keynes 1936:171–2).[16] The second explanation, and the most relevant from a theoretical point of view, refers to the fact that, among speculators, there are different opinions as regards to the 'normal' rate of interest. Therefore, as the market rate decreases, the quantity of money that *some* individuals wish to hold increases 'because their views as to the future of the rate of interest differ from the market views' (Keynes 1936:172). The larger the difference in views as to the future of the interest rate (as in England), the more negatively inclined the curve will be; the smaller the difference (as in the United States), the flatter the curve will be; a perfect unanimity would bring about a perfectly horizontal curve.[17] The third explanation offered by Keynes is more empirical and institutional: 'experience' would confirm the asserted relationship between the quantity of money and the rate of interest, 'because in normal circumstances the banking system is in fact always able to purchase (or sell) bonds in exchange for cash by bidding the price of bonds up (or down) in the market by a modest amount; and the larger the quantity of cash which they seek to create (or cancel) by purchasing (or selling) bonds and debts, the greater must be the fall (or rise) in the rate of interest' (Keynes 1936:197).

I now consider the other way of determining the rate of interest, which can also be found in the *General Theory* (particularly in Chapter 15), as well as in some of Keynes' later contributions (especially in the 1937 article in the *Quarterly Journal of Economics,* reprinted in the *Collected Writings,* vol. XIV).

K2B. The rate of interest is determined by prevailing conventions, 'not rooted in secure knowledge' (Keynes 1936:204), and therefore it can be greatly influenced by monetary policy.[18] According to Keynes,

> It is evident that the rate of interest is a highly psychological phenomenon.... It might be more accurate, perhaps, to say that the rate of interest is a highly conventional, rather than a highly psychological, phenomenon. For its actual value is largely governed by the prevailing view as to what its value is expected to be. *Any* level of interest which is accepted with sufficient conviction as *likely* to be durable *will* be durable; subject, of course, in a changing society to fluctuations for all kinds of reasons round the expected normal.
>
> (Keynes 1936:202–3)

K3. Once it is determined, the rate of interest–i.e. the *money* rate–determines in turn the marginal efficiencies of *other* activities, that is those which are

different from money. The equilibrium position is consequently defined as that state of the economy in which 'the marginal efficiencies of all kinds of assets [including money] are equal when measured in a common unit' (Keynes 1973b, vol. XIV:102–3). It follows that, in the state of equilibrium, the marginal efficency of capital will be equal to the rate of interest. Here, one should notice how Keynes reverses the causal link stated by the Classical theory: it is not the marginal efficiency of capital which determines the rate of interest, as stated by the Classical theory (see proposition C3 above), but it is rather the money rate of interest which determines the marginal efficiency of capital.[19]

On the basis of these three propositions (or better four, since the second is, as we have seen, twofold), Keynes rests his fundamental thesis that an underemployment equilibrium could obtain. The thesis is not univocally argued by Keynes. I will here attempt to reconstruct the main lines of the argument Keynes presents in Chapter 17 of his *General Theory*–'The Essential Properties of Interest and Money'. I will concentrate on this argument since it is precisely the place where Keynes utilises Sraffa's notion of natural rate of interest, and also because it is precisely the argument to which Sraffa raises his strongest objections. As is well known, this is a peculiarly complex and difficult chapter, in which Keynes' analysis attains the highest level of abstraction, and which has aroused conflicting interpretations.[20] It is also well known that this is the (only) chapter in which Keynes explicitly quotes Sraffa, by adopting his idea of the commodity rate of interest, an idea that, as we have seen, the Italian economist had used some years earlier in his attack against Hayek's theory of capital. (In the same chapter, it is worth remembering, Keynes also utilises the idea of 'composite commodity', which will be so important in Sraffa's *Production of Commodities by Means of Commodities*.)

According to Keynes, every commodity or asset which can be borrowed or lent has its 'own-rate of interest'. In general, such rates will differ one from the other, even if measured in a common standard; on the other hand, the economic equilibrium condition requires that a uniform rate be obtained. Such a result is obtained by the working of the 'natural forces' of the market through two distinct mechanisms, the one referring to the short term, the other to the long term: arbitrages on the existing stocks of the assets and therefore changes in the relative demand prices, on the one hand, and changes in the stocks of capital assets (i.e. investments) and therefore in marginal efficiencies, on the other hand.[21] In this double process of adjustment, the money rate of interest plays a truly crucial role: in fact, it is the money rate of interest which, to use Keynes' own words, 'rules the roost', or regulates all other own-interest rates. The reason why (according to Keynes), is that it is the highest among all rates; and that depends on the fact that, while in general own-rates of interest are a decreasing function of the quantity of the commodity or asset, so that as it increases its own-rate

decreases, the same does not apply to the case of money. In this special case, in fact, the own-rate of interest on money, i.e. the money rate of interest, is more 'reluctant' to fall, as output increases, than the own-rates of any other assets (cf. Keynes 1936:229 and 236); and this is due to the peculiar nature of the commodity whose it is the 'price', i.e. money. Since they are very well known, I will not dwell here on the three reasons put forward by Keynes to explain the rigidity of the money rate of interest, namely the two peculiarities of money (zero, or almost zero, elasticity of production and elasticity of substitution) and the ineffectiveness of a reduction in money wages (cf. Keynes 1936:229–34). Instead, I will briefly consider the link between unemployment and the money rate of interest as shown by Keynes' analysis.

Investment, that is the production of new capital goods, takes place if and when the own-rates of interest of the different assets are higher than (or at least equal to) the money rate of interest. As the quantities produced of the capital goods increase, their respective interest rates fall. Now, when prices and quantities of all assets other than money have adjusted so that their rates have become equal to the money rate of interest, investment (and the accumulation process) come to halt, and the wealth owners' demand predominantly concentrates on money. On the other hand, in a monetary economy, nothing guarantees that this occurs at the full employment level. In other words, and this is Keynes' conclusion, the (given) level of the money rate of interest might be too high for the attainment of the full employment equilibrium. Therefore, as Keynes says in his most brilliant style:

> Unemployment develops, that is to say, because people want the moon;– men cannot be employed when the object of desire (i.e. money) is something which cannot be produced and the demand for which cannot be readily choked off. There is no remedy but to persuade the public that green cheese is practically the same thing and to have a green cheese factory (i.e. a central bank) under public control.
>
> (Keynes 1936:235)

Without entering into the details of Keynes' argument as presented in Chapter 17, at least two important points deserve attention: the definition of the rate of interest, and the notion of liquidity premium together with the consequent determination of the money rate of interest.

In the course of his argument, Keynes gives three different definitions of the rate of interest on a commodity or an asset in which it is possible to hold wealth. The first definition (D1), which is introduced in section one of Chapter 17 and then immediately abandoned, is the Keynesian version of Sraffa's 'natural or commodity rate of interest' (as Keynes himself acknowledges at p. 223 of the *General Theory*).

The second definition (D2), which is introduced in section two of Chapter 17, distinguishes three different possible determinants of the own-rate of

interest, identifying them with the advantages or the disadvantages attached to the possession of the different commodities and assets, that is: the yield *(q)* (if the commodity is employed in the process of production or in consumption), the carrying cost *(c),* and the liquidity premium *(l)*. So that, according to Keynes,

> the total return expected from the ownership of an asset over a period is equal to its yield *minus* its carrying cost *plus* its liquidity-premium, i.e. to q-c+l. That is to say, q-c+l *is the own-rate of interest of any commodity,* where q, c and l are measured in terms of itself as the standard.
>
> (Keynes 1936:226; emphasis added)[22]

On the other hand, in order to compare the different rates of interest, i.e. the expected rates of return, on the different commodities and assets, each measured in terms of itself, one needs a common standard. Taking money as the standard of value–but, as Keynes states, 'we could equally well take wheat' (Keynes 1936:227), that is any commodity–one needs also to consider the expected appreciation (or depreciation), and the definition of the rate of interest will then be accordingly modified (D3). Referring to Keynes' own example, the own-rate of interest in terms of money relative to the possession of a house (the expected money return), i.e. 'the house-rate of money-interest' (Keynes 1936:227),[23] will be given by a+q, where a is the expected appreciation in terms of money (for a period of a year), that is the percentage excess of the expected forward price over the spot price of the commodity (here, the house), in terms of money, and q is the own-rate of interest on the house in terms of houses as previously defined (see D2). (In the example, we suppose that the carrying cost and the liquidity premium be, in the case of a house, negligible.) Following Keynes, 'the money-rate of money-interest', i.e. the own-rate of interest on money in terms of money, will be given by l (supposing that, in the case of money, the yield q and the carrying cost c be negligible). In equilibrium, a+q must be equal to l (cf. Keynes 1936:227–8); therefore, assuming that l is given, it will be a and q that change as to ensure the equilibrium condition. Thus, it is the expected rate of return on assets other than money that will become equal to the money interest rate, i.e. to the liquidity premium. I will now consider the second point: the notion of liquidity premium and the consequent determination of the money interest rate.

In Chapter 17 of the *General Theory,* Keynes rests the determination of the rate of interest on the liquidity premium notion: the money interest rate is not determined any more in terms of demand and supply, as in Chapters 13 and 15, but only on the liquidity premium.[24] By 'liquidity premium' Keynes means the amount individuals are willing to pay for the 'potential convenience or security' given by the 'power of disposal over an asset' for a specified period of time, excluding its yield and its carrying cost (Keynes 1936:226). To Keynes, money is not the only asset that has a liquidity

premium, but it is the only asset whose liquidity premium 'much exceeds' its carrying cost, contrary to the case of other assets.[25] Money is therefore the liquid asset *par excellence,* and it is held precisely for such a characteristic. It is worth noticing that this peculiar feature of money is not due to its physical quality, but to a convention. As it is shown by the example of land, it is at the same time a subjective and an historical convention:

> The conception of what contributes to 'liquidity' is a partly vague one, changing from time to time and depending on social practices and institutions. The order of preference in the minds of owners of wealth in which at any given time they express their feelings about liquidity is, however, definite and is all we require for our analysis of the behaviour of the economic system.
>
> (Keynes 1936:240–1)

We may conclude the exposition of Keynes' main theses on the theory of interest by recalling that it was precisely this last question–the nature of the liquidity premium–which occupied Keynes' mind after the publication of the *General Theory,*[26] but it is now time to turn to Sraffa's critique.

5. In the Cambridge oral tradition, Piero Sraffa's judgement on Keynes' *General Theory* was well known: a rather 'muddled', or 'confused' book (whereas Sraffa's opinion with regard to the *Treatise on Money* was more favourable, or at least less critical. According to Sraffa, Keynes' best book remained however *The Economic Consequences of the Peace*).

On the basis of the documentation provided by the *Sraffa Papers (SP),* available at the Wren Library of Trinity College, Cambridge,[27] it has now become possible to understand and evaluate Sraffa's reasons of agreement and disagreement with the *General Theory* better. I will not dwell on the critical comments on matters of definitions or of incoherence between different parts of the *General Theory* which Sraffa, as one would expect from such a fastidious mind, scatters through his analysis, but I will get straight to the core of the question, that is to Sraffa's critique of the theory of money and interest which Keynes put forward in Chapter 17. It is worthwhile noting that this chapter has been generally considered as the most Sraffian. Furthermore, together with Chapter 16 ('Sundry Observations on the Nature of Capital'), Chapter 17 has to be viewed as a development of the controversy on money and capital which had opposed Keynes and Sraffa to Hayek a few years earlier. Although some interpreters of Keynes have judged Chapter 17 as an unnecessary *détour* from the main argument of the *General Theory,* it testifies however the importance and the attention Keynes continued to pay to the questions debated in 1932, and therefore to Sraffa's theoretical stance–which was so decisive in winning the controversy with Hayek.

In short, Sraffa addresses two main objections to Keynes' theory, and both are, as one would have expected, radical, if not 'destructive'.[28] The first critique concerns the liquidity preference theory on which, as we have seen in the previous section, Keynes based the determination of the money interest rate. The second critique concerns the notion of own-rates of interest, and then the relation established by Keynes between that notion and the notion of marginal efficiency of capital. To the liquidity preference theory, Sraffa objects that a unique functional relationship between the rate of interest and the quantity of money held does not exist. Therefore, that descending curve (demand curve) cannot exist, which, according to Keynes, represents the preference for liquidity (for the community as a whole). As regards the Sraffian (!) notion of commodity-rates or own-rates of interest, Sraffa remarks that Keynes confuses this notion with that of the marginal efficiency of capital. I will now proceed to a more detailed analysis of the two critiques.

Sraffa's objections are not addressed to the stability or the independence of the curves that, in Keynes' theory, would determine the rate of interest,[29] but mainly to the shape of the liquidity preference curve. According to Sraffa, the liquidity preference curve is (in Keynes' theory) always decreasing, as a normal demand curve, since Keynes supposes that to hold money is always an advantage, though diminishing. According to Sraffa, Keynes thus identifies liquidity with utility or, more precisely, liquidity preference with the marginal utility of holding cash. Hence, liquidity (or money) has the same properties that in general are attached to utility, i.e. liquidity (or money) has a decreasing marginal utility. From this, Keynes derives the idea that, in order to induce people to part with liquidity (money) or, more precisely, with the pleasure provided by liquidity (i.e. utility), ever increasing quantities of interest are needed. Now, Sraffa's thesis is that, in general, this is not the case. Instead, according to Sraffa, 'liquidity (in the various senses used by Keynes) is an advantage to some people and a positive disadvantage to others' (Sraffa, *SP* I/100). In fact, Sraffa points out, there are agents on the market who, as the interest rate falls, decrease their liquidity, in order to keep up their income level; typically, they are people whose income is derived mainly, if not exclusively, from interest. It logically follows, that the relation between the quantity of money held and the rate of interest cannot have the form of an inverse relation, as it was supposed instead by Keynes. More in general, and this is Sraffa's first conclusion, it is impossible to say that there is a definite relation between the quantity of money and the rate of interest; therefore, it is impossible to say with certainty which will be the effect of a change in the interest rate on the quantity of money held. (In connection to this point, Sraffa's example is that of a man faced with the choice of dividing a sum between investment and cash: a change in the rate of interest could work either way.) According to Sraffa, in spite of Keynes' explicit rejection of the neoclassical idea of a functional dependence of the supply of savings on the

rate of interest, the old theory would re-emerge in Keynes' concentrating on the supply of loans.

Let us now come to the more 'positive' part of Sraffa's critique.

If the Keynesian liquidity preference curve 'does not exist', still 'the fact remains', Sraffa states, 'that abundant cash and low interest go together' (Sraffa, ibid.). How then should one explain this empirical phenomenon? (A phenomenon which, on the other hand, as Sraffa also observes, seems to lend plausibility to Keynes' thesis of an inverse relation between the rate of interest and the quantity of money.)

Sraffa's thesis here is that the causal order should be reversed: 'it is a low interest rate which is the cause of abundant money–not abundant money which causes low rate' (Sraffa, ibid.). In fact, whereas in Keynes' analysis the rate of interest is affected by the lenders' liquidity preference, according to Sraffa the attention should instead be focussed on the borrowers' actions, i.e. not on the supply but on the demand for loans. Sraffa's attention is thus lent on the banks' behaviour. Stressing what he viewed as the correct causation, he states that in order to issue more cash by lending, banks must reduce the interest rate 'in order to find the *borrowers*', because 'people dont borrow unless they find profitable outlets' (Sraffa, ibid.). According to Sraffa, and contrary to the neoclassical assumption, such a fall in the interest rate would *not* affect the lenders' behaviour. On the other hand, increasing the quantity of money is not a sufficient reason for the rate of interest to be affected (i.e. to decrease). In fact, 'if money is originally issued not as loans, but in payment for goods–there is no reason why rate should fall' (Sraffa, ibid.). (Quite significantly, Sraffa's example is war inflation.)[30] If the attention is thus shifted to the demand (for loans) side, it becomes necessary to examine the relation between the rate of interest and the marginal efficiency of capital (to which the demand for loans is in some way connected). Let us then examine Sraffa's second critique of the Keynesian theory.

The 'main point' of Sraffa's second critique is the confusion, into which Keynes falls in the second section of Chapter 17 of the *General Theory,* when he identifies the commodities own-rates of interest with their marginal efficiencies. In fact, as we have seen in the previous section, Keynes presents three different definitions of the rate of interest. Sraffa has nothing to object to the first one (which was, we recall, precisely that given by Sraffa himself in his article against Hayek's theory). However, Sraffa rejects Keynes' second and third definitions (in which that confusion would arise). Let us try to understand Sraffa's reasoning.

In the second section of Chapter 17, Keynes' definitions of the rate of interest are built upon the hypothesis that 'the variety in the rates of interest on different articles corresponds to the different advantages or disadvantages (yield, carrying cost, liquidity) attached to their possession' (Sraffa, ibid.). Furthermore, in a marginal note, Sraffa adds that Keynes 'goes far as *defining* these advantages as the rate of interest' (Sraffa's emphasis). To Keynes'

argument and definitions, Sraffa objects that differences in the own-interest rates of the various commodities are due not to a difference in the 'objective' advantages, or disadvantages which the different commodities provide to their owners, but only to the possible changes in their expected (relative) prices:

> if no changes in price are expected *all* commodities will have the same rate of interest, whether it be a delight or a nuisance to possess them: the discrepancies can *only* be due to expected changes in relative prices.
>
> (ibid.; Sraffa's emphasis)[31]

Behind Keynes' hypothesis, Sraffa argues, there is the assumption that people borrow a commodity 'in order to keep it and to enjoy its advantages (liquidity for money, use for house, carrying costs for wheat)' (Sraffa, ibid.).[32] Hence the idea that commodities are held until the end of the loan, and consequently that only durable assets can be borrowed. Even to this assumption, Sraffa's objection is strong:

> But in fact people borrow money for parting from it, and buying things: the thing they borrow is, not what they want to use, but the standard in which they fix their debt: thus they might borrow fresh fish for 100 years, although it has neither liquidity preference, nor use at so much per annum–and it would have almost infinite carrying costs.
>
> (Sraffa, ibid.)

For these reasons, according to Sraffa, Keynes' argument in the second section of Chapter 17 of the *General Theory,* should be related to investment in fixed capital rather than to borrowing activity. If it is so, then it would have been more logical to refer directly to the marginal efficiencies of different capital goods, rather than to their own-interest rates. However, if even this necessary correction is granted, Keynes' main conclusion–i.e. that, because of the 'special characteristics' of money, the money rate of interest is more 'reluctant' to fall relatively to the own-rates of interest of the other assets (and therefore could remain fixed at a higher level)–would be self-contradictory. In fact, as Sraffa argues in a passage in which he develops Keynes' argument in such a way to exhibit its internal inconsistencies,

> if there is one article the marginal efficiency of which never falls below say 5% (this being the valuation of the pleasure people derived from hoarding any quantity of it) the production of all other durable assets will stop when their stocks are such that marginal efficiency has come down to that level–for otherwise they could not be sold at cost–and all be used for producing the hoardable asset. If this asset cannot be produced (paper money), its

> demand will increase and can only be met by a continuous rise in its value, i.e. fall in general prices. If this hoarding is expected to go on steadily, and all prices are expected to fall in terms of money, the result will be that all own rates of interest of commodities will be higher than the money rate.[33]
>
> Thus in the Keynesian case, the result on rates of interest is opposite to Keynes's conclusion.
>
> (Sraffa, ibid.)

Sraffa's critique to Keynes' theory of interest can be therefore summed up as follows. The 'substance' of Keynes' *General Theory* lies in the idea that, contrary to neoclassical theory (whichever its version), the rate of interest is a monetary phenomenon that does not depend upon the marginal productivity of capital, but 'on something entirely different', that is liquidity preference. On the other hand, a closer inspection at/into Keynes' notion of liquidity preference reveals that it actually corresponds to the marginal utility of holding money, 'i.e. one of the aspects of the marginal productivity of capital', and that is therefore 'determined in the same way' (Sraffa, ibid.).

I now intend to put some elements forward which may help to understand Sraffa's critique better. In the first place, although without any pretence to a detailed analysis, I will point out some elements of the intellectual context into which Sraffa's critique should be placed, so that its original and distinctive features can be better appreciated. In the second place, I will advance an hypothesis regarding the influence of Sraffa's critique on Keynes' own theory after the *General Theory*.

6. As is well known, the theory of money and interest that Keynes advanced in the *General Theory* aroused many discussions even while it was being elaborated, prior to its publication in 1936, and even more vivid and ample debates and controversies after its publication. In addition to Sraffa, the Cambridge economists who were closer to Keynes and who participated in this critical activity were Hawtrey, Robertson, Champernowne, Reddaway, Hicks, Joan Robinson and Kahn, not to mention Pigou. Among those outside Cambridge, one should mention at least Townshend and Kaldor, and the debate Keynes had with Ohlin and Viner.[34] Not being able, on this occasion, to review and discuss the whole of this complex and important literature thoroughly enough, I will limit myself to a brief comparison between Sraffa's critique and some arguments put forward by Robertson. In those years–1935–9–Robertson was quite close to Sraffa, and had long and intense discussions with Sraffa on Keynes' 'new' theory. Robertson too was very critical of Keynes' theory of money and interest, as it results from the unpublished material we find in volumes 13, 14 and 29 of Keynes' *Collected Writings,* as well as from a series of published contributions, starting from his 1936 review article of the *General Theory* in the *Quarterly Journal of Economics,* in

which Sraffa's influence is acknowledged with quite strong and characteristic emphasis.[35] In particular, it was the theory of liquidity preference that failed, according to Robertson, to support the weight of Keynes' argument; i.e. Keynes failed to show how the rate of interest 'does not depend at all on the demand for loanable funds for use in investment' (Robertson 1936:175).[36] In another very telling passage, Robertson stated, in a very Sraffian mould:

> Of course Mr. Keynes never really succeeded in banishing the influence of marginal productivity; it crept in again at the back door under the wing of the 'demand for money' for purposes connected with the conduct of business and the disbursement of income.
>
> (Robertson 1940:11)

Moreover, and again in a very Sraffian fashion, Robertson strongly opposed Keynes' idea that people would demand money in order only to be liquid: 'people do not borrow money in order to be liquid,–the liquidity obtained by holding a borrowed bank balance is an extremely meretricious form of liquidity!' (Robertson, in Keynes *CW,* XIII:509). In fact, according to Robertson, and in accordance with Sraffa's argument, 'people borrow money in order to part with it for commodities and services' (ibid.), and therefore, Robertson adds, 'it is monstruous in discussing its own-rate, as Keynes does in chapter 17 of the *General Theory,* to omit all mention of the thing–productivity, positive or negative–which in your [i.e. Keynes'] theory dominates every other own-rate' (ibid.). It is interesting to note, here, that Hawtrey too had exposed the weakness of the liquidity preference function along very similar lines. For example, in a letter to Keynes, Hawtrey had observed that the liquidity function 'seems to be essentially inapplicable to a borrower. No one borrows money to hold it idle'.[37] We have no time, nor space, to examine here Keynes' rejoinders to this sort of criticism levelled at his liquidity preference theory, which, as is well known, continues to be one of the most controversial aspects of his theory.[38] However, it is important to notice that Keynes, replying to his critics on this point, always kept stressing his distance from the orthodox theory: to Keynes, the rate of interest could not be related, in any simple or direct way, to the productivity of capital as it was supposed by the orthodox theory.[39]

A *caveat* is here in point. The affinities and correspondences between Sraffa's and other Cambridge economists' criticism of Keynes' theory we have pointed out should not conceal the radical difference in Sraffa's general approach to economic theory. While Keynes waved between tradition and revolution, and Robertson and others argued within the overall framework of the neoclassical theory, still believing in the validity of marginalistic tools (first of all, of the notions of marginal utility and marginal productivity), trying to take Keynes back to the Marshallian home, Sraffa had already for quite some time completely broken, and cut any link, with marginal theory

and method. This results very clearly from his twofold critique of Keynes' theory of interest. The criticism levelled at the notion of liquidity preference is in fact due not only to the internal weaknesses and inconsistencies of this notion, as they were pointed out by Robertson and by others, as well as by Sraffa, but also and above all to the refusal of the possibility of establishing in general a *functional* relation between the rate of interest and the quantity of money. This makes the difference between Sraffa's critique and that by the other contemporary Cambridge economists. In fact, in the 1925 article and then again in the 1928–30 *Lectures on Advanced Theory of Value,* Sraffa had clearly criticised the possibility of building meaningful relationships of such kind. In economic theory, the only sound relationships–namely, those that allow the theorist to establish significant causal nexuses–are, according to Sraffa, the descriptive ones: a descriptive relationship is, for example, the kind of relationship Sraffa identifies between the quantity of money and the rate of interest, as we saw earlier on.[40]

With reference to Sraffa's second criticism, it is also clear that Sraffa could not accept Keynes' identification of his notion of commodity rate of interest (by Keynes dubbed as own-rate of interest) with the neoclassical notion of marginal efficiency. As I have pointed out in section three, the Sraffian notion of 'natural or commodity rate of interest' is entirely void of any marginalistic element. Now, even if we leave the controversial issue of its more or less intrinsically marginalistic nature aside,[41] the Keynesian notion of marginal efficiency of capital appears to specify a functional relationship between the own-rate of interest (marginal efficiency of capital) and the level of investments that Sraffa could not accept. The difficulties in Keynes' theory of investment, and in particular the question of how it was possible to preserve the necessary independence between the demand for investment and the rate of interest,[42] could not escape Sraffa's critical analysis.

Before making some conclusive remarks, I now wish to advance a conjecture regarding the influence Sraffa's critique might have had on Keynes.

From the point of view of the history of economics, it would be interesting to find out whether Keynes knew Sraffa's critique and, even more interesting, what he thought of it. To my present knowledge, I may only venture in saying that Keynes was aware of it, as he was aware of the contemporary criticisms raised by Roberston and by other Cambridge economists. Furthermore, I believe that Keynes actually reconsidered his theory of interest in the light of this critique. In fact, if we examine Keynes' writings after the publication of the *General Theory,*[43] we may find various indications of Keynes' dissatisfaction with regard to his theory of interest and money as it was expounded in the *General Theory,* and especially in its Chapter 17. What is more relevant from our point of view, in Keynes' subsequent attempts at reformulating his theory of interest and money, the notion of own-rate of interest was abandoned. This is, I believe, a clear sign that Sraffa's critique

indeed left its mark. On the other hand, even in these later writings Keynes kept wavering between Marshallian orthodoxy and economic heterodoxy, between a determination of the interest rate in terms of supply and demand curves and as a conventional phenomenon.

7. To conclude, it is worthwhile reconsidering Sraffa's paragraph 44, and therefore the relationship between his theory and that of Keynes, in the light of the path followed throughout this paper. What new or sounder interpretation can we offer of the proposition that 'the rate of profits is susceptible of being determined outside the system of production, in particular by the level of the money rates of interest? Is this really an implicit reference to and an acceptance of the substance of Chapter 17 of the *General Theory* (as some interpreters have argued, as we already mentioned)? The answer is twofold: on the one hand, the strong objections Sraffa raised against Keynes' notions of the own-rate of interest and liquidity preference prevent one from reading Sraffa's proposition as an endorsment, though implicit and allusive, of the way Keynes related the rate of profits to the rate of interest in Chapter 17. On the other hand, on the more positive side of the matter, the new evidence available clearly confirm a strong agreement between Sraffa and Keynes both on a monetary and conventional determination of the rate of interest and on the direction of the causal nexus between the two (namely, from the money rate of interest to the rate of profits). Actually, in the *Sraffa Papers* we find passages which are more explicit than the published paragraph 44, although characteristically worded with various provisos and cautions. For example, in one passage Sraffa states that 'it is possible to conceive of it [the rate of profits] as being "given" from outside the system of production, much as conforming to the pattern of money rates of interest determined independently by the banking system or the Stock Exchange.' (Sraffa, *SP* D3/12/78). In another passage, Sraffa affirms that the rate of profits is 'a reflection of the interest rate which is determined by the operation of the Stock Exchange or regulated by banking policy' (ibid.). It is also very significant that at the margin of those very passages in the *General Theory* where Keynes states the conventional nature of the rate of interest, Sraffa had annotated 'è così che si fa una "teoria"'.[44] With all that said, we are however left with a problem, or a gap: by rejecting the theory presented by Keynes in Chapter 17, we lack a sound and satisfactorily specified theory of the relationship between the rate of profits and the rate of interest along Keynesian and Sraffian lines. This is however a problem we cannot deal with here.

Appendix

In this appendix I provide the formal definitions of the rate of interest relevant to the preceding analysis.

Sraffa's 'natural or commodity rate' can be defined, in physical terms, as follows:

$$i_j \equiv \frac{q_j^f - q_j^s}{q_j^s} = \frac{q_j^f}{q_j^s} - 1 \tag{S1}$$

where j is any commodity, q_j^f and q_j^s the quantities of it exchanged in, respectively, the forward and the spot markets. If the relevant commodity were money, m, we would have

$$i_m \equiv \frac{q_m^f}{q_m^s} - 1 \tag{S2}$$

that is the definition of the *money* rate of interest.

One can therefore define the 'natural' rate in terms of money as

$$i_{jm} \equiv \frac{p_j^s}{\dfrac{p_j^f}{1+i_m}} - 1 \tag{S3}$$

where p_j^s and p_j^f are, respectively, the forward and spot money prices of the commodity j and i_m is the money rate of interest as defined sub S2.

From S3 one can derive the following expression:

$$i_{jm} \approx i_m - \frac{p_j^f - p_j^s}{p_j^f} \tag{S4=D1}$$

which, approximately, corresponds to the definition Sraffa gave in the 1932 article against Hayek: the 'natural' rate of interest is equal to the 'interest on the money, plus the excess (or minus the deficiency) of the spot over the forward prices' (Sraffa 1932a:50).

This definition by Sraffa exactly corresponds to the first definition Keynes gives, in Chapter 17 of the *General Theory,* of the *own-rate of interest* (*CW,* VII: 223) and to the definition Hicks gives in *Value and Capital:* 'a commodity rate of interest approximately equals the money rate of interest *minus* the contango (percentage excess of futures price over spot)' (Hicks 1939:142).

The rate of return expected from the ownership of a commodity or an asset, with which, in the second section of Chapter 17, Keynes identifies the own-rate of interest, can be thus formulated:

$$\rho_j = q_{jj} - c_{jj} + l_{jj} \tag{D2}$$

where q, c e l represent, respectively, the return, the carrying cost, and the

liquidity premium associated to the ownership of commodity or asset j in terms of itself. If, following Keynes, one defines such a rate in money terms, therefore considering the expected appreciation (or depreciation) of the commodity, the preceding expression transforms into

$$\rho_{jm} = \rho_j + a_{jm} \tag{D3}$$

where a_{jm} represents the expected appreciation (or depreciation) of commodity or asset j in terms of money, that is

$$a_{jm} \equiv \frac{p_j^f - p_j^s}{p_j^f}.$$

Following Keynes, in the state of equilibrium, the expected rates of return, or own-rates of interest, will be equal to the own-rate on money, uniquely determined by the liquidity premium, i.e.

$$\rho_j + a_{jm} = l_m \equiv i_m$$

and therefore

$$\rho_j = i_m - a_{jm} \tag{D4}$$

that is the expected rate of return, or own-rate, of a commodity is equal to the money rate of interest *minus* the appreciation (or depreciation) of the commodity in terms of money. Comparing the last expression with S4 (equal to D1), one can see their formal identity: both the *commodity rate* and the *own-rate* of interest are, in fact, given by the difference between the money rate of interest and the excess of the expected over the spot prices.

I have written *formal* identity, for both the vision and the theory that stay behind these two formally equal expressions are different.[45] In fact, whilst in Sraffa's theory, given the money rate of interest, the 'natural' rate follows from the divergence between the forward and spot prices of commodities, in Keynes' theory, given the divergence between the money rate of interest (determined by the liquidity preference) and the own-rates of other assets (determined by their expected returns), it follows the appreciation (or depreciation) of the commodities values. The causal nexus is, therefore, different. Furthermore, in Keynes' theory, the own-rate of interest is assumed to be directly linked to the rate of return expected 'by assisting some process of production or supplying services to a consumer' (Keynes 1936:225). Hence, the theory of value implied in the analysis by the two economists does not seem to be the same.

Notes

1 I wish to thank Pierangelo Garegnani, who offered valuable suggestions and, as Piero Sraffa's literary executor, gave permission to quote from the unpublished papers. The assistance and kindness of Ian Smith and the other librarians at the Wren Library, Trinity College, and of Jacqueline Cox at King's College Library are also gratefully acknowledged.

2 'Keynes and Sraffa: a case of non-communication' is the title of Skidelsky's contribution to a volume of essays on Sraffa (see Bellofiore 1986). See also Skidelsky (1992:290): 'If Sraffa's ideas gave Cambridge its second (that is, post-Marshallian) wind, it was not a wind which filled Keynes' sails. Although he found Sraffa's work "very interesting and original"…he was too absorbed in his own project to see its relevance to his concerns…. Keynes did not think the theoretical problems for value theory raised by Sraffa of serious practical importance–an attitude he was to take to the whole of Sraffa's work.'

3 To mention only one for all, see Schumpeter (1954).

4 See Marcuzzo's chapter in this book.

5 This episode was recounted to me by Giovanni Malagodi, whose mother was English and whose father was a friend of Keynes.

6 'Entrepreneur economy' is both Keynes' and Sraffa's way of defining the object of the Keynesian economic theory.

7 Other aspects of the relationship between Sraffa and Keynes are considered in Ranchetti (1996, 1999).

8 Sraffa (1932a). I recall that Sraffa's critique is part of the controversy between Hayek and Keynes on the latter's *Treatise on Money*. I also recall that the idea of a own-rate of interest harks back to Keynes' *Tract on Monetary Reform* and is also traceable in Fisher (1896). In this section I limit myself to a brief exposition of the notion of natural or commodity rate of interest as presented in Sraffa's *Economic Journal* article in 1932. For a fuller account of Sraffa's critique of Hayek and on the controversy between Keynes and Hayek, see the recent analysis by Kurz (1995).

9 For all these definitions, see the Appendix.

10 More precisely, the commodity rate of interest depends also on the divergence between the spot and the forward price of the commodities.

11 By 'Classical theory', here and in the following pages, I refer to the neoclassical or marginalist theory.

12 In addition to Hayek, the *locus classicus* of this thesis is Wicksell, *Lectures,* Part Five, Chapter 9.

13 Again, as in the case of the previous section, this section is not meant to be a complete analysis of Keynes' theory of money and interest in the *General Theory,* even though limited to Book IV, but is purely instrumental to a better appreciation of Sraffa's critique as it is considered in the next sections of the chapter.

14 On the existence of two different lines of research in Keynes' *General Theory,* see Garegnani (1964–5) and, more recently, Panico (1987).

15 Namely the quantity of money held for the speculative-motive, that is for 'the object of securing profit from knowing better than the market what the future will bring forth' (Keynes 1936:170).

16 As it has been observed (Asimakopulos 1991:93), being based upon the effect of a change in income, this argument points to a shift of the curve, rather than to its shape.

17 The elasticity of the liquidity preference curve with respect to the rate of interest implies the existence of two different rates of interest: the current or market rate and the rate people expect in the future, namely the normal or long-run rate of interest. See Tobin (1958), Chick (1983), Eatwell (1987) and Trevithick (1992). On the shape of the liquidity preference function, see also Lange (1938).

18 On this proposition, in addition to Garegnani (1964–5) and Panico (1987) already quoted, see Vicarelli (1977), Carabelli (1988), Ciocca and Nardozzi (1993).
19 See, for example, Keynes (1973b, vol. XIV:123).
20 The differring interpretations go from Hawtrey's enthusiastic statement ('the most original part of the book') to Hansen's very dismissive view ('not much would have been lost had it never been written'), passing through Kaldor's more equilibrate appraisal. See Hawtrey 1937a; Hansen 1953:159; Kaldor 1980:6. Keynes himself was well aware of the difficulties with the material of chapter 17: 'I admit the obscurity of this chapter', he wrote to Robertson (1973a, vol. XIII: 519).
21 See Dardi 1979:460, Asimakopulos 1991:106, and Bonifati 1991:103–7.
22 It is to be noticed that Keynes himself (1973b, vol. XIV:76), provoking Hicks's surprise, stated that this had to be considered as the most 'formal and accurate' definition of the rate of interest.
23 Significantly, in his own copy of the *General Theory,* Sraffa put a question mark next to this definition. Perhaps, it would have been clearer if Keynes had called it 'the money-rate of house-interest'.
24 It may be argued that Keynes, dissatisfied with the theory put forward in Chapters 13 and 15, which we have summarised in propositions K1–K3 of the preceding paragraph, thought to provide in this way a different and more robust foundation for his theory of interest. On the plausibility of this hypothesis, see Mongiovi 1990.
25 Cf. Keynes 1936:226: in the case of money, 'its yield is *nil,* and its carrying cost negligible, but its liquidity-premium substantial'. A monetary economy would thus be an economy in which a commodity or asset exists whose liquidity premium is always greater than its carrying cost, whereas a non-monetary economy would be an economy in which no such a commodity or asset exists.
26 The discussion between Keynes and Townshend was particularly relevant. See, for instance, Keynes' letter to Townshend of 7 December 1938: 'A liquidity premium is a payment, not for the expectation of increased tangible income at the end of the period, but for an increased sense of comfort and confidence during the period' (Keynes 1979, vol. XXIX:294).
27 The following analysis is mainly based on some unpublished material which probably dates back to 1936–37, referred to in the *Sraffa Papers'* catalogue as I100.
28 'Destructive' is the term Keynes used to define Sraffa's criticism of Marshallian theory during the 1930 *Economic Journal* Symposium on Increasing Returns. See Sraffa 1930.
29 In the texts which I am considering, Sraffa prefers to 'leave aside' these other 'difficulties' of Keynes' theory.
30 One needs not call the attention to the title of Sraffa's dissertation: *L'inflazione monetaria in Italia durante e dopo la guerra (Monetary Inflation in Italy during and after the War* (see Sraffa 1920). See also Sraffa's other monetary essays as listed in Ranchetti 1999. On Sraffa's monetary thought see Panico 1988.
31 Actually, Sraffa points out two possible exceptions: agricultural products from one harvest to another ('where the cost of carrying is in effect part of the cost of production'), and markets in which 'hedging is all one way' (and the insurer's profit has therefore to be taken into account).
32 Money, house and wheat, are the three examples presented by Keynes.
33 Sraffa remarks that this is 'Fisher's effect': the expected appreciation (or depreciation) is the only possible cause for a divergence between the various rates of interest
34 I am strictly limiting myself to mention those authors who stressed the importance of Chapter 17 in the interpretation of Keynes' theory of interest and money. See the *Collected Writings of J.M.Keynes,* vols XIII, XIV and XXIX.

35 See, e.g., Robertson 1936:168 and Robertson 1940 (but they were lectures delivered at the London School of Economics in 1939), p. viii: 'Among the many published appraisals of Mr. Keynes' latest work, I have, I think, derived most benefit from those by Mr. Hawtrey and Mr. Hicks; among many conversations on the same theme, those with Mr. Sraffa stand out in memory as most inevitably ending in theft.'

36 Andrea Ginzburg had already, before Sraffa unpublished papers were available, sharply noticed, how Robertson's 'criticisms, from Sraffa's point of view, could be understood as the attempt at showing that the theory of liquidity preference did not provide arguments strong enough to support, in opposition to the 'Classical' theory of interest, the proof of the existence of an unemployment equilibrium.' Ginzburg 1986:69.

37 Hawtrey's argument is that 'When a man borrows money to buy goods he prefers acquiring the goods to holding the money idle, and he also prefers acquiring the goods to avoiding the indebtedness. But no comparison of the relative advantages of avoiding the indebtedness and holding the money idle is involved' (Hawtrey, letter to Keynes, 1 February 1936, in Keynes 1973a, vol. XIII: 9).

38 For recent attempts at making sense of Keynes' liquidity preference theory see, e.g., Panico 1987, Bonifati 1991, Lawlor 1994, Runde 1994, Winslow 1995.

39 See, e.g., Keynes' reply to Hawtrey in Keynes 1973b, vol. XIV:11–3.

40 For an analysis of Sraffa's 1925 article see Ranchetti 1996, in chap. 12 of Ingrao and Ranchetti. On the important distinction between functional and descriptive relationships, see Roncaglia 1975:119–21 and Ingrao and Ranchetti, chapters 10 and 12.

41 Cf. Lerner 1952, Ackley 1961, Garegnani 1964–65, Pasinetti 1974, Vicarelli 1977, Tonveronachi 1983, Asimakopulos 1991, Bonifati 1991, and Pasinetti 1997.

42 On this question see Garegnani 1964–65.

43 See especially the *Economic Journal* article 'The Theory of the Interest Rate' and the *Quarterly Journal of Economics* article 'The General Theory of Employment', both published in 1937 and reprinted in Keynes 1973b, vol. XIV, where one can also appreciate Keynes' reaction to the various criticisms levelled against his theory.

44 The annotation is at page 203 of Sraffa's personal copy of the *General Theory,* kept at the Wren Library, Trinity College under S2644.

45 As it has been precisely noticed by Bonifati 1991:104–5.

20 Sraffa on Hayek

Unexpected influences

Carlo Zappia[1]

> With or without money, if investment and saving have not been planned to match, an increase of saving must prove to a large extent 'abortive'.
>
> (Sraffa 1932a:207)

> There can be no doubt that the decisions of the consumers as to the distributions of consumption over time are something separate from the decisions of the entrepreneur capitalist as to what quantities of consumers' goods he should provide for different moments of time. And the two sets of decisions may or may not coincide.
>
> (Hayek 1941:336)

Introduction

The debate on monetary theory and business cycle theory in the 1930s is still at the centre of the discussion among economists. The Hayek-Sraffa exchange, in particular, has originated contrasting interpretations and interpretative puzzles which still come to the fore.[2]

There are mainly two different approaches in the secondary literature discussing the Hayek-Sraffa exchange. On the one hand, a number of authors explicitly link the debate on *Prices and Production* between Hayek and Sraffa with the debate on the *Treatise on Money* between Keynes and Hayek. Sraffa's critiques are interpreted as supporting Keynes in his attempt to divert the interest of the economics profession towards a macroeconomic approach in the face of the mainstream inability of explaining permanent unemployment: undermining the soundness of Hayek's theory of a real economy was Sraffa's main goal, on behalf of Keynes (among others, see Milgate 1982; Eatwell and Panico 1987; Mongiovi 1990; Dostaler 1991). On the other hand, many critics have dedicated their interest to the respective internal consistency of Hayek's and Sraffa's arguments and to the intricacies of Sraffa's review and Hayek's reply (among others, see Desai 1982; Lachmann 1986; Lawlor and Horn 1992; Cottrell 1994, Kurz 1995).

My main goal in this chapter is not to provide a comprehensive assessment of the debate. The aim is to investigate whether, contrary to Hayek's assertion, Sraffa's comments had enduring effects on the evolution of Hayek's thought in the 1930s and early 1940s. As is well known, Hayek's 'transformation' in the mid-1930s is usually characterised as a retreat from general equilibrium theorising (Caldwell 1988; Foss 1995), that is from the framework Hayek considered as firmly established in the development of his theory of business cycle in the late 1920s and early 1930s (see in particular Hayek 1933a, Ch. I). My main point in the chapter is that Sraffa's comments on *Prices and Production* have a relevant part in the story of Hayek's transformation, though of course Hayek never followed Sraffa's suggested way out from mainstream equilibrium theory, which found internal consistency in Sraffa's 1960 volume. In order to argue my point, two issues are examined in detail. First there is an analysis of the development of the notion of intertemporal equilibrium. Hayek introduces intertemporal equilibrium in a 1928 article, but seems to make no use of it in *Prices and Production,* and seems to be surprised by Sraffa's reference to the divergence between the own rates of interest of different commodities (as noted by McCloughry 1982). I show that Hayek's reaction to Sraffa's point is the first example of that revision in his appreciation of the characteristics of a non-monetary economy announced by Hayek himself in the preface to the second English edition of *Prices and Production,* and constituting the basis for the *Pure Theory of Capital.*[3] Second, there is an analysis of the different attitude shown by Hayek in the late 1930s as regards the ability of real economic systems to automatically adjust to exogenous real shocks. I argue that Hayek's viewpoint that monetary shocks are a necessary conditions for business cycles to occur, his main thesis in *Prices and Production,* is no longer held in *The Pure Theory of Capital,* though he still maintains, on different grounds, that monetary shocks are the most relevant sufficient condition for business cycles.

A preliminary factual question regarding my interpretation is worth noting from the outset. Hayek did not explicitly acknowledge the range and power of Sraffa's critique, so evidently superior to those of most contemporary critics. Yet, as many commentators have pointed out (among these, on the Austrian side, Lachmann 1986), Sraffa's critique was likely to have had an impact on Hayek, especially as Sraffa managed to spotlight some implications of Hayek's mode of reasoning which he was not aware of. But to the best of my knowledge there is a single reference to Sraffa's review in Hayek's later writings, and this is in the bibliography of *The Pure Theory of Capital* in spite of the absence of any explicit treatment of Sraffa's views in the text.[4] As a result, it might be concluded that Hayek did not seem to pay much attention to Sraffa's methodological observations after his 1932 reply. However, it might be appropriate to make clear at this early point that, in my opinion, Hayek basically accepts Sraffa's general objection that his model of a non-monetary

economy is not correct, in spite of his vibrant refusal to admit that the review contains a set of criticisms capable of sapping the foundations of his theoretical construction. In effect, both in his reply to Sraffa and, more openly and amply, in his later works, Hayek comes to recognise that his description of the workings of a real economy is faulty–and it is on the basis of such a description that his conception of a monetary economy rests. In particular, in the model which was progressively taking shape in the course of the 1930s, even more room is made for Sraffa's view that savings may prove 'abortive' regardless of the banking system.

The chapter develops as follows. The next section examines the main critical points raised by Sraffa in his review of *Prices and Production* and the details of Hayek's reply on these points. The next section highlights a number of passages from Hayek's later works which I found strictly related to Hayek's 1932 reply to Sraffa's critique concerning the actual workings of a non-monetary economy. In particular, I refer to an excerpt from *The Pure Theory of Capital* which, in my view, denotes a clear shift in emphasis as regards the role of monetary shocks as the necessary cause of the business cycle. The evolution of Hayek's thought in the 1930s and early 1940s, with specific regard to the phenomenon of cyclical disequilibrium, is the topic of the subsequent section. The concluding section summarises the main thesis of the chapter.

Sraffa's review of *Prices and Production* and the ensuing debate with Hayek

There are three foci in Sraffa's review: the role of money in the economic system, the use of the concept of forced saving, and the meaning of the real (or 'equilibrium', as Hayek maintained) rate of interest.[5] Through the analysis of these three points, Sraffa intends to show how Hayek's peculiar distinction between a monetary and a real economy is pointless; hence, since Hayek's business cycle theory is based on the different effects of real versus monetary induced modifications in the rate of savings, Sraffa concludes that the whole construction is a plethoric 'terrific steamhammer' (Sraffa 1932a:201).

Sraffa starts off by observing that money plays no role in Hayek's construction but that of a medium of exchange. Yet, Sraffa continues, this clashes with Hayek's claim that business cycle theory rests on the difference in the workings of a monetary and a real economy respectively. This limitation becomes even more serious if a rule of monetary policy is sought. In Sraffa's words,

> the money which he [Hayek] contemplates is in effect used purely and simply as a medium of exchange. There are no debts, no money-contracts, no wage-agreements, no sticky prices in his supposition. Thus he is able to neglect altogether the most obvious effects of a general fall, or rise, of

> prices. This attitude, which amounts to assuming away the very object of the inquiry, appears to originate in the wellfounded objection to the vagueness of the conception of the 'general price-level' understood as nothing different from one out of many possible index-numbers of prices, and in the opinion that such a conception can have no place in the theory of money. Such a theory, according to him, ought simply to consider the influence of money on relative prices of commodities–which is excellent, provided that money itself is one of the commodities under consideration.
> (Sraffa 1932a:200)

As for forced savings, Sraffa's objection is well known, and often cited. Sraffa criticises Hayek's view that a proper capital accumulation cannot take place in a monetary economy if this accumulation is boosted by credit money without any corresponding increase of voluntary savings by consumers. A short reminder of Hayek's argument is useful at this point. Taking a state of equilibrium as starting point, Hayek characterises the growth of an economy by an increase in the production of capital goods in relation to consumption goods.[6] If this increase is financed by 'real' savings it leads to a new equilibrium which takes account of the changed propensities to save. The same mechanism operates when a new investment in capital goods is financed not by savings but by an expansion of credit autonomously carried out by the banking system. However, since there would be no rise in real savings in this case, the increased investment in capital goods would bring about consumers' 'forced' savings (since during the upswing the required amount of consumption goods is no longer produced). Yet, as investments eventually result in payments to the holders of the primary factors of production, the monetary demand for consumption goods would go up again at a later stage. Therefore, investment in capital goods would be frustrated, and the capital already produced but not yet put to work devalued. In dissenting from the idea of the reversal of capital goods investment or 'capital consumption', Sraffa puts forward a celebrated image: 'As a moment's reflection will show,…, one class has, for a time, robbed another class of a part of their incomes; and has saved the plunder. When the robbery comes to an end, it is clear that the victims cannot possibly consume the capital which is now well out of their reach' (Sraffa 1932a:203–4).

Sraffa's third focus has entailed significant and lasting implications for economic theory. According to Sraffa (1932a:205), Hayek's fundamental mistake is 'the belief that the divergence of rates is a characteristic of a money economy'. But 'if money did not exist, and loans were made in terms of all sorts of commodities', a natural rate of interest could be identified for each of the commodities which are traded in a forward market, and thereby there would exist as many natural rates as the number of commodities, 'though they would not be "equilibrium" rates'.[7] Sraffa elucidates that the equilibrium rate of each good amounts to the point at

which market price equals production cost, a situation in which individual agents do not expect the spot to differ from the forward price. It is only at this point, in fact, that the rate of interest of money and the natural rate (that is, the single rate of interest of all goods) coincide. Sraffa (1932a:206) adds that, in a competitive system, the divergence in the natural rates 'is as essential to the effecting of the transition as is the divergence of prices from the costs of production; it is, in fact, another aspect of the same thing'.[8] But, if one believes, as Hayek does, that what counts for business cycle theory is the divergence between the monetary rate and the natural rate running the whole economy, it becomes apparent that the existence of a number of natural rates corresponding to the number of commodities makes analysis impossible. Actually, 'the "arbitrary" action of banks is by no means a necessary condition for the divergence; if loans were made in wheat and farmers (or for that matter the weather) "arbitrarily changed" the quantity of wheat produced, the actual rate of interest on loans in terms of wheat would diverge from the rate on other commodities and there would be no single equilibrium rate' (Sraffa 1932a:205). Sraffa concludes his treatment of this theme by returning to that crucial question, already considered by Keynes (1931:151–4) in his reply to Hayek's review of the *Treatise,* of the autonomy of saving and investment decisions; he quotes Robertson stating that 'savings may be the "inducement" but cannot in general be the "source" of investment' (Sraffa 1932a:208).

By and large, Hayek's answer to Sraffa is concerned only with the second and the third of the three main issues the Italian economist raised. As regards the first, in fact, Hayek merely refers to the forthcoming translation of his previous work on business cycle theory, *Monetary Theory and the Trade Cycle,* where, he contends, he shows his familiarity with the functions of money Sraffa indicated; additionally, Hayek alludes to Sraffa's point about monetary policy as virtually offensive:

> Mr. Sraffa's suggestion that I am surreptitiously shifting my position from the theoretical analysis of 'neutral' money to the defence of one particular maxim of monetary policy is entirely due to his misunderstanding of this point… But there is no justification for the suggestion that after this my exposition illegitimately takes certain aims of economic policy for granted–which I assume "will be found desirable by every right-thinking person'.
>
> (Hayek 1932a:211–12)

As will be shown in the next section, Hayek will take up and clarify this topic in the light of Sraffa's remark in an appendix attached to the second English edition of *Prices and Production.*

To come to forced saving, it has been often recalled that Hayek acknowledges that this is 'a peculiar characteristic of my own theory of the

credit cycle' and 'it is upon the truth of this point that my theory stands or falls' (Hayek 1932a:212). First, Hayek maintains that 'the additional money is, in the normal course of things, lent to somebody who, at that lower rate of interest, is willing to invest more money than before–and to borrow for that purpose', and specifies that 'I do not suggest, and my arguments do not rest on this assumption, as Mr. Sraffa believes, that the banks have "the power to settle the way in which money is spent". The only essential assumption I actually make is that money lent at interest will normally, for the reasons discussed in the text, go to the purchase of producers' goods' (Hayek 1932a:214, 214n.). Granted that in the final analysis the additional money used to purchase capital goods cannot but remunerate the primary factors of production, the demand for consumption goods will eventually return to its initial amount in real terms, albeit with 'a considerable lag', that is 'only when all the new money has passed backwards through the successive stages of production until it is finally paid out to the factors' (Hayek 1932a:215).[9] As a result, 'if entrepreneurs in one stage of production find it impossible or unprofitable to replace for example their machines, then this will cause the capital instruments which are devoted to the production of these machines to lose their value' (Hayek 1932a:216). Hayek judges Sraffa's objection 'a surprisingly superficial objection' since Sraffa neglects the fact that 'capital sometimes falls in value because the running costs of the plant have risen' or it might happen that 'by a sudden increase of the demand for consumers' goods, capital may be destroyed against the will of their owners' (Hayek 1932a:217). With respect to this issue, too, I will argue in what follows that it is possible to trace later clarifications which reveal a partial recognition on the part of Hayek of the importance of Sraffa's criticism, although the distance between the two would not be bridged or shortened as far as the crux of the matter is concerned.

The part of Hayek's reply concerning the natural rates of particular commodities, which has been labelled by Lachmann (1986:237) 'a fatal concession to his opponent', is well known:

> Mr. Sraffa denies that the possibility of a divergence between the equilibrium rate of interest and the actual rate is a peculiar characteristic of a money economy.... It think it would be truer to say that, in this situation, there would be *no single rate* which, applied to all commodities, would satisfy the conditions of equilibrium rates, but there might, at any moment, be as many "natural" rates as of interest as there are commodities, *all* of which would be *equilibrium rates;* and which would all be the combined result of the factors affecting the present and future supply of the individual commodities, and of the factors usually regarded as determining the rate of interest.
>
> (Hayek 1932a:218)

Although Hayek implicitly admits not being able to address the question of the multiplicity of equilibrium rates within the framework of his theory, he seeks to deny that 'the fact that any of these "natural" rates, in terms of a single commodity, may be out of equilibrium in consequence of the disparity between the supply of and the demand for this particular commodity can have effects which are anything like those of a divergence between the actual money rate and the equilibrium rate which is due to an increase in the quantity of money…(with the exception of one particular case)' (Hayek 1932a:219).

This argument of Hayek's is quite overlooked in the literature, but, in spite of the confused way in which it is carried out, is remarkable. In an attempt at clarification, Hayek is looking for an example of rates divergence which contains the seeds of the reversal of capital accumulation with money as one of the commodities; but he ends up providing a demonstration which unfolds entirely in real terms. Firstly, he refers to a statement of his as early as 1929, taken from the German version of the book later translated into English as *Monetary Theory and the Trade Cycle,* that large fluctuations in the rate of savings may have effects similar to those of changes in the quantity of money' (Hayek 1932a:219n.). Secondly, he deals with the 'particular case', and observes that in a barter economy:

> if the actual supply of wheat were not changed, but if, under the mistaken impression that the supply of wheat would greatly increase, wheat dealers sold *short* greater quantities of future wheat than they will actually be able to supply…anything corresponding to the deviation of the money rate from the equilibrium rate could possibly occur. And if we assume that, in the community where this happens, wheat is the most important consumption good, then the consequences might be similar to those which occur when the money rate is below the equilibrium rate. The relatively low price at which (for example, in terms of machines) consumers' goods are offered for the immediate future will, in this case, make it worthwhile to secure sufficient supplies of them to start longer processes of production. But a time must come when the error is noticed, prices of consumers' goods rise, and it becomes obvious that it is not possible to wait as long as had at first seemed practicable for the product of the investment.'
>
> (Hayek 1932a:219–20)

Hayek (1932a:220) characterises this 'sketchy outline' as an exception to the general case represented by his own theory. In fact, 'if we…assume that it is not the promise of a particular kind of consumers' good, but the claim on present goods in general which is offered in exchange for promises of future goods in excess of present goods available for that purpose, then we have the

case of an increase of money by means of additional loans for investment purposes', and the analytical framework is therefore validated.

In a short rejoinder, Sraffa limits himself to a re-assertion of the weakness of Hayek's reasoning on two of the themes under discussion. As regards the one which has come second in my treatment, forced saving, Sraffa remarks that, since remuneration of the factors required for the production of new capital goods will rise in a ratio proportional to the quantity of money, and since these payments are made in instalments, stage of production after stage (a fact which accounts for Hayek's 'lag' in income re-adjustment), 'the whole of additional money is absorbed in cash holdings for performing such payments' (Sraffa 1932b:225). It is therefore reasonable to believe that investors may withhold the corresponding sums, and, arguably, that they will actually do so when a recession is looming, since they would be able to trigger a profitable redistribution of income.[10] Sraffa's comment on the third theme is laconic: he simply says that monetary policy rules like those Hayek recommends are now devoid of any significance on the grounds that 'his maxim of policy now requires that the money rate should be equal to all these divergent natural rates' (Sraffa 1932b:226)

To provide a concise overview of the discussion between Hayek and Sraffa, I would argue that what was really at stake was a twofold methodological problem: first, what are the characteristics of a monetary economy (that is, what are the functions of money); and, second, how does a real economy work (that is, what is implied by a divergence between the own rates)? As to the question of forced savings, on the other hand, it was too closely linked to the specific hypotheses of Hayek's model to fuel a real discussion. There was no chance of conciliatory views here, and, interestingly enough, this holds true with respect to later commentators as well (see, e.g., the diverging interpretations of Lachmann 1986 and Lawlor and Horn 1992). As illustrated above, Hayek assumes that the extra money is borrowed with the single purpose of investment in capital goods; this is why he does not understand Sraffa's objection, based on the possibility of alternative uses. Hayek had adopted the hypothesis of anticipated remuneration of factors (relative to production), a hypothesis entailing the impossibility for the investors to withhold the additional money, as Sraffa suggested.[11]

As regards the first half of my 'methodological' problem, Hayek agrees on the functions of money put forward by Sraffa, as will be shown in more detail in the next section. However, Hayek adds that, granted that he ignored important functions of money in his business cycle model, this was due to the necessity of adopting radically simplified assumptions to tackle a challenging and multi-sided problem. At all events, I think that the truly fundamental question, which is particularly rewarding to deal with from my angle, is the second half of the problem, concerning the features of a real economy.

It is established among interpreters that the contradiction between different own rates of interest of commodities, either non-equilibrium as Sraffa maintained or equilibrium as Hayek did, may be traced back to the contrasting views of equilibrium the two authors held. In fact, Sraffa kept faith to a classical and stationary concept of equilibrium, whereas Hayek had steered himself towards a neoclassical and intertemporal notion (McCloughry 1982). Yet, whereas Sraffa's viewpoint has appeared limpid to contemporaries and later interpreters alike, Hayek's perspective seems quite nebulous. *Prices and Production* depicts a real economy shifting from an initial to a final state of long-period equilibrium, but it is not immediately understandable–nor was it to contemporary observers, in particular–how this can match a multiplicity of equilibrium rates. The puzzle is solved, however, once it is assumed that Hayek had the concept of intertemporal equilibrium in mind when taking differing rates as equilibrium rates. This reading comes to be substantiated if an essay published in German in 1928 and translated into English as 'Intertemporal price adjustments and the value of money' (in Hayek 1984) is taken into account, since this essay has been hailed as the first statement of intertemporal equilibrium in economic theory (as argued in Milgate 1979). Once the intertemporal perspective is taken as Hayek's background, a permanent divergence between spot and forward market prices becomes possible, contrary to Sraffa's argument, and the disparities in the own rates of various commodities would be compensated (or explained) by the disparities in future prices. On the other hand, Hayek does not mention this article in his reply to Sraffa, although the article figures in the first edition of *Prices and Production* as a reference for the development of equilibrium analysis towards an intertemporal set up. In effect, the article does not hint at the possibility pointed out by Sraffa. At all events, it has been easy for Hayek to include this possibility in the model once his attention has been drawn to it.[12]

It is nevertheless unquestionable that the problem of a tendency towards the elimination of the discrepancy among own rates was Hayek's regular concern, unlike the theory of intertemporal equilibrium as commonly intended, that is in its short-term equilibrium version derived from the Arrow-Debreu model. At bottom, the model of *Prices and Production* is tailored for a real economy moving from an initial state of long-term equilibrium to a final state with the same characteristic; and thereby Hayek reveals that he is far from a thorough awareness of all the implications of the notion of equilibrium he introduced in 1928 and then, but only implicitly, assumed in his answer to Sraffa.

The next section, however, deals with neither the model of real economy Hayek actually put to use in the 1930s nor the possible changes this model underwent, either as a result of Sraffa's critique or as an internal development of the 1928 position. Rather, I will focus specifically on Hayek's attitude towards the possibility that cyclical disequilibrium may be induced by causes

other than monetary ones–a possibility which, it is pertinent to stress, Hayek explicitly rejected at the beginning of the 1930s and represents the core of his work as a theoretical economist.[13]

Real and monetary economy in Hayek's theory

Between the first and the second edition of *Prices and Production,* Hayek unrelentingly refined his business cycle theory through answers to reviewers, translations into English of previous writings, and original essays on circumscribed aspects of the theory. As a result, he referred to ten works of this kind in the preface to the second edition alone. Yet, as already mentioned, in this preface it is said that the scope of the intended revisions concerning real economy issues is so wide that, rather than re-working *Prices and Production,* he prefers initiating a new book designed 'considerably to elaborate the foundations on which I have tried to build' (Hayek 1935a:viii). The book in question is evidently *The Pure Theory of Capital.* However, it should be emphasised that Hayek's confidence in the validity of the main line of argument of *Prices and Production* was not shaken: in 1939, he collected some earlier essays to demonstrate that the relaxation of most of the simplifying hypotheses at the basis of *Prices and Production* did leave the main conclusion untouched. In particular, the essay from which the book title is taken, 'Profits, interest and investment', was intended to show that the dropping of that crucial hypothesis, the full employment of resources, did not entail the abandonment of the theory–basically, because of the complementarities in the various components of the capital structure (Colonna 1994).

In this section of the chapter, after a short consideration of the revisions which may be directly ascribed to Sraffa's critique, I will deal with some crucial passages drawn from writings subsequent to the exchange with Sraffa which support my view that Hayek progressively changed his mind about the characteristics of a real economy which really have an impact on the dynamics of economic systems. To put it another way, I would argue that textual evidence supports the thesis of a lasting influence of Sraffa's ideas on Hayek, since the relevant passages reveal close affinities with that already quoted concerning the own rates. However, I must admit that no explicit reference to Sraffa is made in any of the places I have considered. It is plausible, then, to conjecture that Hayek had other critics' remarks in mind, either in addition to or in place of Sraffa's, even if, again, there is no acknowledgement to substantiate this claim either. Briefly, I am saying that one might view the following as just the story of the evolution of Hayek's thought, since it is impossible to bring incontrovertible evidence of Sraffa's influence. I will tackle this difficulty in the next section.

From the collation of the two editions of *Prices and Production,* there emerge some marginal alterations and limited concessions which may remind us of Sraffa's critique, especially in relation to the possibility for the producers to

withhold part of the additional money and hence originate real savings. In this respect, the point forcefully raised by Sraffa is the eventuality that a redistribution of income in favour of producers could occur during the upward phase of the cycle. In answering Sraffa in 1932, Hayek made non-essential concessions on this: 'to the extent that entrepreneurs may not consume part of the extra profit made during that period [when new money becomes available for investment]', it is possible that 'the shift of incomes from a class less inclined to save to a class more so inclined will ultimately have produced some real savings' (Hayek 1932a: 215). In a later text, Hayek (1935b:147) re-examines the problem, and, apropos of possible variations in the amount spent on current consumption goods in proportion with variations in factor income, he observes that 'only in so far as redistribution of income has taken place during the whole process, favouring those more inclined to save at the expense of those less inclined to save, a certain increase of the proportion of the income actually saved may be expected'. It is very likely that Sraffa's remark struck Hayek, for a similar point is also made in *The Pure Theory of Capital* (Hayek 1941:35, f. 2). But in no place it is hinted that a rise in savings so induced might compensate for the fundamental shortage of savings bringing about capital consumption.

On the contrary, another of Hayek's clarifications, regarding the possibility of defining a rule for monetary policy as simple as that advocated in 1931, is so substantial that it easily turns into a piece of (undeclared) self-criticism. I have documented above how Sraffa's accusation of ignoring the role of money was taken to heart by Hayek–nonetheless Sraffa was obviously right. In the second English edition of *Prices and Production,* Hayek includes an appendix to Lecture IV entitled 'Some supplementary remarks on "neutral" money', which consists for the most part of a short article which appeared in German in 1933 (1933b). Here Hayek stresses that the concept of money neutrality is 'destined in the first instance to provide an instrument for theoretical analysis'; moreover, the solution of the practical problem of monetary governance is complicated by the fact that 'the existence of a generally used medium of exchange will always lead to the existence of long term contracts in terms of this medium of exchange, which will have been concluded in the expectation of a certain future price'; furthermore, it should 'be necessary to take into account the fact that many other prices possess a considerable degree of rigidity'. In the light of these considerations, 'the elimination of the active influences of money has ceased to be the only, or even a fully realisable, purpose of monetary policy' (Hayek 1935a:131).[14]

What I have just recounted shows how far the direct confrontation with Sraffa went. However, the changes Hayek introduced in the mid-1930s were much more momentous, resting on his constant attempt to redefine his model of a real economy.[15] Interestingly, in his reply to Sraffa, Hayek (1932a:211) gave vent to his doubts about the Italian's real theory and spoke of 'an

extreme theoretical nihilism which denies that existing theories of equilibrium provide any useful description of the non-monetary force at work'. Paradoxically enough, from then on the bulk of Hayek's effort, both in the reply to the review and afterwards, was designed to alter the 'existing theories of equilibrium'.

Additionally, it is well known that Hayek's contribution to economic theory over the course of the 1930s was outstanding, and not just for its quantity, however remarkable, but for the wide range of topics considered. Together with celebrated writings on business cycle theory and the impressive set of related refinement pieces just mentioned, there were contributions now hailed as classics on a variety of themes, ranging from the efficiency of centralised systems, capital theory, and equilibrium theory to the philosophy of economics and methodology–not to mention his study of the cognitive structure of human mind dating back to the 1920s. A problem facing critics is that some of these contributions mark, or seem to mark, breaks and innovations in the evolution of Hayek's thought.

If I had to fall back on a single intervention epitomising Hayek's work in economic theory in the 1930s, I would certainly choose 'Economics and knowledge' of 1937. Here Hayek on the one hand delineates his almost conclusive standpoint on equilibrium, regarded as a situation in which the intertemporal plans of individual agents are coordinated, and on the other opens up the analysis of 'the actual acquisition' of the necessary knowledge for equilibrium to hold, that is of those aspects of economic theory which in his view have empirical substance (unlike equilibrium theory). Since it is arguable that all of his successive writings deal, in various ways, with knowledge dispersion, this essay has been often seen as an acid test of Hayek's change of attitude towards economic theory (Caldwell 1988).[16]

The following passage is an excerpt from the part of the essay addressing the 'pure logic of choice':

> Before I proceed further I should probably stop for a moment to illustrate by a concrete example what I have just said about the meaning of a state of equilibrium and how it can be disturbed. Consider the preparations which will be going on at any moment for the production of houses. Brickmakers, plumbers, and others will all be producing materials which in each case will correspond to a certain quantity of houses for which just this quantity of the particular material will be required. Similarly we may conceive of prospective buyers as accumulating savings which will enable them at certain dates to buy a certain number of houses. If all these activities represent preparations for the production (and acquisition) of the same amount of houses, we can say that there is equilibrium between them in the sense that all people engaged in them may find that they can carry out their plans. Another example of more general importance would, of course, be the correspondence between 'investment' and 'saving' in the

> sense of the proportion (in terms of relative cost) in which entrepreneurs provide producers' goods and consumers' goods for a particular date, and the proportion in which consumers will at this date distribute their resources between producers' goods and consumers' goods…[17] This need not be so, because other circumstances which are not part of their plan of action may turn out to be different from what they expected. Part of the materials may be destroyed by an accident, weather conditions may make building impossible, or an invention may alter the proportions in which the different factors are wanted. This is what we call a change in the (external) data, which disturbs the equilibrium which has existed. But if the different plans were from the beginning incompatible, it is inevitable, whatever happens, that somebody's plans will be upset and have to be altered and that in consequence the whole complex of actions over the period will not show those characteristics which apply if all the actions of each individual can be understood as part of a single individual plan, which he has made at the beginning.
>
> (Hayek 1937:42–3)

This is the single place in 'Economics and knowledge' where Hayek considers the dynamic features of economic systems constituting the subject matter of his business cycle theory. The argument develops in a way which is analogous to that which Hayek adopted in the example made to answer Sraffa's third question, and quoted in the previous section. In this respect, it is worth observing that in neither 'Economics and knowledge' nor in the reply to Sraffa does Hayek mention monetary shocks as causes of discrepancies in the intertemporal actions carried out by individuals. What is more, in the essay in question investment and saving are introduced as mere examples, albeit 'of more general importance', of a general phenomenon of possible mutual inconsistency of intertemporal decisions by individual agents. Furthermore, Hayek distinguishes between variations in the objective data which can induce modifications of intertemporal plans, and variations resulting from the implementation of programmed plans; where the latter variations are, seemingly, endogenous rather than exogenous but whose disequilibrating effects are, in principle, identical to the former type. Within the subjectivist framework which Hayek has come to adopt, it is apparent that there is no guarantee for the plans to succeed: even apart from the immutability, or not, of objective data, Hayek stresses that it remains to be explained why individual plans based on a subjective reading of the available data should be compatible.[18]

From my angle, the last step in the evolution of Hayek's thought is *The Pure Theory of Capital,* and is undoubtedly a momentous one. Curiously perhaps, this book is said by its author to be largely incomplete, while at the same time Hayek presents it as the most perfected stage of research on a real economy which he managed to reach. In this work Hayek seeks, first, to redefine the concept of capital against the background of intertemporal

equilibrium, under the assumption that agents have the ability to correctly anticipate future events, and, second, mentions a series of consequences stemming from the abandonment of the perfect foresight assumption. But Hayek also asserts that these tasks should be viewed as preliminaries to a fully-fledged 'causal explanation of the process in time', which he considers 'the ultimate goal of economic analysis'; he makes it clear that 'equilibrium analysis is significant only in so far as it is preparatory to this main task' (Hayek 1941:17).[19] As often remarked, the fact that Hayek did not go beyond a declaration of intent, failing to deliver what he himself judged of paramount importance, reveals his limitations as a theoretical economist (though, it is worth stating, a convincing solution to this problem of Hayek is still to come). However, even limiting ourselves to the relevance of the equilibrium analysis put forward,[20] one is struck by the different context in which his usual account of the reversibility of actions caused by monetary shocks is set. Of course, Hayek continues to point to the reversible effects of expansionary policies, but, in the model outlined in the book, money-induced discrepancies are presented with more clarity than ever as special cases of the incompatibility of intertemporal plans.

The following passage is drawn from Chapter 25, which, as its title evidences ('Savings', 'Investment', and the 'Consumption of Capital'), is a compendium of all the most intricate consequences of the equilibrium theory of capital as developed in Part I, II, and III of the book, which Hayek (1941:334) thinks must be briefly commented upon, 'although they fall for the most part outside the scope of pure equilibrium analysis'.[21] In a section devoted to a 'Comparison between shape of income streams provided and demanded', Hayek considers savers' behaviour as follows:

> If the values consumers attach to the sources of future income (in terms of present income) is higher than the cost (in terms of present income) of reproducing new sources of future income of the same magnitude, more such sources will be produced and *vice versa*. And assuming that the relative valuations of the consumers do not change abruptly–as they are unlikely to do if their income that becomes available in each successive period is equal to the income and the sources of future income which they have planned–the amounts of present income and sources of future income which production will provide in each successive period will tend to be such that their relative costs (in terms of each other) will approximately correspond to the relative values attached to them by the consumers. But if, for some reason, the prices of the sources of future income have been raised out of correspondence with the valuations of the consumers, the result will be that more sources of future income will be provided for the next period than consumers will then be willing to take at prices corresponding to their relative costs. Consumers will find themselves getting less current real income, and consequently will attach

> greater value to it compared with the sources of future income. In spite of the special senses recently attached to the idea of differences of saving and investment is difficult not to describe this case as one in which saving exceeds investment (or *vice versa*). And we shall indeed see later that the special cases to which these terms have recently been applied are only particular instances of the *general case* we are considering. They differ from the general case only through the cause which brings about the difference between saving and investment, which in the *special case* is a monetary cause. But the effects are the same and they are in turn instances of an even more general case, that of demand exceeding or falling short of supply…. The case is simply one where, because of wrong expectations on the part of producers, the supply of certain types of commodities will exceed, and the supply of other kinds of commodities will fall short of, demand. And the changes of prices relative to cost will be exactly of the kind which is necessary to bring about the appropriate changes of production. We shall see later why monetary changes are particularly apt to cause this sort of wrong expectations.
>
> (Hayek 1941:338–40; my italics)

Hayek examines in more detail the case of disequilibrium in intertemporal actions which he deems most worrying, and labels it 'savings falling short of expectations–may mean an actual consumption of capital'. Again, Hayek (1941:343) declares his intention to investigate how this happens in relation to 'monetary complications', and to delineate the special features of such a case; but the possibility of capital consumption remains associated first and foremost with an entirely real case.

To conclude this section, I would add a strictly philological facet to my reconstruction. Most of Chapter 25 of *The Pure Theory of Capital* amounts to an enlargement of a paragraph of the 1935 essay 'The maintenance of capital'. In particular, the sentence I quoted at the beginning of the chapter regarding a possible discrepancy between the decisions of consumers and those of entrepreneurs is extracted from this essay (Hayek 1935c:124), although the context of the sentence differs slightly in the two texts. Furthermore, the passage I have just quoted is very much alike that in this essay (1935c:126). But what is remarkable is that the third paragraph in the 1935 text includes a parenthetical sentence which is dropped in 1941 (and which I italicise): 'But if for some reason–*say because additional money has become available for investment purposes*–the prices of the sources…' It is also remarkable that the part of the 1941 text defining the hierarchy between general and special cases does not figure in the 1935 essay, where the treatment ends with the fifth paragraph.

The gist of this collation is to argue, first, that Hayek's doubts about the theory of capital used to assess the business cycle were already an object of inquiry at the time of the publication of the second English edition of *Prices and Production,* rather than being a fresh intuition as alleged in its

introduction. Second, it becomes clear that the full awareness of the different role of money which is to be found in the 1941 book emerged at the end of a process of revision of the real theory underlying business cycle theory–this granted, my contention is that the process started with Hayek's reply to Sraffa.[22]

The evolution of Hayek's theory in the 1930s and early 1940s

Within the landscape of business cycle theory in the 1930s, Hayek's theory stands out for its attempt to explain cycles within the theoretical framework of general equilibrium. This approach is put forward mainly in *Monetary Theory and the Trade Cycle*. Here it is claimed that the business cycle is to be analysed within the 'modern theory of the general interdependence of all economic quantities' which he sees, at least until the debate with Sraffa, as 'most perfectly expressed by the Lausanne School of theoretical economies' (Hayek 1933a:42n.). Indeed, in *Monetary Theory and the Trade Cycle,* Hayek is clear in distinguishing between the disequilibrium movements of the endogenous variables associated with exogenous shocks of any type and cyclical disequilibrium. In the first instance, following 'the logic of equilibrium theory', it must be assumed that any variation in economic data induces changes in the endogenous variables towards a new equilibrium by means of prices, which provide 'an automatic mechanism for equilibrating supply and demand' (Hayek 1933a:43). This process of adjustment towards equilibrium is definitely not cyclical disequilibrium. The Hayekian trade cycle, on the other hand, is a kind of disequilibrium in which intertemporal plans are started which must be inevitably frustrated, that is will turn out not to be accomplishable from a certain future date on. To be more specific, investment in new capital goods due to the discovering of new technological knowledge gives birth to a phase of disequilibrium, but cyclical disequilibrium entails investment in new capital goods which, for some reason, will be 'consumed' in a later phase. Whatever the starting cause of new investments, the business cycle is precisely defined as a situation in which individual agents undertake intertemporal actions which cannot be executed because of 'incorrect expectations' based on 'wrong prices' (Hayek 1933a:85, 65); and the 'wrong' price *par excellence* is a monetary interest rate which, for some reason, does not match the real interest rate.[23]

As early as 1929 one thus finds that Hayek admits that the discrepancy between monetary and natural (or equilibrium) interest rates creating new investments may be traced to a real variation, such as an improved profitability of investments (Hayek 1933a, Ch. 4). But the point at issue, the point raised by Sraffa, is whether the absence of a banking system unable to adjust the monetary rate in the wake of the changes occurred to the natural one makes it possible to speak of investment projects carried out on the basis of wrong expectations. Hayek (1933a:141–7) does not consider this

possibility. He admits that a cycle may be sparked off by real causes, but only in relation to the behaviour of the banking system–a behaviour which he regards as typical. All other accounts are flawed, he contends, by reason of their non-endogenous characterisation. For example, Hayek (1933a:205) examines the possibility of severe oscillations of the propensity to save, and acknowledges that these may entail a cyclical course of the endogenous variables; but in his opinion this case is of little theoretical interest. In fact, under the circumstances the cycle would not be originated by a structural mechanism of propagation of external impulses–such as the behaviour of the banking system–but by erratic shocks, and hence would not require further analysis outside what is known from 'existing theories of equilibrium'.

Underlying this perspective, there is a view of money as the factor capable of upsetting the routine of the economy. The necessary alternation of booms and slumps can only be accounted for through the existence of the banking system. At bottom, this is why interventionist monetary policies are counter-productive, unlike neutral ones. The role of Hayek as a 'political economist' cannot be established regardless of this stance. However, the constant reformulation of the foundations on which his business cycle theory is built testifies to the fact that, since the early 1930s, he deemed the model justifying his policy views unsatisfactory.[24]

The previous sections have made clear the extent of Hayek's uneasiness with the real theory which in 1929 and in the immediately following years he had regarded as definitely achieved. The deeper he went into the real structure of an economic system, the more his prefaces abounded with promises of revision; the more he dealt with the notion of general equilibrium, the more he realised that this must be radically reformulated to be used as the cornerstone of business cycle theory. Hence the evolution that first led him to revise the treatment of the changes in the productive system linked to cyclical phenomena put forward in *Prices and Production,* and then to operate a complete theoretical reconstruction in *The Pure Theory of Capital.* His incessant taking up the concept of equilibrium also originated here, and culminated in the definition of equilibrium as mutual consistency of intertemporal plans in 'Economics and knowledge'.

According to the previous section, the evolution of Hayek's theory is connected with a reworking of the model of real economy as a stepping stone to the analysis of the effects of variations in the amount of available credit. For this reason, it is especially in *The Pure Theory of Capital* that one expects to find Hayek's 'definitive' account of the functioning of a real economy. As hinted at in the previous section, Hayek introduces this book as an investigation 'confined to that part of the subject which belongs to equilibrium analysis proper'. But, although Hayek aims at renewing Böhm-Bawerk's theory of capital, he ends up with a theory which appears closer to the Walrasian tradition of general equilibrium with capital accumulation than to the Austrian tradition.[25] It is well-known that models in the general

equilibrium tradition have so far failed to provide a convincing account of the adjusting process.

Thus, Hayek comes to pursue two goals which, after *The Pure Theory of Capital,* appear contradictory. On the one hand, he wants to reformulate equilibrium theory in terms of a temporal horizon making a satisfactory theoretical analysis of the revision of unfulfilled intertemporal plans possible. On the other hand, he wants to devise a causal model of the evolution of the economic system over time. To carry out the second goal–which is the most important of the two according to both 'Economics and knowledge' and *The Pure Theory of Capital*–he makes a choice which alienates him from pure theory. In fact, Hayek famously ignored the conceptual frameworks that, put forward in the 1930s, have marked the course of economic dynamics since then. In particular, Hayek did not endorse either Hicks' idea of a series of states of temporary equilibria or the concept of pure disequilibrium associated with Lindahl. Rather, Hayek's peculiar route was the analysis of competition as process for the discovery of new knowledge; but this approach entailed the definitive abandonment of pure economic theory.[26]

A final question remains to be dealt with in this section: Hayek's vision as a political economist. Here, one wonders how Hayek managed to assert again and again that the business cycle was inextricably linked to the use of money in real economic systems. In other words, the question amounts to understanding how come Hayek failed to realise the self-contradictory nature of his position after *The Pure Theory of Capital*. This apparent contradiction can be explained by looking at the already mentioned essay on 'Price expectations, monetary disturbances and malinvestments', where the rationale of Hayek's argument is clearer than in Part IV of the 1941 volume–which though intended to show how money would disrupt equilibrium is mainly a comment on Keynes' *General Theory*. In the 1935 article Hayek clarifies his thought as follows:

> Every explanation of economic crises must include the assumption that entrepreneurs have committed errors. But the mere fact that entrepreneurs do make errors can hardly be regarded as a sufficient explanation of crises. Erroneous dispositions which lead to losses all round will appear probable only if we can show why entrepreneurs should all simultaneously make mistakes in the same direction. The explanation that this is just due to a kind of psychological infection or that for any other reason most entrepreneurs should commit the same avoidable errors does not carry much conviction. It seems, however, more likely that they may all be equally misled by following guides or symptoms which as a rule prove reliable. Or, speaking more concretely, it may be that the prices existing when they made their decisions and on which they had to base their views about the future have created expectations which must necessarily be disappointed.
>
> (Hayek 1935b:141)

Thus, any cause which entails entrepreneurs' systematic errors 'in the same direction' is, in principle, likely to cause the cycle. Yet even in the passage just cited, Hayek carefully distinguishes between his monetary perspective and that of the advocates of real business cycle theories: in fact, he does not credit either psychological factors or error-inducing external shocks with the potential for inducing joint error by entrepreneurs.[27] That is to say that at the time of writing 'Price expectations, monetary disturbances and malinvestments' (which was originally given in form of lecture in 1933) money was still an indispensable, necessary element for the cycle to occur. But this privileged status was renounced as, first, the real structure of the economy as delineated in his new theory of capital, and, second, the concept of equilibrium as intertemporal equilibrium became Hayek's frame of reference. Hence, again, Hayek (1941, Part IV) proceeded to contend that money was a (sufficient, but not necessary) cause of the business cycle. However, it was the systematic character of the effects resulting from monetary shocks that allowed him to continue claiming the validity of the core message established in his business cycle theory of the early 1930s, in spite of the developments I have chronicled, and which seem to invalidate it.

Concluding remarks

This chapter has argued that the evolution of Hayek's thought in the 1930s might have been influenced by Sraffa's review of *Prices and Production* to a much larger extent than usually recognised in the literature on the debate between the two authors. In his last attempt to give a satisfying formal content to the theoretical basis of his theory of the business cycle, which is to be found in *The Pure Theory of Capital,* Hayek comes to the conclusion that the causes of the intertemporal discrepancies between consumption and investment are mainly real causes. Though money is still considered the most important element to cause planned actions to be revised, as typically occurs in a Hayekian cycle, it is no longer a necessary condition for the cycle to take place, but only one of the possible sufficient conditions. Indeed Hayek shows clear awareness that in an intertemporal context even unanticipated contingency can cause, in principle, phenomena of consumption of capital. It is in this sense that the monetary cause is now only a special case, thought, as we have seen in the previous section, 'the' special case, of a more complete theory of the reversibility of planned actions in an intertemporal context when expectations turn out to be incorrect.

Hayek, on the other hand, continues to attribute to the banking system the crucial role in generating the cycle. His own vision of the workings of the economy and the role of monetary intervention, and his fierce, long-standing opposition to Keynesian 'inflationism', does not change after *The Pure Theory of Capital*. Hayek's point is that only credit by banks to entrepreneurs can induce incorrect expectations of the same manner in the investment sector of

the economy. Real causes can not. However, this is no longer a definitive result of pure economic theory, such as Hayek presents it in *Monetary Theory and the Trade Cycle* and *Prices and Production*. Once it is admitted that expectations can be falsified because of unanticipated real events, it turns out that a Hayekian cycle can be induced both by monetary and real causes. To take an example which is largely discussed in modern macroeconomics, a common modification of individual expectations which is not related to monetary causes can be found in sunspots models, where the intertemporal discoordination of individual actions is a main outcome.[28]

This chapter has argued that Hayek's recognition that his own model of a real economy in *Prices and Production* is not correctly defined can be attributed to Sraffa's influence. This holds notwithstanding an accurate consideration of the 1928 article, which introduces the notion inter-temporal equilibrium later developed in *The Pure Theory of Capital,* and of the 1929 volume, which shows a much more detailed consideration of the possible, alternative causes of the business cycle than can be found in *Prices and Production*. Therefore the explicit admission by Hayek in the 1941 volume that money can be considered in principle inessential as regards uncoordinated actions in an intertemporal context can be seen as the outgrowth of Hayek's reaction to a point first raised by Sraffa in his 1932 review. This is why I have tried to show that Sraffa's review of *Prices and Production* can be considered as a main cause of the evolution of Hayek's thought.

Notes

1 Jack Birner, Marina Colonna and Nicolò De Vecchi made useful comments on an earlier draft of the chapter. Though they showed interest in the analysis of the evolution of Hayek's thought I provide, none of them completely agreed with my insistence on Sraffa's role. As a result something more than the usual disclaimers apply. Financial assistance from MURST is gratefully acknowledged.

2 A classic reference is Hicks (1967). For a recent assessment see Caldwell (1995).

3 The second English edition of *Prices and Production* (Hayek 1935a) is substantially the same as the original one of 1931. As admitted in the preface, Hayek himself was unhappy with the lack of changes. At any rate, the reader cannot but be perplexed to find in the preface the statement that important alterations to the analytical framework put to use in the book would be considered in the near future. For the moment, Hayek simply refers the reader to his replies to the many critics of the first edition of *Prices and Production,* as well as to the 1933 English version of his 1929 volume on the business cycle, translated as *Monetary Theory and the Trade Cycle*.

4 No reference to Sraffa can be found in the autobiography *Hayek on Hayek* (Hayek 1994) either, though there are plenty of references to other 1930s prominent scholars and to the intellectual atmosphere in general. However, as we shall see, *The Pure Theory of Capital* contains a number of statements which can be read as comments on Sraffa's points.

5 My summary of the Hayek-Sraffa debate is necessarily selective. A more detailed treatment can be found in Lachmann (1986) and Lawlor and Horn (1992). A

suggestive reconstruction of the intellectual environment in which this episode is included can be found in Caldwell (1995).

6 Hayek (1935a:37–38) does not deal with changes in technological knowledge. He is clear in stating that he is interested in 'the increase in output made possible by a transition to more capitalistic methods' given a certain technological knowledge, that is 'by organising production so that, at any moment, the available resources are employed for the satisfaction of the needs of a more distant future than before.... The *raison d'être* of this way of organising production is, of course, that by lengthening the production process we are able to obtain a greater quantity of consumers' goods out of a given quantity of original means of production'.

7 As is well known, it is Keynes (1936, Ch. 17) who defined as own rate of interest the natural rate of any commodity. On the diverging views on use of the notion of own rates between Sraffa and Keynes, see Mongiovi (1990).

8 It might be worth noting that this brief reference to disequilibrium adjustments is more than anything one can find in Sraffa (1960a) on this topic.

9 That lags in the adjustments of factors income are crucial for Hayek's argument is the main point highlighted in Hicks (1967). Moreover, complementarities among capital goods used in different stages of production constitute another important source of friction.

10 It is worth noting that Sraffa's argument contains also a mechanism of adjustment to the new equilibrium, since the redistribution of income favours a class with a higher propensity to save and overall savings would therefore increase as required to match increased investments.

11 This point is convincingly argued in Graziani (1998).

12 See for instance Hayek (1941:166–9), which is difficult to better as a synthesis of the modern intertemporal equilibrium approach such as represented, for instance in Bliss (1975, Ch. 3). Among the implicit references to Sraffa which one can find in *The Pure Theory of Capital* there must be the point which analyses the divergences among own rates in a non-stationary context which closes as follows: 'To distinguish, in any particular case, between the part (of the rate of increase of the product) which is due to circumstances affecting the value of the particular commodity and the part which is due to the productivity of investment is just as impossible as to divide the change in the relative value of two commodities into the part which is due to a change in the value of the one, and the part which is due to the change in the value of the other. Although the search for this philosopher's stone is probably still pursued by some economists, nothing more need be said about it here' (Hayek 1941:168–9). This is still today a main point on which the views of the neo-Ricardian scholars diverge from those inspired by the Walrasian model. See for instance the exchange between Garegnani (1997) and Tosato (1997).

13 Hayek's viewpoint concerning the relationship between stationary and intertemporal equilibrium is made explicit only in Hayek (1941) (see footnote 19 below). The main question is that Hayek's long-run equilibrium requires that, though relative prices can change as in an intertemporal equilibrium, the capital stock given at a certain date is structured so as to guarantee the matching of demand for and supply of consumption and investment goods at each future date (Graziani 1998).

14 Two points are worth noting. First Hayek closes this analysis by correctly identifying the theoretical assumptions for monetary neutrality: perfect foresight and price flexibility. It is in this sense that the Hayek-Sraffa debate anticipates the debate on the role of money in the Arrow-Debreu general equilibrium model, as contended in Desai (1982). Second, as noted by De Vecchi in his comment (in this volume) the more restrictive are the conditions for monetary neutrality to

hold, the less plausible is the intervention for a monetary aim to be pursued. The rationale for monetary intervention by the government in general is thus implicitly denied, in agreement with Hayek's fierce opposition to the incoming Keynesian revolution. Surprisingly enough Hayek (1941:30 f.) comes to the conclusion that he 'must plead guilty of some responsibility…for the incautious way in which attempts have occasionally been made to use it [the concept of "neutral money"] as a practical ideal of monetary policy'.

15 Certain light revisions of the 1929 German original introduced in the 1933 English translation of *Monetary Theory and the Trade Cycle* are interesting in this regard. For instance Hayek notes that 'Since the publication of the German edition of this book, I have become less convinced that the difference between monetary and non monetary explanations is *the most important* point of disagreement between the various Trade Cycle theories…it seems to me that the differences between these explanations (the so-called non-monetary theories), which seek the cause of the crisis in the scarcity of capital, and the so-called under-consumption theories, is theoretically as well as practically of much more far-reaching importance than the difference between monetary and non-monetary theories' (1933a:41 f.). Hayek identifies his theory as a monetary theory, but seems to think that the main difference with other theories ultimately concerns the underlying description of a real economy. In any case, it must be recalled that Hayek was involved in a harsh discussion with German supporters of non-monetary theory of the business cycle such as Löwe (see Hagemann 1994).

16 For a different assessment, see O'Driscoll (1977) and Moss (1994).

17 In the original this last sentence is a footnote to the previous sentence. The footnote refers to the 1933 Copenhagen lecture, first published in 1935 and translated in English in 1939 as 'Price expectations, monetary disturbances and malinvestment', which clarifies the role of expectations in Hayek's business cycle model, and to 'The maintenance of capital', which anticipates Hayek's discussion of what is to be understood for constant capital in an intertemporal context given in *The Pure Theory of Capital*.

18 Hayek's reliance in the ability of 'existing theories of equilibrium' to assure that 'the individual sets of subjective data correspond to the objective data' by means of a fast, automatic adjustment process is thus lost. 'Economics and knowledge' points towards a market process analysis of the competitive mechanism which characterises most of his later works (see in particular Hayek 1948 and 1968). On Hayek's notion of equilibrium as mutual consistency of intertemporal plans, see Zappia (1996).

19 Eventually, Hayek clarifies what the relationships are that, in his view, hold between the notions of intertemporal and stationary equilibrium, with specific regard to a correct treatment of capital: 'Stationary equilibrium presupposes the existence of equilibrium relations between the existing things, that is it assumes that the existing goods are of exactly the same kind as those which under existing conditions it will be profitable to reproduce…. The equilibrium in which we are interested here is not an equilibrium which is embodied in the things, but an equilibrium between different activities of creating new goods, as determined by the goods which happen to exist at the outset. This concept is in fact no less realistic than that of stationary equilibrium: since in order to arrive at a stationary equilibrium it would be necessary to pass through a phase in which the changes required to bring about a stationary state were still going on but their result were correctly foreseen' (Hayek 1941:16n.).

20 Though Hayek's volume is characterised by the eventual failure to provide a significant re-assessment of capital theory there are plenty of perceptive insights

in it (Shackle 1981). For instance, De Vecchi (1982) and Steedman (1994) emphasise the relevance of Hayek's denial of any usefulness of the notion of capital as a monetary fund.

21 Hayek (1941:334–5) elucidates from the outset that 'if unforeseen changes in the data occur, the value of the stock of capital that exists and will have to be maintained if income is to be kept constant from now onwards will also change, and that consequently there is no reason to expect that in a dynamic world any of the conceivable dimensions of capital will remain constant'; moreover 'in a world of imperfect foresight, not only the size of the capital stock, but also the income derived from it, will inevitably be subject to unintended and unpredictable changes which depend on the extent and the distribution of foresight, and there will be no possibility of distinguishing any particular movements of these magnitudes as normal'. As a result, Hayek maintains, even the concepts of investment and saving are obscure.

22 Incidentally, it might be add that the following excerpt is not only reminiscent of Sraffa's argument (as noted in Lawlor and Horn 1992:320), but also an implicit endorsement of it, since it applies to Hayek's own use of the notion of natural rate in *Prices and Production:* 'Much confusion has been caused in this connection by the assumption sometimes made that there could be a real capital market without money on which there would be some determinate *in natura* rate of interest. In fact there would not and could not be one rate of interest without money, and the effect of the limitation placed on the possible amount of waiting by the scarcity of the stock of non-permanent resources would make itself felt exclusively via the changes in relative prices of the different kind of commodities' (Hayek 1941:35, n. 1).

23 In his reply to Sraffa, Hayek (1932a:211) maintains: 'I have been assuming that the body of existing pure economic theory demonstrates that so long as we neglect monetary factors, there is an inherent tendency towards an equilibrium of the economic system; and what I tried to do in *Prices and Production,* and in certain earlier publications, was to show that monetary factors may bring about kind of disequilibrium in the economic system–which could not be explained without recourse of these monetary factors.'

24 This is analogous to seeing Hayek's retreat from pure economic theory after the early 1940s as an implicit declaration of inability of using this new theory of a real economy as a basis for his unchanging political view, such as in Donzelli (1988:86–9). Donzelli attributes to Sraffa's comment a role which is similar to the one discussed in this chapter.

25 Steedman (1994) points this out, referring to the analysis in Fabbrini (1950).

26 Hayek deals with this issue in Part I of *The Pure Theory of Capital,* even though Hayek's main argument, that individual 'theories' of how the economy works cannot be taken as given in a proper dynamic context is clarified only later (see, for instance Hayek 1945:15–16). This point is dealt with in detail in Zappia (1999).

27 On Hayek and the contemporary critics endorsing real theories of the business cycle see Colonna (1994:32–9). Hayek's 1941 'real' business cycle theory, must be recalled, differentiates itself from all the other real theories because of his reliance on intertemporal versus stationary equilibrium.

28 It is worth noting that sunspots models were put forward to provide a non-monetary explanation of cycles in the face of Lucas' theory of the business cycle, where, in a 'Hayekian' fashion (Lucas 1977), only a monetary mechanism of 'falsification' of prices can induce cycles.

Comments

21 A comment on Steedman

Salvatore Baldone

The core of Steedman's contribution lies in section 2 which is devoted to asking how the small open economy framework impinges on various wellknown Sraffian concepts and arguments such as basic and standard commodities and the method of 'given sectoral outputs'. On these points I wholly agree with Steedman's analysis, a fact which makes a discussant rather uncomfortable.

Therefore all that is left for me to start from is really a marginal point in Steedeman's chapter. Yet the analysis of the selected point has two positive implications: first, it makes it possible for me to appear for a moment as a *defensor fidei;* second, it refers to a very important analytical point developed by Steedman in section 2.

Here is the problem. Opening section 1, Steedman considers an economy in which all produced commodities are internationally tradeable at fixed relative prices. Then any operated production process would immediately yield a linear frontier relating a uniform real wage rate to a uniform rate of profit. That being so, the economy will exhibit all the features of the 'surrogate production function' and capital and distribution theory would be entirely straightforward from a marginalist point of view. Steedman says he is surprised that no defence of marginalist theory along these lines was put forward during the debates of the 1960s and 1970s.

Unfortunately, this life-belt thrown to the marginalist theory is faulty (as the surrogate production function is). Let us suppose that in the above economy there were three processes producing three tradeable commodities and that domestic prices corresponding to a uniform rate of interest (r^*) and to a uniform wage rate (w^*) are equal to the fixed international prices. If processes have different capital-labour ratios, then the linear w-r frontiers are shaped as in Figure 1.

Well, what will happen to the wage rate if the rate of profit (the wage rate) changes while prices are frozen at their international level? As you can see, it is impossible to maintain a uniform wage rate (rate of profit), the implication being that each w-r frontier shows the distributive possibilities of the corresponding process, but it is not representative of the whole economy. If prices are fixed, the economy gets rid of all sectoral interdependencies

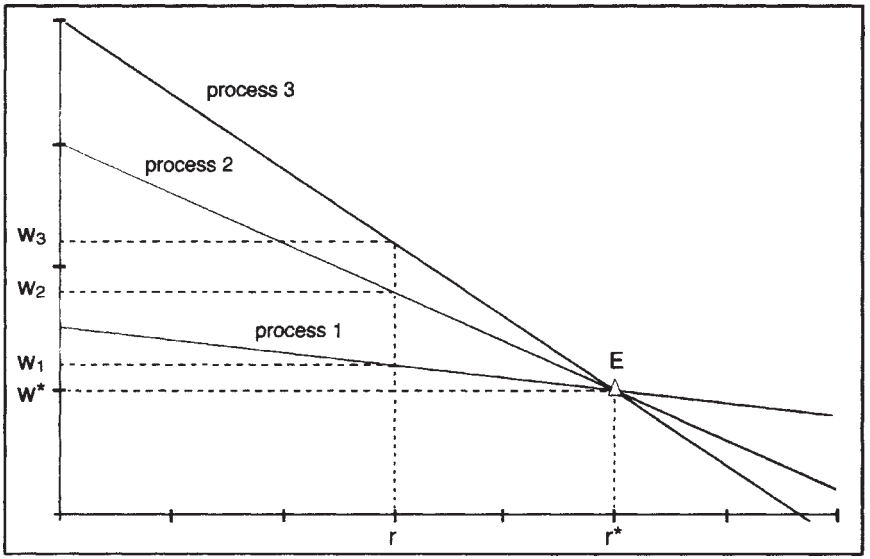

Figure 1.

changing, from the point of view of the theory of value, into an archipelago of independent single commodity subeconomies. Such subeconomies cannot be put together in a competitive setting with uniform distributive variables. I doubt all that can be of great help to the marginalist theory.

But, the above example has also a different purport. The multisectoral small open economy producing only tradeable commodities at international prices can show a uniform rate of profit and a uniform wage rate only if $r = r^*$. Except in this particular case, it is impossible to have the presence of many exported commodities with a uniform rate of profit and a uniform wage rate. If r (uniform) is different from r^*, the wage rate must differ from industry to industry according to their efficiency ranking (measured at international prices).

This is nothing but the problem raised by Steedman at the end of section 2 where the matter is analysed in terms of 'overdetermination' if we try to account for multiple-commodity exports in linear models with a uniform rate of profit and a uniform wage rate.

This apparent difficulty in trade theory is generated by the same mechanisms which, in a closed economy setting with single-product technologies, allow to select, flukes apart, only one process for each produced commodity if a uniform rate of profit and a uniform wage rate are imposed. In an open economy any process producing a tradeable commodity may produce any other tradeable commodity, exporting the domestically produced commodity and then using the foreign exchange to import other commodities. With uniform distributive variables only one among the process producing tradeable commodities will be chosen (the most efficient at given prices).

Is this last outcome to be considered a serious weakness of trade theory? I believe it is not, just as the fact that in a closed economy setting only one process is selected to produce each commodity is not a weakness. At the same time it is undoubtedly true that real capitalist economies export many commodities, as it is true that they frequently use more than one process to produce each commodity. Why does this happen in the real world? Because real economies are inhabited by (few) leaders and by (many) followers. The presence of this latter class of agents is due to the fact that managerial capabilities and technical knowledge are not common goods: followers can survive in the market only if they are ready to absorb their lower efficiency on their own rate of profit (or are able to transfer it to the wage rate).

From this point of view the way of handling the problem of multi-commodity exports, which is proposed by Steedman at the end of section 2, is not a particular solution but is the solution to this 'difficulty'.

To sum up, it would seem that linear models can account for the multiplicity of exports if this feature of capitalist economies is modelled correctly.

If the multiplicity of exports is not a real problem, what about those Sraffian concepts and arguments such as basic commodities and composition of the standard commodity which, as proved by Steedman, do not appear to retain their full value when extended to an open economy? Must we worry about that? Maybe we should, if we are devoted to a mere exegetical analysis of *Production of Commodities by Means of Commodities*. However, if Sraffa's theoretical work is taken as the core underlying new positive developments of economic theory, then such failures and the concepts themselves may be of no great importance. Evidence in support of this statement is supplied, for instance, by Steedman's brilliant contributions to the theory of international trade, undoubtedly written in the spirit of *Production of Commodities by Means of Commodities*.

22 Monetary analyses in Sraffa's writings

A comment on Panico

Riccardo Bellofiore[1]

Introduction

Carlo Panico offers an overall reconstruction of how Piero Sraffa's thoughts about monetary policy developed. He makes full and balanced use of the unpublished *Sraffa Papers (SP),* which are now available for consultation in the Wren Library of Trinity College, Cambridge.

Panico's view may be briefly summarised. Sraffa took account of monetary problems especially in the early phase of his thought, in the period that runs from his dissertation to the criticisms he mounted of Hayek and to his discussion of Keynes' claims in the *Treatise on Money* and the *General Theory*. In this period, Sraffa's thought about the theory of value and distribution had not yet reached either a sophisticated reading of the Classical and Marxian paradigm, nor a total break with the marginalist conception, whose logical incoherence he had not sufficiently appreciated. Nevertheless, from the dissertation on, he saw that the distribution of income is a 'conventional' phenomenon. The equilibrium or normal value of wages can be permanently influenced not only by social conflicts, but also by variations in monetary magnitudes, especially under the influence of central bank policies. This idea is clearly at the core of his critique of *Prices and Production*. Panico also notes Sraffa's comments on Keynes' *General Theory,* in which his criticism of liquidity preference goes hand in hand with approval of Keynes' effort to formulate a 'conventional' theory of interest.

The second phase of Sraffa's thought is certainly under way in the early 1940s, when the analytic structure of *Production of Commodities* was taking its final shape. Though his explicit comments on money become rarer, the issue nevertheless remains central. In particular, Panico insists on Sraffa's suggestion, in §44 of the book, that the assumption of the profit rate as an independent variable should be justified by the thought that, once we understand distribution of the net product as the outcome of social conflict, and thus we abandon the idea that wages are anchored to 'subsistence', then supposing the real wage to be a given quantity raises obstacles that disappear if we choose the other distributive variable as the exogenous given. Indeed, expressing wages in abstract units translates into a determinate real wage

only once prices are known; conversely, the profit rate understood as a relation has a meaning independent of prices, and it can be given in advance of them. Sraffa concludes that the profit rate can therefore be fixed by the monetary interest rate, which is in turn either set by contracting on the financial market or regulated by monetary policy.

Sraffa's unpublished papers do not play a crucial role in setting up this picture, which is already present in the secondary literature that owes much to Panico's valuable contributions. Panico himself observes that the *Sraffa Papers* served him only to complete and fill in a line of thought already known at least in outline. After all, Panico's reconstruction borrows significantly from his own analytic work. I cannot here consider the model that Panico takes as his reference point. So I shall restrict myself to raising some queries about the theoretical background and about the perspective on the history of economic analysis that Panico employs in his chapter, and to suggesting some alternative uses to which Sraffa's unpublished papers could be put.

Money and equilibrium

A good place to start is with Panico's claim that, even in the 'early' Sraffa, we find a break with the theoretical tradition that was then and is still now dominant. This break resides in the thought that monetary phenomena either directly or indirectly 'have permanent effects on the social conflicts which regulate the *equilibrium* or *normal* real wage rate' (see Panico's chapter, p. 286; original emphasis). As already noted, this point was already present in Sraffa's dissertation, relative to inflation and deflation. And it returns in Sraffa's criticism of Hayek's claim that forced saving, arising from the inflationary action of the banking system, is reabsorbed once the 'turbulence' is past, thus allowing the initial situation to be reproduced.

By way of preliminary, the first thing to discuss is the identification of the normal level with the level of equilibrium among the distributive variables. Panico introduces this identification at the beginning of his piece and it plays a key role in the theoretical direction that his argument takes. But there seem to be two reasons for querying it. The first is that the identification does not seem to hold for at least a significant part of (what he calls) the neoclassical tradition. And second, I do not think that Sraffa ever uses the word 'equilibrium' to describe the 'indeterminate' distribution among social classes, or, for that matter, the prices fixed by his analytical scheme.

As to the former of these points, Panico himself in the paper at the conference on which this volume is based recognised the presence of similarities in Sraffa's 'conventionalist' approach and some of Cassel and Fisher's conclusions on monetary policy, even if he relegates it to the determination of the value of money, which he rigidly separates from the theory of distribution.[2] As to Hayek and the Austrian tradition, I am unsure

about seeing the neutrality of money as lying at the heart of their approach, as Panico suggests on p. 293.[3] If anything, from von Mises onwards, the specific and ever more clearly expressed view of these writers is that in a monetary economy money can *never* be neutral. For the problem of forced saving does not reside in the fact that the system returns to equilibrium after the disequilibrium; but rather in the fact that an artificial political intervention will fix an equilibrium that is far from agents' 'voluntary' choices, from which there will necessarily follow a process of the 'destruction of capital'. On these grounds, a latterday Hayekian would not wish to object to Panico's observation that monetary policy and inflation determine the distribution of income, so long as one adds that they do so 'arbitrarily'.

The point at which the identification of the normal position with the system's equilibrium position shows its most severe limits is in the judgement that Panico, following a long tradition, makes about Keynes' *Treatise on Money,* which is overhastily assimilated to the neoclassical tradition according to which the 'real' part analyses the equilibrium positions, and the 'monetary' part examines the cyclic oscillations. A judgement of this sort is only possible if one does not allow that in this case–as also, after all, in the whole heterodox tradition from Wicksell's *Interest and Prices* down to the *General Theory*–a monetary economy is *normally* in a position of disequilibrium, without there being automatic mechanisms to bring it back into alignment, *either* from the monetary side *or* from the real side.[4]

A doubt might reasonably arise about the dividing line between orthodoxy and heterodoxy. Though a definition has already been implicitly furnished, it is worth paying attention to the long quotation that Panico offers from the *Sraffa Papers* on p. 294. Here, Sraffa is noting the originality of the task that Hayek ascribes to monetary theory. While traditionally non-monetary theory was set to analyse the equilibrium state and monetary theory was limited to studying disequilibrium, Hayek wanted monetary theory to cover the whole field treated by both 'pure' and 'real' theory, and to clarify the consequences of the presence of money by seeing what difference it makes.

It is clear that neither of these procedures is satisfactory. Certainly not the former, to which Hayek shows his superiority. But nor is the latter, for all that it begins its analysis with a description of the economy starting with value and distribution *without* money, to move only in a second phase to introduce it. Both ways of viewing things presuppose that equilibrium is the normal reference of the economic system, and hence that the conflict between the theories is, in the first instance, at the level of *real* theories about value and distribution. By contrast, it seems to me that Schumpeter is right to suggest that the analysis of an authentically monetary economy introduces 'money' as an element at the very basis of the analytic structure and excludes the possibility that all the essential features of economic life can be represented in a scheme of a barter-economy. The break with what has been and still is the dominant scheme is made, therefore, only if the analysis of

value is *not separated* from the analysis of money. More specifically, money must be viewed as an *essential* element in the construction of the theoretical scheme, and as an *endogenous* element, lest equilibrium, however defined, be re-established automatically as a general characteristic of the economic system. On such a view, it is clear that no equilibrium and no trend can be defined as the real 'norm' independently of the movement of the variables outside the equilibrium.

In short, I think that these thoughts should lead us to agree with Hyman Minsky when he writes: 'Another great American philosopher, Vincent Lombardi, who, like George Allen, was a successful football coach, once said "Winning isn't everything, it is the only thing". I would like to paraphrase this sage and make the radical statement that for an analysis of capitalist economies "Money isn't everything, it is the only thing"' (Minsky 1990:369). Though we cannot answer it on the basis of a simple reading of the book, there remains the important question of whether the analytic underpinning of *Production of Commodities* should be understood as a scheme that seeks to identify the fundamental relations between prices and distribution in a 'core' fully defined in real terms, to which money is added as if 'from without', or, running in the opposite direction, the book moves from an analysis of a particular moment in the capitalist circuit where that is understood from the beginning as a monetary sequence.

Related to this last matter, there is the second doubt I raised earlier. This was whether we should attribute to Sraffa a belief in the existence of 'equilibrium' values of real wages, of the money interest rate and so on. All the more so if that equilibrium is to be connected to 'normal' values in the sense of long-term positions. The doubt is a real one, to which at present I have no definite solution. But it seems to me that Sraffa keeps terminology of this sort as much as possible at arm's length, and is right to do so. One place that seems to support this is his letter of 1 March 1968 to Rüdiger Soltwedel (*SP* D3/12/18), in which he likens his scheme to a 'photograph', picking up an image used by John Eaton in his review of *Production of Commodities* that had pleased Sraffa.[5] But there are also many passages in his letters that support it, where he insists that in the book it is not the quantities that are given, but the methods of production. For instance, in the letter to Garegnani that Panico quotes, Sraffa repeatedly maintains that his scheme could be rewritten as a set of equations that represent the production of a unit (kilo, meter, etc.) of each product.

If things were as described, then there is reason to think that Sraffa's theses, including those of the later period, might be compatible with real and monetary approaches that differ from what Panico suggests. Among these there might quite possibly be the approach that Keynes adopted prior to the *General Theory*. After all, the same quotation from the 1932 rejoinder to Hayek's reply seems to be fully in line with Keynes' conclusions in the *Treatise*. As it does also with our claim that the dividing line between orthodoxy and heterodoxy should be drawn in terms of the denial that

there are automatic readjustment mechanisms in a capitalist monetary economy:

> one class has, for a time, robbed another class of a part of their incomes; and has saved the plunder. When the robbery comes to an end, it is clear that their victims cannot possibly consume the capital which is now well out of their reach.
>
> (Sraffa 1932a:48; see also 1932b:249)

Panico claims that Sraffa's criticism of Hayek's theory of the consumption of capital is that it is in conflict 'with common sense'. It may be that there is some feature of Sraffa's use of English that has passed me by. But it seems to me that what is at issue here is quite explicitly the *class* nature of the capitalist economy considered as a monetary economy of production.

Interest rate and distribution

I proceed now to suggest an alternative use of the Sraffa archive. Panico's employment of it is perfectly just and, in a certain sense, natural. The *Sraffa Papers* can fill in the gaps in an intellectual development that is already mostly well established. Thus, as already noted, Panico is telling us that, at bottom, the news from the *nachlass* is 'no news'. I am not by any means a Sraffa specialist, though I do nurse (or have nursed) strong opinions about him. But these have not been strong enough to smother what we might call the promptings of pure curiosity. One might guess that the Sraffa archive is a rich source of questions. All the more so when it tends to confirm pre-existing doubts about the commonest readings of this author, or to open up lines of research that have received little or no attention. In what follows, I act on these promptings, conceding all the while that the conclusions I offer are very partial and preliminary, not to say hypothetical.

Let us begin with the question that Panico rightly places very centrally in his work. This concerns the non-'mechanical' nature of the distribution of income that Sraffa uses to show how indeterminate it is relative to the internal 'technical' conditions of the productive system. What is crucial here is what to make of the suggestion that Sraffa made in 1960 to assume the profit rate as an independent variable which is in turn fixed by the forces that set the monetary interest rate. The importance of this is further reinforced in the letter to Garegnani of 13 March 1962, which Panico quotes on pp. 301–2. I here carry part of the passage and restore to it (in italics) some parts that Panico omits:

> I am convinced that the maintenance of the interest rate by the bank and the stock exchange had a role in determining the distribution of income among the social classes: *because this is a necessary move for those who lend and for those who borrow*.

> *As regards the review* [that Garegnani was intending to write of *Production of Commodities* for *Moneta e credita*], *perhaps it would be better not to go too far into this matter:* I didn't mean to say anything too committal, and in general I only wanted to put out some signals to stop people thinking that the system was being presented as the 'foundations' of a theory of the relative supplies of capital and labour! It is the denial that seems important: as to the affirmation, I do not at all mean to put forward yet another mechanical theory that, in one way or another, claims that distribution is determined by natural, technical or even accidental circumstances that are anyway sufficient to make all action, in any direction, futile to modify the distribution.
>
> *In conclusion, I'd say that the review would do well not to insist too heavily on the passing remark about the monetary interest rate.*[6]
>
> (*SP* D3/12/111:149; emphasis mine)

It seems to me that we can draw stronger conclusions from a text like this than Panico is inclined to do. For him, Sraffa is doing no more that stressing the 'provisional character of his conclusions on the determination of the rate of profit and the possibility of further analysis on the influence of government intervention on the equilibrium level of distributive variables'. In my view, rather, Sraffa is asserting the *negative* nature (the underlinings are his, not mine), that is to say, critical of marginalism, that characterises most of the claims of *Production of Commodities*.

As regards the building of an alternative account of distribution, Sraffa goes no further than to say that he does '*not* at all mean to put forward yet another mechanical theory' of it. This means, what is not unusual with Sraffa, that the affirmative begins with a negation: 'I didn't mean to say anything too committal', he insists. In further support of the destructive character against marginalism, rather than constructive in favour of a Classical-Marxian revival, he wanted only 'to put out some signals to stop people thinking that the system was being presented as the "foundations" of a theory of the relative supplies of capital and labour'. The task of reconstruction is thus pointed at. But it is not carried out.

The caution is, if anything, further reinforced by two points that Panico omits. First, there is the suggestion not 'to go too far into this matter'; the other is the description of what he says about the rate of monetary interst as 'a passing remark'. Of course, we have to pay proper attention to the reason that Sraffa gives for thinking that the interest rate is important in determining the distribution of income. This is that it is 'a necessary move for those who lend and for those who borrow'. Here, clearly enough, the stress is on the demand for money 'to spend', which for various reasons sits ill with the Keynesian scheme of 1936.

In this last connection, there is something startling about Panico's silence (apart from a swift reference) over Sraffa's criticism of the *General Theory* account of liquidity preference. As Fabio Ranchetti (1998) has recently

shown, Sraffa moves from criticising the way that Keynes founds the falling relation between the quantity of money and the interest rate, while accepting that the relation does in fact hold, to inverting Keynes' causal sequence as between the abundance of money and the interest rate. Where Keynes takes it that an abundant supply of loans pushes the interest rate down, Sraffa places centre stage the behaviour of the banks, which, in order to increase the quantity of money, must reduce interest rates so as to stimulate demand for loans. (A criticism of this sort once again clearly shows Sraffa's closeness rather to the Keynes of the 1930 *Treatise* and of the writings on finance in 1937–9, than to that of the *General Theory*.)

Of course none of the foregoing subtracts from the importance of the efforts to develop Sraffa's suggestion in §44, and hence from Panico's independent research. All it means is that that suggestion does not amount to a high road pointed out by Sraffa as the basis of a 'conventionalist' theory of distribution. Here again, as in so many others of his letters, Sraffa shows a diffidence about presenting his scheme as the core of a positive construction. And it might all the same be suggested that it would be none too hard to fit his cautious moves towards specifying the relations between money and distribution, and towards a 'conventional' determination of the interest rate, within a radically anti-marginalist version of the heterodox monetary thought of the early 1930s to which I have already alluded.

Interest, money and exploitation

Proceeding with the issue of the relation between the monetary interest rate and distribution, and of the conflictual nature of the division of the surplus, Sraffa's papers are full of suggestions where one might least expect them. Here perhaps something new, or at least unexpected, can be found. I have in mind the reflections on the 'transformation problem' in Marx, a writer to whom the *Sraffa Papers* return again and again particularly, but not exclusively, in the early 1940s, when the composition of *Production of Commodities* was under way in earnest.[7] It is worth quoting some passages from a document of 1960–1:

> The tiresome objector says. Suppose that the (ratio of wages to profits) the rate of surplus value is 100% at values, but 150% if calculated at current prices of production. Which is the correct one?
>
> Now M. would, I think, reject this question. He would say that his system is based on the assumption that the ratio of these aggregates is approximately constant, whether at values or prices. And that such deviations do not occur in fact.
>
> Although still correct in fact, this answer is not found adequate at the present day, after 100 years onslaught. It must be faced.
>
> And if such a situation occurred, it is clear that the 'price' rate would be the correct one. In effect, the workers get 40% of the nat. income: on what

commds. they spend it, depends on 'utility': whether they choose to spend their 40% on high or low org. comp. commodities does not affect the degree of exploitation. From which I should conclude that the relevant rate of s.v. is to be taken at 'prices.'

(*SP* D3/12/111, 138; original in English)

The propositions of M. are based on the assumption that the comp. of any large aggr. of commodities (wages, profits, const cap.) consists of a random selection, so that the ratio between their aggr. (rate of s.v., rate of p.) is approx. the same whether measured at 'values' or at the p. of prod, corresp. to any rate of s.v.

This is obviously true, and one would leave it at that, if it were not for the tiresome objector, who relies on hypothetical deviations: suppose, he says, that the capitalists changed the comp. of their consumption (of the same aggr. price) to commods of a higher org. comp., the rate of s.v. would decrease if calc. at 'values', while it would remain unchanged at p. of prod which is correct?–and many similar puzzles can be invented.

[Better: the caps switched part of their consumption from comms of lower to higher org. comp., while the workers switched to the same extent theirs from higher to lower, the aggr. price of each remaining unchanged….]

It is clear that M's pros are not intended to deal with such deviations. They are based on the assumption (justified in general) that the aggregates *are* of some average composition. This is in general justified in fact, and since it is not intended to be applied to detailed minute differences it is all right.

This should be good enough till the tiresome objector arises. If then one must define which is the average to which the comp. should conform for the result to be exact and not only approximate, it is the St. Comm….

But what does this average 'approximate' to? i.e. what would it have to be composed of (what weights shd the average have) to be exactly the St. Com.?

i.e. Marx *assumes* that wages and profits consist *approximately* of quantities of st. com.'

(*SP* D3/12/111:140; original in English)

'It seems to me that the only rational way to calculate is by starting with the interest rate $\underline{r}$ (which is a matter of observation) and to deduce from it the rate of exploitation (that is, the standard wage w and from that arrive at the surplus value rate

$$\frac{1-w}{w} = \frac{1}{w} - 1$$

> The wage and the aggregate profit in this situation are, at best, rough approximations of the standard wage and profit. But the profit rate in this situation is identical with the standard one.
>
> (*SP* D3/12/111:139[8])

Sraffa's reasoning that can be drawn from his papers and from the foregoing,[9] seems, in short, to be the following.

Both Marx and Ricardo refer the value of the product to human labour. In doing so, they use the phrase 'human labour' in two distinct senses. In the first, which is correct but not quantifiable, they identify the *whole* production process as human labour. In the second, which is quantifiable but potentially misleading, they reduce human labour to the number of labour hours, that is, to only *one* of the facets of production.

The second sense of the term is potentially misleading because it might lead one to suppose, wrongly, that relative prices can be directly identified with the relation between the respective amounts of embodied labour, and that profit is proportional only to labour. According to Sraffa, taken in this sense, the labour theory of value must be rejected. He adds, however, that this was not Ricardo's or Marx's sense, as is evident in the latter's 'transformation' of values into prices. At least from the early 1940s, Sraffa took a view of Marx's transformation that he maintained as a firm judgement even after *Production of Commodities*. This was that Marx assumed in his procedure that the social product constitutes the standard of the average organic social composition. Sraffa took some assumption of this sort to be reasonable and in the main adequate to account for large-scale aggregates, even if it furnishes only approximately accurate results. Nevertheless, Sraffa thought that Marx's procedure could be developed to yield exact results if one is prepared to employ his Standard commodity. And, even in historical conditions where wages appropriate part of the surplus, we can quantify 'exploitation' by employing a rational–indeed, the 'only rational'–calculation of the rate of surplus value, namely, by starting with the interest rate and its determinative influence on the profit rate.

For reasons of space, we cannot pursue this line of thought. But there are two points that may be stressed.

The first is that, as we see in the final passage cited, Sraffa uses the idea of a fixing of the profit rate by the interest rate to determine the rate of exploitation (with wages measured in terms of the Standard commodity). Here, the original justification is given that the interest rate is a 'matter of observation'.

The second is, as the other passages indicate, that, in a monetary economy, the rate of surplus value should be measured in 'prices' and not in 'values'. This is exactly the conclusion that, in the last two decades, the new, broadly 'monetary', approaches to the labour theory of value have been attributing to Marx.[10] There is some analogy between these approaches with

what is argued in §§10 and 12 *of Production of Commodities*. In the new approaches, as in Sraffa, the national income, expressed in prices, is placed in a one-way relation with the total annual labour of society. More exactly, the relation is with the direct labour multiplied by the 'monetary expression of labour', or the inverse of the 'value of money', which Sraffa implicitly posits as equal to one. Hence, the national income is assumed as the measure of value. Further, on this account, wages are regarded as anticipated in nominal terms and the rate of surplus value is (re-)interpreted as the share of the labour 'represented' in national income going to monetary profits over the fraction going to the monetary wage bill.

As an interpretation of Marx, the foregoing is foreign to Sraffa. And, from the point of view of a close reading of the nature of the rate of surplus value, there is some reason to doubt that the New Interpretation's exegesis exhausts the meaning of that notion in Marx. The need to find a bridge between his own views about distribution–not too far from the new approaches–and Marx's may even be seen as one of the reasons why Sraffa gives so much weight to the Standard commodity, even though he stresses that it is a mere 'auxiliary construction'.

As the passages cited from the *Sraffa Papers* in conjunction with § 10 and §12 of the 1960 book shows, the distance between Sraffa and the new readings of Marx is much less if we look at the issue from other angles. This is particularly true as regards an updated and positive rewriting of a Marxian theory of exploitation *that takes full account of the monetary nature of the capitalist economy*. And this is obviously closely connected to the theme that interests Panico. Here, again, of course, we are talking about *suggestions* that Sraffa was careful not to develop or to present as in some way privileged or, in the present case, even to publish. Yet they are there. There is another annotation, taken this time from among the many detailed criticisms that Sraffa made of Bortkiewicz, dated to 1943 that might show that is the way that things stand:

> What Marx does is, on the one hand (1) to take wages as given (inventory) in commodities, for subsistence, and on the other (2) to take the mass of profits as a given proportion of the product of labour. The two points of view are incongruous, and are bound to lead to contradictions. But B. wants to solve the contradiction by bringing (2) into agreement with (1). On the contrary, the correct solution is to bring (1) into agreement with (2). For the point of view of (1) useful as it is as a starting point considers only the fodder-and-fuel aspect of wages, it is still tarred with commodity-fetishism. It is necessary to bring out the Revenue aspect of wages; +this is done by regarding them as w, or a proportion of the Revenue. This is (1) brought to agree with (2); and the conclusion that *all* capital must be taken into account for the rate of profit becomes true.
>
> (If the objection is made, that wages must have been paid in advance +therefore are fixed in definite concrete goods, the answer is that the

> problem of w is that of the replacement of the wages advanced, out of the product.
>
> Now, what is wanted is a similar step in regard to the "advanced" constant capital, to divert it of its fetish character (of machines, etc.)+ considers its replacement as a proportion of the gross product.
>
> The answer seems to be: as wages are here, in effect, assumed to be advanced in money–so that the constant capital must be assumed to be advanced in money. And the advance of Const. Cap. must change with the change of *r*.)
>
> (*SP* D1/91; original in English)

Once again we see clearly the role that money plays in the way that Sraffa sets up the account of distribution, as well as the relation of both continuity and discontinuity with Marx.

What is certain is that, at least in his notes on, and also *against,* Bortkiewicz, Sraffa is worried lest an 'absolutely exact' determination of the relation between values and prices sacrifice the 'essential nature of the question', which is the 'fundamental fact' that the production of commodities by means of commodities is production *by means of labour:*

> But the real objection (though somewhat vague) is this: that B's point of view, for the sake of obtaining absolute exactness in a comparatively trifling matter, sacrifices (by concealing it) the essential nature of the question–that is, that commodities are produced by labour out of commodities.
>
> (*SP* D1/91; original in English)

Sraffa's position in the early 1960s does not seem to have changed much relative that of the early 1940s.

Once again, nothing we have said is conclusive about whether criticism of Marx on the basis of Sraffa is valid or not. It shows, however, that, as a matter of the history of economic thought, we can formulate some conjectures about Sraffa's reading of Marx in the light of the *Sraffa Papers.* These conjectures goes against the conventional wisdom on the issue of 'Sraffa after Marx' and, of course, may be falsified: but, given the quotes from Sraffa given in this chapter and the countless others which may be found, they must be taken into account by serious scholars.

The conjectures are as follows. First, that Sraffa never really meant his reflection to constitute a break with Marx, including the labour theory of value. Rather, in agreement with the witness of Joan Robinson and Antonio Giolitti, Sraffa sought continuity. Second, that Sraffa regarded the 'first sense' of the phrase 'human labour' that he attributed to Ricardo and Marx–in the argument justifying the reference of the value of the product to labour–as important and basically correct. Third, that Sraffa likewise regarded Marx's procedure of transformation as substantially correct, and many criticisms of it, as well as Bortkiewicz's and Tugan-Baranovski's 'corrections', as controversial.

And, finally, that Sraffa regarded his analytical approach as a fair starting point, once the assumption of a given subsistence basket has been jettisoned, for modifying Marx's argument so as to adapt it to the hypothesis of an anticipated wage in money and defined as a proportion of revenue.

The lectures on monetary matters

It is much to Panico's credit that he gives a concise account of the lectures that Sraffa gave at Cambridge on monetary matters.[11] He gives a summary of the lectures, 1929–31, on *Continental Banking,* and, in a note, a reference to the 1941–2 course on *Industry*. Because I have gone well beyond the limits of a comment, I shall make just a few points about the former and offer a single quotation from the latter, which seems to me to be useful to clarify Sraffa's attitude to the analysis of distribution.

As usual, Panico's discussion of the 1929–31 course seems to me very balanced. He describes how Sraffa emphasises the difference between the English banking system, with its 'specialised' banks, and the German 'mixed' system. These latter, in Panico's view, do better at 'channelling financial resources to productive sectors', and, unlike the English ones, they are the product of genuine 'project' of intervention on society.

Two questions arise. The first is whether the phrase 'channelling financial resources' might not give the impression that Sraffa had not fundamentally rejected the idea that banks are intermediaries. My impression, on an initial reading of the Continental Banking course and other notes on monetary questions, is that Sraffa is quite at home with the model of the banking system in which loans 'make' deposits, and in which, if the central bank does not intervene as the lender of last resort to ensure the level of liquidity in the banking system, the individual major banks could provide it by mutual agreements and compensations, allowing a coordinated, and theoretically unlimited, expansion of loans.

The second thing I wonder is whether, in these early writings, Sraffa does not come close, as it seems to me he does, to the idea that the analysis of a monetary system can best be carried out by seeing it in its developed form, rather than looking at its initial stages. If this were so, Sraffa would not be limiting himself to a comparison, more empirical than theoretical, between two different banking systems. Rather, he would be presenting in his lectures an enquiry that is also, if not especially, of theoretical interest.

Among the notes for the lectures on Industry, in which Sraffa adopted Berle and Means as his main reference points, he cites a sentence from the two Americans and adds a critical remark that has a certain bearing on the matters we have been considering to do with distribution:

> Berle-Means: 'The greatest question is–in whose interest are the great industrial companies operated.'
>
> This, they (B & M) say, is the same as asking 'who receives the profits of industry.

> This is characteristic of the vice of economists. Thinking that all can be reduced to the extreme simplicity of the money measure: also, that production is a purely technical question+that economic problems arise only in distribution. This represents the situation as being that the question of who controls industry makes a difference only after the production year has been closed. The profits being earned, a balance sheet drawn up, +then the question arises–how to divide the profits.
>
> However, the questions of who controls industry has much more extensive effects than that suggests: it chooses the methods of production, the size of the unit, the size of the profits available, the proportion in which profits are retained+distributed, etc.–all this before the question arises of who should receive whatever is regarded as profits. Also, patronage, salaries, etc. (Examples: winding up, or amalgamation (directors v. stockholders) machinery+labour.)'

It has not gone unnoticed that the 'vice of economists' that Sraffa deprecates resembles, at least in some respects, the limitation that, rightly or wrongly, was attributed to the author of *Production of Commodities*. Rather, the 'distribution' that this passage discusses must be understood as the result of choices that are not merely technical and that should be analysed 'before the harvest'. These, presumably, are choices where money, not in its extremely simple role as measure of value, but as monetary *capital* is of the utmost importance. For this reason the question again arises of the wider vision into which to fit the theses of *Production of Commodities*. From such a vision, analytical consequences would have to flow. But on this point, as on others, Sraffa wanted to keep silent, so that everyone can, to use his own words, interpret him in his own way.[12]

Unless I am much mistaken, this attitude of 'openness' constitutes one of his most lasting lessons.

Notes

1 Thanks to a grant from the Italian Ministry for the Universities and Scientific and Technological Research, I was able to have access to the *Sraffa Papers* that form the subject of this chapter. I wish to thank Pierangelo Garegnani for the permission to quote from the *Sraffa Papers*.

2 In the fn. 15 of his Italian paper (1998), Panico held a stronger view, which recognised in Fisher some traces of a 'conventionalist' perspective: 'Fisher didn't follow a conventionalist approach in his theory of distribution, which was coherent to the neoclassical tradition then prevailing, but in the statement that the equilibrium value of money depend on the evolution of monetary policy' (my translation). In the phrasing of the English final draft of his chapter Panico seems to have further weakened the connection between Fisher and the 'conventionalist' approach, but this does not affect the main thrust of my comment

3 Here again Panico inserted into the final English draft fn. 39 which was absent in the text I commented at the conference. The note refers to a document in the *Sraffa Papers* which shows that Sraffa felt the need to criticise the distinction

between 'natural' and 'artificial' actions, confirming my view that the point cannot be avoided in a critical confrontation with the Austrians.

4 In fact, we should here distinguish at least between the notions of real equilibrium (investments against voluntary saving) and that of monetary equilibrium (the closing of the monetary circuit), which are present in this line of thought. The latter is possible without the former, which does not act as a norm of actual, but of only ideal, reference.

5 Eaton (1960:721) observes in discussing the subsystem of the Standard commodity, defined as a simplified model that abstracts from technical changes, that 'it photographs, so to speak, the rotary and repetitive process typical of the lifecycle that is constantly repeated by the capital employed to produce other capital and so on'.

6 The original is in Italian, and reads as follows: 'sono convinto che il mantenimento del saggio di interesse da parte della banca e della borsa abbia avuto la sua parte nel determinare la distribuzione del reddito fra le classi sociali: *perché è un passaggio obbligato per chi dà e per chi prende a prestitoMa per la recensione èforse meglio non avventurarsi troppo su questo terreno:* io non ho inteso dir niente di molto impegnativo, e in generale ho solo voluto metter fuori qualche segnale per evitare che si creda che il sistema viene presentato come 'fondamenta' per una teoria delle offerte relative di capitale e di lavoro! E' la *negazione* che mi sembra importante: quanto alla *affermativa* non ho nessuna intenzione di mettere avanti un'altra teoria meccanica che, in una forma o nell'altra, ribadisca l'idea che la distribuzione sia determinata da circostanze naturali, o tecniche, o magari accidentali ma comunque tali da rendere futile qualsiasi azione, da una parte o dall'altra, per modificarla. *In conclusions direi che nella recensions è meglio non insistere troppo sull'obiter dictum del saggio monetario dell'interesse.'*

7 It is a pity that at the conference there was no paper specifically devoted to the relation between Sraffa and Marx, which goes well beyond his relations with the other Classicals of political economy. It is a pity not merely because of the vigorous controversy that the matter raised not so long ago, but also, and especially, for the reason given in the text, namely, the mass of previously unknown notes explicitly about Marx. Whatever line one were to take towards this material, it would be sure to put the discussion on a wholly new footing.

8 The original is (mostly) in Italian, and reads as follows: 'A me sembra che l'unico modo razionale di calcolo sia di partire dal saggio di interesse r (che è un fatto di osservazione) e da questo dedurre il saggio di sfruttamento (e cioè il salario standard w e da questo il saggio di sopravalore

$$\frac{1-w}{w} = \frac{1}{w} - 1$$

Il salario e il profitto aggregate della realtà sono, at best, rough approximations al salario e profitto standard. Ma il saggio del profitto della realtà è identico a quello standard'.

9 Where the 'tiresome objector' means Sraffa himself.

10 See the 'New Interpretation' of Foley (1982 and 1986), Duménil (1980), Lipietz (1982). Even if there are some important differences, we may refer more widely also to the lines of thought represented by Wolff-Roberts-Callari (1982) and Moseley (1993).

11 Before discussing the lectures, Panico also records Sraffa's other contributions to issues to do with money. These include his articles about the banking crisis and his exchange with Tasca. In commenting on these Panico says that 'Sraffa's analysis is not based on a rigid class division, but acknowldges the existence of

conflicts within the capitalist class', thus distancing himself 'from the Marxian tradition that was then prevailing'. It is less than certain who Panico is referring to. But I do not see that it would be a novelty for Marxism to recognise that different sections of capital are in conflict with each other in such a way as to influence politics and economic policy. It seems to me that Sraffa himself goes no further than to criticise mistaken applications of a method that he holds to be basically right.

12 The expression 'everyone has been left free to interpret *Production of Commodities* in his own way' appears in a letter (in Italian: 'ognuno è stato lasciato libero di interpretare *Produzione di merci* a suo modo') to Aurelio Macchioro, dated 3 December 1960 and published by its recipient in Macchioro (1991).

23 Whatever happened to Dr Hayek after the debate with Mr Sraffa?

A comment on Zappia

Nicolò De Vecchi[1]

According to Carlo Zappia, Sraffa's criticism had a crucial impact on the development of Hayek's thinking. Zappia contends that it contributed in a profound way to the critical review of the equilibrium theory which Hayek undertook in the 1930s. This apparently was because not only had Hayek to restate his definition of intertemporal equilibrium in the light of the Sraffian notion of 'own rates of interest', but he had to effectively revise his opinion about 'the ability of real economic systems to automatically adjust to exogenous real shocks'. In particular, according to Zappia, it was only on account of Sraffa's criticism that Hayek realised that the lack of coordination of intertemporal plans of action can also occur in real economic systems.

Sraffa's criticism supposedly forced Hayek to substantially change his conclusions regarding the causes of disequilibria in modern economies. While he continued to hold that the disequilibria generated by monetary variations are quite distinctive, Zappia holds that Hayek was induced to present them 'as special cases of the incompatibility of intertemporal plans'. Zappia admits that Hayek did not alter his views on the role of money and of monetary policy, though they were, allegedly, invalidated by the developments of his business cycle theory.

Zappia carried out a very careful philological examination of Hayek's texts and identified passages in which Sraffa's influence is evident either from the similarity in the language used or from the addition of more precise explanations that seem to be dictated by Sraffa's criticisms. Nevertheless, it is difficult to agree with Zappia when, on precisely these grounds, he concludes that Sraffa's criticism had a 'lasting influence' on the evolution of Hayek's thinking and even constitutes 'a main cause' of the changes which Hayek made during the 1930s to his depiction of the structure and operation of economic systems.

I will attempt to show that Zappia overestimates Sraffa's influence on Hayek and that many of the changes that he alleges were directly or indirectly determined by Sraffa's criticism are in actual fact reworkings of theories already formulated by Hayek in his earlier works, and which differ, if anything, only in their presentation.

In short, Zappia's proposal suffers from an excess of emphasis. The connection which he establishes between Hayek's debate with Sraffa and the former's 'transformation' during the 1930s is so strong and direct as to lead to an unduly unilateral interpretation of the development of Hayek's thought. It seems somewhat excessive to attribute Hayek's views in the 1930s on the structure of real economies and the circumstances which lead to the lack of coordination of the intertemporal plans of action solely or mainly to Sraffa's criticisms.

Zappia contends that, after the debate with Sraffa, Hayek no longer considered money 'an indispensable, necessary element for the cycle to occur'. I do not subscribe to this belief. I am inclined to think that Hayek's modifications to his theory on the causes of the disequilibria in the market economies in those years concern presentation, rather than content. Hayek gave up using some analytical instruments in favour of others and looked at the individual interrelations from a different prospective with respect to the past (section 1), but he did not alter his theory on the causes of the disequilibria (section 2). As a consequence, there is no contradiction between the developments of Hayek's business cycle theory and his conclusions regarding the role of money and of monetary policy (section 3).

What was the reason for Hayek's 'transformation' in the 1930s?

Many scholars of Hayek have become involved in the debate which was triggered by the writings of Hutchison (1981) and Caldwell (1988) on the change in Hayek's theoretical attitude during the 1930s and which became known as Hayek's transformation. It can hardly be denied that 'Economics and Knowledge' represents a turning point in Hayekian thinking, but it proves more difficult to find any common elements among the numerous explanations of this phenomenon. The basic contrast is between those who consider that shift sudden in time and radical in theoretical content, and those who interpret it as a moment of 'subtle and gradual transformation in Hayek's views on the scope and applicability of equilibrium theory' (Foss 1995:346; see also McCloughry 1984; Butos 1985). With his work Zappia enters the debate, putting forward the proposition that a 'relevant' and perhaps even leading role in the Hayekian transformation should be attributed to Sraffa. While he does provide a valuable contribution to the debate, he leaves himself open to charges of telling just 'a part of the story' (Foss 1995:346).

In order to recount the story in such a way as to make it not only sufficiently credible, but also to facilitate an understanding of the theoretical contribution of Hayek and his importance as a *political economist,* account must be taken of the context in which the Hayekian transformation took place.

In the early 1930s Hayek received from the works of others–or sought himself–various stimuli of a cultural nature. At the same time he was

reconsidering the Austrian foundations which supported his theory. It is only by starting from the combination of these circumstances that it is possible to grasp the real nature and importance of the development in his thinking.

At the root of it all is the fact that he was called to London by Lionel Robbins to assist in the dispute with Keynes on the policies to combat the depression. This circumstance is not irrelevant. It highlights the fact that Hayek is essentially interested in *political economy*. He was such before arriving in London and he continued to be so thereafter. In the early 1930s he took a firm stance on a concrete economic problem–that of the depression and its causes–and not only did he endeavour to combine theoretical research and prescriptions for economic policy, but he also tackled debates of a purely theoretical nature, always with the objective of defending a particular theory on the role and limitations of economic policy.[2] The criticisms of Sraffa, but also–and particularly–those of John Maynard Keynes and Gunnar Myrdal[3] did not cause him to deviate from that objective, but instead led him to reinforce the theoretical basis of his supporting arguments. He did not react to the criticisms that he received on the analytical level by modifying the content of his theory. On the contrary, as the way in which it had been articulated had not proved convincing, he accordingly rewrote it, changing the form but not the substance.

Also in the early 1930s Hayek re-evaluated the works of Menger, which were re-issued under his editorship (Menger 1934–6). Just like Menger, Hayek openly manifested his aversion to the positivist mentality of those who assume that the data are 'objective real facts' instead of highlighting the problem of 'the division of knowledge' (Hayek 1937:38–9, 50–5). Consistently with the rediscovery of these aspects of his training in Austria, in these years Hayek also showed a growing interest in the elements of subjectivism present in the work of Mises.[4]

'Economics and Knowledge' and the other writings which closely preceded or followed it are the 'overall' result of the not entirely coincidental conjunction of these circumstances. From the debates in which he was involved and from his own personal deliberations, Hayek was induced to limiting his use of equilibrium analysis as an instrument for dealing with the problems of the coordination of intertemporal decisions, and to highlighting the inherent ambiguity in concepts such as capital or savings and investment used by himself and especially by others, Keynes in particular (Hayek 1931/2:125, 131–3, 138, 179–81; 1935b:151–6; 1935c; 1941:334–50; 1995:165, 170–1). Above all, Hayek was induced to consider the structure and the dynamic of economic systems by emphasising the subjectivist basis of his theory, which was already present, albeit latently, in earlier works. In the writings in question, Hayek subjected the analytical instruments employed up to then to a comprehensive revision and radically overhauled his exposition. However, he did not alter the basic proposition that he sought to demonstrate

with his research–that of the inevitability of phenomena of disequilibrium in the presence of money–nor did he modify his prescriptions on the nature and limits of an active economic policy to deal with such disequilibria.

Individual knowledge, prices and the trade cycle

Hayek had already reformulated his theory on the causes of disequilibria in his replies to Keynes and Sraffa by highlighting the role of decisions, intentions, expectations of consumers and entrepreneurs. In Hayek (1935b, 1935c, 1937) there is much more. In these works Hayek redefines the equilibrium with reference to knowledge and individual expectations and founds his study of the 'mechanism' which brings to equilibrium an economic system which had departed from it, no longer on 'objective data' and 'objective facts', but on 'relevant foresight' of individuals (Hayek 1937:50–5), that is the knowledge of the particular circumstances of time and place which is scattered amongst individuals. 'The equilibrium relationships cannot be deduced merely from the objective facts, since the analysis of what people will do *can start only* from what is known to them' (Hayek 1937:44; italics added).

Now the reference data used to explain the dynamic process to which the market economies are subjected are no longer directly the stock of capital, the current saving, the time preferences, etc., but the 'extent and distribution of foresight' (Hayek 1935c:116–25; 1935b; 1936:164–5; 1941:334–5). 'It has become clear that, instead of completely disregarding the time element, we must make very definite assumptions about the attitude of persons towards the future' (Hayek 1935b:139).

Against this important novelty there is the invariance of the 'mechanism' which brings the system which had departed from it back into equilibrium. The idea remains that prices are the main, if not the only 'guide and regulator of all economic activity in the exchange economy' (Hayek 1928:71; 1933a:68–78, 80–5). What directs the knowledge scattered amongst individuals, allowing them to spontaneously render their respective plans compatible, are the price variations of the goods available at various points in time.

If all entrepreneurs 'simultaneously make mistakes in the same direction'–i.e. provide quantities of consumers' goods at various dates, which do not coincide with the distribution of consumption over time, decided by consumers–this is a result of the fact that they were deceived by the prices in force at the moment when they made their decisions (1935b:141–2). The theorist's task is to consider the nature of price variations and why it should happen that, in the presence of a system of prices which is normally a reliable indicator, a conflict between the intentions of the consumers and the intentions of the entrepreneurs should be generated, or–to use an expression which Hayek was loath to–why the correspondence between saving and investment is disturbed.

In the writings of the 1930s and early 1940s–in particular *The Maintenance of Capital* (1935) and in *The Pure Theory of Capital* (1941)–Hayek enumerates the various causes which disturb the correspondence between saving and investment.

A first distinction must be made between permanent variations of the rate of saving and other causes. The former induce the entrepreneurs to adjust the production of consumers' goods available at different dates. The variations of the prices of goods and in the money rate of interest supply the information necessary to restore the correspondence between the intentions of the consumers and the intentions of the entrepreneurs. Addressing the problem from a subjectivist prospective Hayek acknowledges that the information deriving from the price system may not be sufficient to allow the coordination of the plans of action, and in 'Economics and Knowledge' he paves the way for future research on the role of competition and the system of abstract rules of conduct in the formation and maintenance of spontaneous order.

The other causes which disturb the correspondence between saving and investment share the characteristic of being temporary and not permanent. It must be assumed that a temporary change in the economy took place which was registered by the prices and which the entrepreneurs perceive as permanent. This differentiates these causes from the previous ones. Hayek distinguishes them into real causes and monetary causes.

Real causes include, for example, shifts in demand and very large, unforeseen and violent fluctuations in the rate of savings (Hayek 1933a: 79 note 2; 1934:167; 1935b:143; 1935c:125–9; 1941:338–40, 343). Monetary causes can be 'shortly described as changes in the quantity of money and changes in the velocity of circulation' (Hayek 1935b:144). Including, in particular, on the one hand changes in the desire of individuals to hold money as a general reserve which enables the holder to take advantage of unforeseen opportunities (Hayek 1941:353–68, 406–7) and, on the other, the credit created by commercial banks, which deliberately lower the rate of interest and make additional money for investment purposes available to entrepreneurs. In *The Pure Theory of Capital,* where this classification is considered with more attention than elsewhere, Hayek immediately notes that 'the essence of the difference' between real and monetary changes is that monetary changes 'are bound to set up expectations which will *inevitably* be disappointed' (Hayek 1941; 343 italics added) and this seems to constitute the *sole reason* for distinguishing them as 'special cases' or 'particular instances' among the various causes which generate wrong expectations.[5] As regards the real causes, these give rise to cyclical fluctuations *insofar as* they are accompanied, as is the norm, by changes in individuals' desire to hold money or in the behaviour of the credit system.

These views are not at odds with what Hayek had already affirmed in *Monetary Theory and the Trade Cycle,* for example. Considering a stationary economy, Hayek indicated numerous possible causes of the cycle:

> 'It must be emphasized first and foremost that there is no necessary reason why the initiating change, the original disturbance eliciting a cyclical fluctuation in a stationary economy, should be of monetary origin. Nor, in practice, is this even the case. The initial change need have no specific character at all, it may be among a thousand different factors which may at any time increase the profitability of any group of enterprises.'
>
> (Hayek 1933a:182–3)

Basically, Hayek never tires of reiterating that the original cause could be either real or monetary,[6] but a real cause *only* gives rise to a trade cycle *insofar as* it is accompanied either by discretional variations in money supply or by changes in total money expenditure.[7] This is one of the most important tenets of Hayek's thought: before and after the debate with Sraffa, and independently of the differences one can perceive in the exposition of his theory.

A necessary and impracticable monetary policy

The inevitability of the disturbances generated by the presence of money in market economies is undoubtedly another tenet of Hayek's thought. On this issue, his opinion has always been steadfast and unwavering. From this belief Hayek draws an important conclusion on the nature and the limits of monetary policy, and this conclusion is equally firm in his thinking,[8] even if sometimes, especially in his early works, it is expressed in terms that have generated some misunderstandings, as he himself has acknowledged.

In their debate with Hayek, both Keynes and Sraffa accuse him of having indicated a precise monetary policy rule in *Prices and Production:* to keep the supply of money 'absolutely and forever unaltered' (Keynes 1931:152–3; Sraffa 1932a:199). As is widely known, Hayek rejects this accusation and he concentrates on re-expounding his ideas on the nature and the limits of monetary policy already in the second edition of *Prices and Production* so as to avoid misunderstandings.[9] Nevertheless, it is necessary to emphasise that what Hayek expresses on this point in the first German edition of *Prices and Production* (1931), in *Intertemporal Price Equilibrium and Movements in the Value of Money* (1928), and even as far back as *Währungspolitik in der Vereingten Staaten* (1925), substantially coincides with his stance following the debate with Keynes and Sraffa.[10]

The fundamental theoretical premise is that the trade cycle is an immanent characteristic of a monetary economy: it is in fact necessarily connected with the monetary organisation of the modern economic systems. Economic disturbances are 'irreparably bound up with the operation of *all* conceivable monetary systems', even the systems based on a commodity money or on a tied currency (Hayek 1928:95–106, 189 (italics added); 1925:11–12; 1933b:141–8).

This proposition is continuously confirmed by Hayek, albeit with arguments which change over time. When Hayek makes use of the logic of static equilibrium theory or of the intertemporal equilibrium system, he shows that money, given that it makes indirect exchange possible, 'does away with the rigid interdependence and self-sufficiency of the closed system of equilibrium and makes possible movements which would be excluded from the latter' (Hayek 1933a:44–5, 86–8, 91–5, 101–9, 176–80, 1928:102). In order to explain the trade cycle, it is not necessary to assume that banks implement a discretionary monetary policy. Hayek shows that in monetary economies it is impossible to keep the correspondence between the monetary rate of interest and the natural rate of interest if changes in investment productivity or in the rate of saving occur. Even if banks pursued the goal of keeping the supply of credit in equilibrium with the supply of saving, they would not be able to achieve such a goal.[11]

As we have already seen, in later works Hayek adopts the subjectivist approach, which is still latent but not absent in earlier works. At this stage, he does not modify in any way his view on the immanent feature of cyclical fluctuations in monetary economies, but he arrives at it from a different route. In fact, now he moves from the premise that individuals have an incomplete knowledge of the future. As a consequence, they hold money in the hope that it will prove more useful at some uncertain later date.[12] Even time that unforeseen and abrupt changes of the rate of saving or changes in investment productivity occur, such that individuals modify their liquidity holdings, even the money rate of interest and the prices of goods change. Firms are induced to formulate wrong expectations and the whole economy is subjected to a cyclical fluctuation (Hayek 1941:334–46).

On this theoretical premise Hayek builds his thesis that a policy of 'deliberate management' of money or a policy aiming at keeping the quantity of money constant leads not to the restoration of the equality between savings and investment, but rather to a misdirection of production.

As for the policy of deliberate management of money, Hayek always maintains a negative opinion. In his early works he states that the deliberate management of money can be proposed only if one does not take into consideration 'how little we really know of the forces which we are trying to influence'.[13] After having developed his theory of knowledge, Hayek firmly states his opinion that the monetary authority and the banks do not in fact possess knowledge that is different from that held by individuals; hence, they are not able to associate variations of the money rate of interest with the attainment of particular goals. Throughout his life, Hayek considers Keynes as the most dangerous champion of the idea that 'we can hope to shape events at will by controlling money', and that we can do 'more or less what we please with the economic system by playing on the monetary instrument' (Hayek 1941:407–10; 1932c:126–9). In short, Keynes is prey of a 'fatal conceit' (Hayek 1988a), and he makes the mistake of defending public intervention because he is not aware of the real limits of human reason.

An identical error is at the basis of a monetary policy which restricts money to the role of 'a passive mediator', with the aim of eliminating those that according to Hayek's theory are 'the most important influences from the money side…which prevent the automatic adjustment of the economy to changes in external conditions' (Hayek 1928:103).

In this regard, Hayek formulates at least three ways to show that this policy is 'undesirable'. Before the debate with Keynes and Sraffa, he emphasises the positive influence that the existence of money has had on the development of modern economies;[14] this circumstance seems to him already sufficient to consider with great cautiousness the aim of fixing the quantity of the means of exchange. He also states that in the real world the stability of the total amount of bank deposits could be pursued as a policy goal only if it were possible to reduce the banks 'to the role of brokers, trading in savings', which is 'purely Utopian' (Hayek 1928:100–6; 1933a: 190–2). After the debate, he accepts and makes his own Sraffa's point that the presence of many long-term contracts and of rigidities in many prices generate 'very substantial frictional resistances' to the realisation of this monetary policy (Hayek 1933b:160–1; 1935a:106, 130–1).

The decisive consideration justifying Hayek's objection to a monetary policy which restricts money to the role of 'a passive mediator' is the observation that Hayek makes before the debate with Keynes and Sraffa and that he reiterates, undoubtedly spurred by Sraffa's aforementioned comments. He shifts his attention to the theoretical concept of neutral money, and he enumerates all the conditions that are postulated by the neutral money theory. On examination thereof, he concludes that no monetary policy can make these conditions real, in practice: 'they will never be given in the real world'.[15] In this way, he succeeds in radicalising the thesis that sets him against Keynes and the advocates of money management. In fact, Hayek can assert the impossibility of *any* monetary policy.

In synthesis, 'the only practical maxim for monetary policy' is 'the negative one that the simple fact of an increase of production and trade forms no justification for an expansion of credit'. Save in an acute crisis, bankers 'need not be afraid to harm production by overcaution' and central banks have to 'follow a cautious policy during the upward swing of the cycle, and so to mitigate the following depression, and to resist the well-meaning but dangerous proposals to fight depression by "a little inflation"'. Since in every moment the state of the economic system fundamentally depends on the degree of foresight of entrepreneurs, it is possible to intervene only with the goal of improving individuals' knowledge (Hayek 1935a:125).

This is a conclusion that Hayek has always upheld (Hayek 1933a:192). Moreover, this is a conclusion which is coherent both with the theory of knowledge that he elaborates starting from the 1930s and with the equilibrium analysis approach that he proposes in his early works.

Notes

1 This chapter was funded by the Ministero dell'università e della ricerca scientifica within the project 'Conceptual innovations in economics: a contextualized analysis of the contribution made by the Cambridge economists'.
2 See for instance Hayek (1933a:22; 1935c:127–8; 1941:406–10).
3 Myrdal (1933), Hayek (1935b:155–6), Foss (1995).
4 The growing attention for Mises' subjectivism as well as for the Misesian contribution to the problem of calculation in a socialist economy is accompanied by a clear detachment from Mises' 'pure logic of choice'. See Hayek (1937:33, 47–8; 1977:158; 1979:204–5 note 51; 1981:142; 1992:55–6; 1994:72–3). See Klein (1992:10–12 text and note) for a survey of the debate as to when Hayek decisively broke with Mises' approach to economics.
5 The definition in *The Pure Theory of Capital* of monetary causes as special cases can hardly be defined as novel. See for instance Hayek (1933a:147–8, 168, 182–7; 1935a:105–28).
6 According to Zappia, Hayek 'explicitly rejected at the beginning of the 1930s' the possibility 'that cyclical disequilibrium…may be induced by causes other than monetary ones'.
7 An element, which seems important, especially for its economic policy consequences, and which is present in Hayek (1941:346–7) and in Hayek (1932b:138) is the specification of the circumstances capable of producing an abrupt, violent and unforeseen change in the rate of saving. Hayek affirms that this is the result of 'a change in the distribution of incomes brought about either by a change in the external data or by the action of the Government or of monopolistic group'. He continues that the most common and significant case of such change is an income redistribution unfavourable to saving and hence to investment. In particular, this happens as a consequence of a monopolistic extortion carried out by combinations of labour which obtain a rise of wages. The action of the government generates capital consumption via the property levies and the estate duties, or in general via 'all taxes paid out of the substance of capital' and via 'any redirection of capital towards less profitable purposes such as e.g. subsidising particular branches of industry, or public works, or something similar'. Hayek puts forward these observations in stark contrast with the 'underconsumptionist' view, in which he inserts Keynes. Of course, capital consumption occurs because 'the compulsory transfer of income from capitalists to other classes' (or 'from saving to non-saving classes') is accompanied by changes in money expenditure.
8 See McCloughry (1984:viii), Caldwell (1995:43).
9 Hayek (1932a:211; 1935c:129–31; 1933b). Hayek (1941:30–1 text and note 1) admits his responsability and he apologises for contributing to the confusion between the concept of neutral money, which he considers a useful theoretical expedient to understand the structure and the functioning of the economic system, and the policy of neutral money, which he instead rejects even as an ideal reference rule.
10 The distinction between the concept of neutral money and the policy of neutral money on the one hand, and the related 'practical and theoretical difficulties' on the other hand, are already clearly set out in Hayek (1925:21–3; 1928:95–106; 1935a:115–25).
11 Hayek puts forward the argument to support these theses in Hayek (1928:95–106; 1933a:148–76).
12 See Hayek (1941:29, 357–62) but also, for example, Hayek (1928:83).
13 See the preface to Hayek (1933a:23). See also Hayek (1928:95–100).
14 See Hayek (1925:21; 1928:103; 1935a:110–12).
15 See Hayek (1935a:115–25); Caldwell (1995:16–17, 30–1).

Bibliography

Abraham-Frois, G. (1991) 'Corporate behaviour, valuation ratio and macroeconomic analysis', in E.Nell and W.Semmler (eds) *Nicholas Kaldor and Mainstream Economics: Confrontation or Convergence?* London: Macmillan.

Ackley, G. (1961) *Macroeconomic Theory,* New York: Macmillan.

Agosti, A. and Bravo, G.M. (eds) (1979) *Storia del movimento operaio, del socialismo e delle lotte sociali in Piemonte,* Bari: De Donate.

Amendola, G. (1976) *Intervista sull'antifascismo,* Bari: Laterza.

Antonicelli, F. (1961) 'Un professore antifascista: Umberto Cosmo', in F.Antonicelli (ed.) *Dall'antifascismo all Resistenza. Tren'anni di storia italiana (1915–45). Lezioni con testimonianze,* Torino: Einaudi.

Asimakopulos, A. (1991) *Keynes's General Theory and Accumulation,* Cambridge: Cambridge University Press.

Babbage, C. (1993) [1832], *On the Economy of Machinery and Manufactures,* London: Routledge.

Backhouse, R. (1995) 'Comment', in F.Moseley (ed.) *Heterodox Economic Theories. True or False?* Aldershot: Edward Elgar.

Bharadwaj, K. (1988) 'Sraffa's Ricardo', *Cambridge Journal of Economics* 12:67–84.

Bharadwaj, K. and Schefold, B. (eds) (1990) *Critical perspectives of the revival of the classical theory,* London: Routledge.

Baranzini, M. (ed.) (1982) *Advances in Economic, Theory,* Oxford: Blackwell.

Baranzini, M. and Harcourt, G.C. (eds) (1993) *The Dynamics of the Wealth of Nations. Growth, Distribution and Structural Change. Essays in Honour ofLuigi Pasinetti,* New York: St Martin Press.

Barber, W.J. (ed.) (1991) *Themes in Keynesian Criticism and Supplementary Modern Topics,* Aldershot: Edward Elgar.

Barone, E. (1894) 'Sul trattamento di quistioni dinamiche', *Giornale degli Economisti* 9:407–35.

Becattini, G. (1961) *Il concetto di industria e la teoria del valore,* Torino: Boringhieri.

––(1975) 'Introduzione', in A.Marshall and M.P.Marshall, *Economia della produzione,* Milano: ISEDI.

––(1986) 'L'interpretazione sraffiana di Marshall', in R.Bellofiore (ed.) *Tra teoria economica e grande cultura europea: Piero Sraffa,* Milano: Angeli.

––(1990) 'Alfred Marshall e la vecchia scuola economica di Cambridge', in G. Becattini (ed.) *Il pensiero economico: temi, problemi e scuole,* Torino: UTET

Bellofiore, R. (ed.) (1986) *Tra teoria economica e grande cultura europea: Piero Sraffa,* Milano: Angeli.

Bellofiore, R. and Potier, J.P. (1998) 'Piero Sraffa: nuovi elementi sulla biografia e sulla ricezione di *Produzione di Merci* in Italia', *Il Pensiero Economico Italiano* 6: 55–103.

Bergami, G. (1993) 'Intellettuali e formazione del consenso nel socialismo torinese: 1891–1920', *Studi Piemontesi* XXII:45–65.

Bermani, C. (ed.) (1987) *Gramsci raccontato,* Roma: Edizioni associate.

Bernabei, A. (1997) *Esuli ed emigrati italiani nel Regno Unito, 1920–1940,* Milano: Mursia.

Berta, G. (1986) 'Un governo democratico del lavoro? Luigi Einaudi e le relazioni industriali,' in V.Castronovo (1986) *La cassetta degli strumenti,* Milano: Angeli.

Bharadwaj, K. (1978) 'Classical political economy and the rise to dominance of supply and demand theories', *Dutt Lectures on Political Economy,* Calcutta: Orient Longman.

––(1984) 'Piero Sraffa: The man and the scholar–a tribute', *Economic and Political Weekly,* reprinted in K.Bharadwaj (1989) *Themes in Value and Distribution. Classical Theory Reapprised,* London: Unwin Hyman.

––(1988) 'Sraffa's Ricardo', *Cambridge Journal of Economics* 12:67–84.

––(1989) *Themes in Value and Distribution. Classical Theory Reapprised,* London: Unwin Hyman.

Bharadwaj, K. and Schefold, B. (eds) (1990) *Essays on Sraffa: Critical Perspectives on the Revival of Classical Theory,* London: Unwin Hyman.

Blaug, M. (1968) *Economic Theory in Retrospect,* Homewood, Illinois: Irwin.

––(1975) *The Cambridge Revolution: Success or Failure?,* London: Institute of Economic Affairs.

––(1991) 'Afterword', in N.de Marchi and M.Blaug (eds) *Appraising Modern Economics. Studies in the Methodology of Scientific Programs,* Aldershot: Edward Elgar.

––(1995) 'Reply', in F.Moseley (ed.) *Heterodox Economic Theories. True or False?,* Aldershot: Edward Elgar.

Bliss, C.J. (1975) *Capital Theory and the Distribution of Income,* Amsterdam: North-Holland.

Blitch, C.P (1983a) 'Allyn Young on increasing returns', *Journal of Post Keynesian Economics* 53:359–72.

––(1983b) 'Allyn A.Young: a curious case of professional neglect', *History of Political Economy* 15:1–24.

––(1990) 'Kaldor's lecture notes from Allyn Young's London School of Economics class, 1928–29', *History of Political Economy* 22:443–63.

Bobbio, N. (1986) *Italia civile. Ritratti e testimonianze,* Firenze: Passigli.

Boffito, C. (1973) *Teoria delta moneta,* Torino: Einaudi.

Bonifati, G. (1991) *Saggio dell'interesse e distribuzione del reddito,* Torino: Rosemberg & Sellier.

Brescacin, P.A. (1991) *Umberto Cosmo e la pratica della libertà,* Susegana: Arti Grafiche Conegliano.

Bresso, P. (1996) 'Dal riformismo al liberalismo. I primi quindici anni del Laboratorio di Economia Politica', *Quaderni di Storia dell'Università di Torino* 1:57–85.

Brown, M., Sato, K. and Zarembka, P. (eds) (1976) *Essays in Modern Capital Theory,* Amsterdam: North Holland.

Brunner, K. and Meltzer, A. (eds) (1977) *Stabilisation of the Domestic and International Economy,* Amsterdam: New Holland.

Bulferetti, L. (1951) *Le ideologie socialistiche in Italia nell'età del positivismo (1870–1922),* Firenze: Le Monnier.

Butos, W.N. (1985) 'Hayek and general equilibrium analysis', *Southern Economic Journal* 52:332–43.

Caldwell, B. (1988) 'Hayek's tranformation', *History of Political Economy* 20:513–42.

––(1995) 'Introduction', *The Collected Works of F.A.Hayek,* vol. 9, *Contra Keynes and Cambridge,* London: Routledge.

Cannan, E. (1893) *A History of the Theories of Production and Distribution in English Political Economy from 1776 to 1848,* London: King and Son.

––(1929) *A Review of Economic Theory,* London: King and Son.

Carabelli, A. (1988) *On Keynes's Method,* London: Macmillan.

––(1992) 'Organic interdependence and the choice of units in the *General Theory*', in B.Gerrard and J.V.Hillard (eds) *The Philosophy and Economics of J.M.Keynes,* Aldershot: Edward Elgar.

Caravale, G. (ed.) (1997) *Equilibrium and Economic Theory,* London: Routledge.

Cattini, M., Decleva, M., De Maddalena, E., Romani, M.A. (eds) (1997) *Storia di una libera università: L'Università commerciale Luigi Bocconi dal 1915 al 1945,* Milano: Egea.

Cavalieri, D. (1984) 'Keynes e Sraffa. Premesse, sviluppi ed esiti della *Critica di Cambridge*', in *Keynes in Italia,* Proceedings of the Florence Convention held on 4–5 June 1983, Annali dell'economia italiana, Torino: IPSOA.

––(1991) 'Marshall, l'economia classica e la "logica della separazione": riconsiderazione di un percorso teorico', *Quaderni di storia dell'economia politica* 9:147–80.

Chamberlin, E.H. (1929) 'Duopoly: value where sellers are few', *Quarterly Journal of Economics* 44:63–100.

––(1933) *The Theory of Monopolistic Competition,* Cambridge, Mass.: Harvard University Press.

Chick, V. (1983) *Macroeconomics after Keynes,* London: Philip Allan.

Chini, M. (1923) *Corso speciale di Matematiche con numerose applicazioni ad uso principalmente dei chimici e dei naturalisti,* Livorno: Raffaello Giusti Editore-Libraio-Tipografo.

Chipman, J.S. (1970) 'External economies of scale and competitive equilibrium', *Quarterly Journal of Economics* 85:347–85.

Chrystal, G. (1889), *Algebra,* Edinburgh: A & C Black.

Ciocca, P. and Nardozzi, G. (1993) *Lalto prezzo del danaro,* Roma: Laterza (English edn: *The High Price of Money,* Oxford: Oxford University Press, 1994).

Ciocca, P. and Rinaldi, R. (1997) 'L'inflazione in Italia, 1914–20. Considerazioni a margine della tesi di laurea di Piero Sraffa', *Rivista di Storia Economica* 13: 3–40.

Clapham, J.H. (1922a) 'Of empty economic boxes', *Economic Journal* 32:305–14.

––(1922b) 'The economic boxes. A rejoinder', *Economic Journal* 32:560–63.

Clark, J.B. (1899) *The Distribution of Wealth,* New York: Macmillan Company.

Clark, J.M. (1923) *Studies in the Economics of Overhead Costs,* Chicago: University of Chicago Press.

Coates, J. (1997) 'Keynes, vague concepts and fuzzy logic', in G.C.Harcourt and P.A.Riach (eds) *A 'Second Edition' of The General Theory,* vol. 2, London: Routledge.

Collard, D.A. (1990) 'Cambridge after Marshall', in J.K.Whitaker (ed.) *Centenary Essays of Alfred Marshall,* Cambridge: Cambridge University Press.

Colonna, M. (1994) 'Hayek's trade cycle theory and his contemporary critics', in M. Colonna and H.Hagemann (eds) *Money and Business Cycles. The Economics of F.A. Hayek,* vol. I, Aldershot: Edward Elgar.

Colonna, M. and Hagemann, H. (eds) (1994) *Money and Business Cycles. The Economics of F.A.Hayek,* 2 vols, Aldershot: Edward Elgar.

Colonna, M., Hagemann, H. and Hamouda.O.F. (eds) *Capitalism, Socialism and Knowledge. The Economics of F.A.Hayek,* 2 vols, Aldershot: Edward Elgar.

Corsi, M. (1991) *Division of Labour, Technical Change and Economic Growth,* Aldershot: Avebury.

Cottrell, A. (1994) 'Hayek's early cycle theory re-examined', *Cambridge Journal of Economics* 18:197–212.

d'Orsi, A. (1997–8) 'Cultura accademica e cultura militante. Un itinerario fra docenti e allievi delle facoltà umanistiche', *Quaderni di Storia dell'Università di Torino* 2–3:3–52.

––(ed.) (2000) *La vita degli studi. Carteggio Gioele Solari–Norberto Bobbio (1931– 1952),* Milano: Angeli.

Dardi, M. (1979) 'Indicizzazione dei salari e sistema monetario: un problema di teoria', *Giornale degli Economisti* 38 (new series): 457–69.

Davis, John (1993) 'Sraffa, independence and demand: the Gramscian influence', *Review of Political Economy* 5:29–36.

––(ed.) (1994) *The State of Interpretation of Keynes,* Norwell: Kluwer.

––(1998a) 'Sraffa and Keynes: differences and shared perceptions', *Il Pensiero Economico Italiano* 6:133–54.

––(1998b) 'Sraffa's early philosophical thinking', *Review of Political Economy* 10: 477–91.

De Cecco, M. (1993) 'Piero Sraffa's "Monetary inflation in Italy during and after the war": an introduction', *Cambridge Journal of Economics* 17:1–5.

––(1994) 'Introduzione a Piero Sraffa, "L'inflazione monetaria in Italia durante e dopo la guerra"', *Economia Politica* 11:155–62.

Delorme, R. and Dopfer, K. (eds) (1994) *The Economics of Diversity,* Aldershot: Edward Elgar.

De Marchi, N. and Blaug, M. (eds) (1991) *Appraising Modern Economics. Studies in the Methodology of Scientific Programs,* Aldershot: Edward Elgar.

De Vecchi, N. (1982) 'Capitale', in G.Lunghini (ed.) *Dizionario di Economia Politica,* Torino: Boringhieri.

De Vecchi, N. and Marcuzzo, M.C. (eds) (1998) *A cinquant'anni da Keynes: teorie dell'occupazione, interesse e crescita,* Milano: Unicopli.

De Vivo, G. (1994) '(Mis)interpreting Ricardo', *Contributions to Political Economy* 13: 29–43.

––(1996) 'Piero Sraffa and the Mill-Ricardo papers: a comment', *Cambridge Journal of Economics* 20:637–9.

––(1998) 'On the path to *Production of Commodities by Means of Commodities*', unpublished paper prepared for the conference 'Piero Sraffa's Work and Personality. Contributions in the Centenary of his Birth', Roma, 14–16 December 1998.

Desai, M. (1982) 'The task of monetary theory. The Hayek-Sraffa debate in a modern perspective', in M.Baranzini (ed.) *Advances in Economic Theory,* Oxford: Blackwell.

Dobb, M. (1978) 'Random biographical notes', *Cambridge Journal of Economics* 2: 115–20.

Donzelli, F, (1988), 'Introduzione' to F.A.Hayek, *Conoscenza, mercato, pianificazione,* Bologna: Il Mulino.

Dorigo Martinotti, S. (1997–8) 'Il professore e i suoi libri. Note sulla biblioteca di Luigi Einaudi', *Quaderni di Storia dell'Università di Torino* 2–3:539–49.

Dostaler, G. (1991) 'The debate between Hayek and Keynes', in W.J.Barber (ed.) *Themes in Keynesian Criticism and Supplementary Modern Topics,* Aldershot: Edward Elgar.

Dow, S. and Hillard, J. (eds) (1995) *Keynes, Knowledge and Uncertainty,* Aldershot: Edward Elgar.

Downie, J. (1958) *The Competitive Process,* London: Duckworth.

Duménil, G. (1980) *De la Valeur aux Prix de Production,* Parigi: Economica.

Eaton, J. (1960) 'Il modello di Sraffa e la teoria del valore-lavoro', *Società* 16:5.

Eatwell, J. (1987) 'Own-rates of interest', in J.Eatwell, M.Milgate and P.Newman (eds) *The New Palgrave. A Dictionary of Economics,* vol. 1, London: Macmillan.

Eatwell, J. and Panico, C. (1987) 'Sraffa, Hero', in J.Eatwell, M.Milgate and P. Newman (eds) *The New Palgrave. A Dictionary of Economics,* vol. 4, London: Macmillan.

Eatwell, J., Milgate, M. and Newman, P. (eds) (1987) *The New Palgrave. A Dictionary of Economics,* 4 vols, London: Macmillan.

––(eds) (1990) *Capital Theory,* New York and London: Norton.

Edgeworth, F.Y. (1897) 'La teoria pura del monopolio', *Giornale degli Economists* 15 (serie II): 13–31, 307–20, 405–14.

––(1905) 'Review of *A Geometrical Political Economy* by H.Cunynghame', *Economic Journal* 7:62–71 (reprinted in F.Y.Edgeworth (1925) *Papers Relating to Political Economy,* vol. 3:136–43, London: Macmillan).

––(1925) 'Review to Pigou's *Economics of Welfare*', *Economic Journal* 35:30–9.

Einaudi, L. (1901) 'Salvatore Cognetti de Martiis', *Giornale degli Economists* 12–13: 15–22.

––(1921) *Gli ideali di un economista,* Firenze: La Voce.

––(1929) 'James Pennington or James Mill: an early correction of Ricardo', *Quarterly Journal of Economics* 44:164–71.

––(1932) *Bibliografia di Achille Loria.* A supplement of *La Riforma Sociale* vol. 43.

––(1946) 'Achille Loria (1857–1946)', *Economic Journal* 56:147–50.

––(1950) 'Mauro Fasiani', *Rivista di diritto finanziario e scienza delle finanze* 9: 199–201.

––(1951) 'Dalla leggenda al monumento', *Giornale degli Economisti* 10:329–34 .

––(1953) 'Di una nuova edizione delle opere di Davide Ricardo', in *Rendiconti della Accademia Nazionale dei Lincei,* vol. 8.

––(1961–73) *Cronache economiche e politiche di un trentennio,* 6 vols, Torino: Einaudi.

Fabbrini, L. (1950) 'La teoria del capitale e dell'interesse di F.A.Hayek', *Rivista Internazionale di Scienze Sociali* 22:250–86.

Faucci, R. (1982) 'Cognetti de' Martiis', in *Dizionario Biografico degli Italiani,* vol. 26, Roma: Istituto dell'Enciclopedia Italiana.

––(1986) 'Fra Einaudi e Gramsci: Sraffa e la cultura italiana degli anni venti', in R.Bellofiore (ed.) *Piero Sraffa: tra Teoria Economica e Grande Cultura Europea,* Milano: Angeli.

Fausti, L. (1998) *Intellettuali in dialogo. Antonio Gramsci e Piero Sraffa,* Brescia-Celleno: Fondazione G.Piccini-La Piccola.

Firpo, L. (1975) 'Luigi Einaudi bibliofilo', *Commemorazione di Luigi Einaudi nel centenario della nascita (1874–1974),* Torino: Fondazione Luigi Einaudi.

––(1983) *Gente di Piemonte,* Milano: Mursia.

Fisher, I. (1896) *Appreciation and Interest,* American Economic Association Publications, 11.

––(1907) *The Rate of Interest,* New York: Macmillan.

Foley, D.K. (1982) 'The value of money, the value of labor power and the Marxian transformation problem', *Review of Radical Political Economics* 14:37–47.

––(1986) *Understanding Capital: Marx's Economic Theory,* Cambridge, Mass.: Harvard University Press.

Forget, E. and Peart, S. (2000) *Reflections on the Classical Canon: Essays in Honour of Samuel Hollander,* London: Routledge.

Foss, N.J. (1995) 'More on "Hayek's transformation"', *History of Political Economy* 27: 345–64.

––(1996) 'More on Hayek's transformation', *History of Political Economy* 28:345–64.

Frisch, R. (1950) 'Alfred Marshall's theory of value', *Quarterly Journal of Economics* 65: 495–524.

Gallegati, M. (1990) 'The spread of Marshall's economics in Italy, 1885–1925', in R. McWilliams and R.Tullberg (eds) *Alfred Marshall in Retrospect,* Aldershot: Edward Elgar.

Garegnani, P. (1960) *Il capitale nelle teorie della distribuzione,* Milano: Giuffrè.

––(1964–5) 'Note su consumi, investimenti e domanda effettiva', *Economia Internazionale*. Reprinted in P.Garegnani (1978–9) 'Notes on consumption, investment and effective demand I & II', *Cambridge Journal of Economics* 2: 335–53; 3:63–82.

––(1976) 'On a change in the notion of equilibrium in recent work on value', in M.Brown, K.Sato and P.Zarembka (eds) *Essays in Modern Capital Theory,* Amsterdam: North Holland.

––(1979) *Valore e domanda effettiva,* Torino: Einaudi.

––(1982) 'On Hollander's interpretation of Ricardo's early theory of profit', *Cambridge Journal of Economics* 6:65–77.

––(1987) 'Surplus approach to value and distribution', in J.Eatwell, M.Milgate and P.Newman (eds) *The New Palgrave. A Dictionary of Economics,* vol. 4, London: Macmillan.

––(1988), 'Actual and normal magnitudes: a comment on Asimakopulos', *Political Economy* 4:251–8.

––(1990a) 'Sraffa: Classical versus marginalist analysis', and 'Prof. Samuelson on Sraffa: A comment', in K.Bharadwaj and B.Schefold (eds) *Essays on Piero Sraffa. Critical Perspectives on the Revival of Classical Theory,* London: Unwin Hyman.

––(1990b) 'Quantity of capital', in J.Eatwell, M.Milgate and P.Newman (eds) *Capital Theory,* New York and London: Norton.

––(1997) 'On some supposed obstacles to the tendency of market prices towards natural prices', in G.Caravale (ed.) *Equilibrium and Economic Theory,* London: Routledge.

––(1998a) 'Sui manoscritti di Piero Sraffa', *Rivista italiana degli economisti* 3, 1: 151–5.

––(1998b) 'Sraffa: the theoretical world of the "old classical economists"', *European Journal of the History of Economic Thought* 5:415–29.

––(2000) 'Savings, investment and capital in a system of general intertemporal equilibrium', in H.D.Kurz (ed.) *Critical Essays on Piero Sraffa's Legacy in Economics,* Cambridge: Cambridge University Press.

Gerrard, B. and Hillard, J.V. (eds) (1982) *The Philosophy of Economics of J.M.Keynes,* Aldershot: Edward Elgar.

Giammanco, M.D. (1998) 'Scarcity of resources in a Sraffian framework of international trade', *Metroeconomica* 49:301–18.

Gilibert, G. (1999) 'Gramsci, Sraffa e il secondo libro del *Capitale*', Trieste: Mimeo.

Ginzburg, A. (1986) 'Sraffa e Keynes su inflazione e deflazione', in R.Bellofiore (ed.) *Tra teoria economica e grande cultura: Piero Sraffa,* Milano: Angeli.

Giva, D. (1986) 'Economisti e istituzioni. *La Riforma Sociale* 1894–1914', in V. Castronovo (ed.) *La cassetta degli strumenti: ideologie e modelli sociali nell'industrialismo italiano,* Milano: Angeli.

Goetz, H. (1983) *Der freie Geist und seine Widersacher. Die Eidverweigerer an den italieniscen Universitaten im Jahre 1931,* Frankfurt a.M.: Haag-Herchen.

Gramsci, A. (1965) *Lettere dal carcere,* edited by S.Caprioglio and E.Fubini, Torino: Einaudi.

––(1971) *La costruzione del Partito Comunista 1923–26,* Torino: Einaudi.

––(1975) *Quaderni del carcere,* Torino: Einaudi.

––(1978) *Selections from Political Writings 1921–1926,* edited by Q.Hoare, London: Lawrence and Wishart.

––(1980) *Cronache torinesi 1913–1917,* edited by S.Caprioglio, Torino: Einaudi.

––(1987) *L'Ordine Nuovo, 1919–1920,* edited by V.Gerratana and A.A.Santucci, Torino: Einaudi.

––(1992) *Lettere 1908–1926,* edited by A.Santucci, Torino: Einaudi.

Gramsci, A. and Schucht, T. (1997) *Lettere (1926–1935),* edited by A.Natoli and C. Daniele, Torino: Einaudi.

Grassl, W. and Smith, B. (eds) (1986) *Austrian Economics: History and Philosophical Background,* London: Croom Helm.

Graziani, A. (1986) 'La visione del processo capitalistico secondo Piero Sraffa', in R. Bellofiore (ed.) *Tra teoria economica e grande cultura europea: Piero Sraffa,* Milano: Angeli.

––(1998) 'A note on Hayek's macroeconomic equilibrium', in F.Michon (ed.) *L'économie. Une science pour l'homme et la societé. Mélanges en l'honneeur d'Henri Bartoli,* Paris: Publications de la Sorbonne.

Gregory, T.E. (1929) 'Obituary: Allyn A.Young', *Economic Journal* 39:297–301.

Groenewegen, P.D. (1991) 'Alfred Marshall and the history of economic thought', *Quaderni di storia dell'economia politica* 9:59–87.

––(1995) *A Soaring Eagle: Alfred Marshall 1842–1924,* Aldershot: Edward Elgar.

Groenewegen, P.D. and Halevi, J. (eds) (1987) *Altro Polo. Italian Economists Past and Present,* Sydney: University of Sydney.

Grosso, G. (1971) 'La Facoltà Giuridica dell'Università Torinese negli anni Venti', *Studi Piemontesi* 1:93–7

Hagemann, H. (1994) 'Hayek and the Kiel School: some reflections on the German debate on business cycles in the late 1920s and early 1930s', in M.Colonna and H.Hagemann (eds) *Money and Business Cycles. The Economics of F.A.Hayek,* vol. I,

Aldershot: Edward Elgar.

Hagemann, H. and Steiger, O. (eds) (1988) *Keynes's General Theory nach Funfzing Jahren,* Berlin: Duncker and Humblot

Hahn, F. (1982) 'The neo-Ricardians', *Cambridge Journal of Economics* 6:353–74.

––(1985) 'In praise of economic theory', in F.Hahn, *Money, Growth and Stability,* Oxford: Basil Blackwell.

Hahn, F. and Brechling, F.P.R. (eds) (1965) *The Theory of Interest Rates,* London: Macmillan.

Hamilton, W. (1870–4) *Lectures on Metaphysics and Logic,* 4 vols, ed. by H.L.Mansel and J.Veitch, Edinburgh: W.Blackwood & Sons.

Hamminga, B. and De Marchi, N. (eds) (1994) *Idealization VI: Idealization in Economics,* Amsterdam: Rodopi.

Hansen, A.H. (1953) *A Guide to Keynes,* New York: McGraw-Hill.

Harcourt, G.C. (1972) *Some Cambridge Controversies in the Theory of Capital,* Cambridge: Cambridge University Press.

––(1983) 'On Piero Sraffa's contribution to economics', in P.Groenewegen and J. Halevi (eds) *Altro Polo. Italian Economists Past and Present,* Sydney: University of Sydney.

––(ed.) (1985) *Keynes and his Contemporaries,* London: Macmillan.

––(1991) 'Marshall's principles as seen at Cambridge through the eyes of Gerald Shove, Dennis Robertson and Joan Robinson', *Quaderni di Storia dell'Economia* 9: 355–72.

Harcourt, G.C. and Riach, P.A. (eds) (1997) *A 'Second Edition' of the General Theory,* London: Routledge.

Harcourt, G.C., Roncaglia, A. and Rowley, R. (eds) (1995) *Income and Employment in Theory and Practice,* London: Macmillan.

Harrod, R.F. (1961) 'Production of commodities by means of commodities. Prelude to a critique of economic theory', *Economic Journal* 70:783–7.

Hart, N. (1996) 'Marshall's theory of value: the role of external economies', *Cambridge Journal of Economics* 20:353–69.

Hausman, D.M. (1981) *Capital, Profits and Prices: An Essay in the Philosophy of Economics,* New York: Columbia University Press.

––(1992) *The Inexact and Separate Science of Economics,* Cambridge: Cambridge University Press .

Hawtrey, R.G. (1937a) *Capital and Employment,* London: Longman.

––(1937b) 'Alternative theories of the rate of interest: III', *Economic Journal* 47: 437–43.

Hayek, F.A. (1925) 'Die Währungspolitik der Vereingten Staaten seit der Ueberwindung der Krise von 1920', *Zeitschrift für Volkswirtschaft und Sozialpolitik* N.F., V: 25–63, 254–317 (English translation: 'The monetary policy of the United States after the recovery from the 1920 crisis' in F.A.Hayek, *Money, Capital and Fluctuations. Early Essays,* London: Routledge, 1984).

––(1928) 'Das intertemporale Gleichgewichsystem der Preise und die Bewegungen des "Geldwertes"', in *Weltwirtschaftliches Archiv* 28/2:247–94 (English translation: 'Intertemporal price equilibrium and movements in the value of money' in F.A.Hayek, *Money, Capital and Fluctuations. Early Essays,* London: Routledge, 1984).

––(1931) *Preise und Production,* Wien: J.Springer (English translation: *Prices and Production,* London: G.Routledge & Sons).

——(1931/2), 'Reflections on the pure theory of money of Mr. J.M.Keynes', *Economica* 11:270–95; 12, 35:22–44 (reprinted in *The Collected Works of F.A. Hayek,* vol. IX, London: Routledge, 1995).

——(1932a) 'Money and capital: a reply', *Economic Journal* 42:237–49 (reprinted in *The Collected Works of F.A.Hayek,* vol. IX, London: Routledge, 1995).

——(1932b) 'Kapitalaufzehrung', *Weltwirtshaftlches Archiv* 36:86–108 (English translation: 'Capital consumption' in F.A.Hayek, *Money, Capital and Fluctuations. Early Essays,* London: Routledge, 1984).

——(1932c) 'Das Schicksal der Goldwährung', *Deutsche Volkswirt* 20/21:642–5, 677–81 (English translation: 'The fate of the gold standard' in F.A.Hayek, *Money, Capital and Fluctuations. Early Essays,* London: Routledge, 1984).

——(1933a) *Monetary Theory and the Trade Cycle,* London: Jonathan Cape.

——(1933b), 'Über "neutrales" Geld', *Zeitschrift für Nationalökonomie* 4:659–61 (English translation: 'On "neutral" money', in F.A.Hayek, *Money, Capital and Fluctuations. Early Essays,* London: Routledge, 1984).

——(ed.) (1933c) *Beiträge zur Geldtheorie,* Vienna: Springer.

——(1934) 'Saving', *Encyclopaedia of the Social Sciences,* vol. 13, New York: Macmillan (republished in F.A.Hayek, *Profit, Interest and Investment,* Clifton: Kelley, 1975).

——(1935a) *Prices and Production,* 2nd edn, London: Routledge & Kegan Paul.

——(1935b) 'Preiserwartungen, monetäre Störungen und Fehlinvestitionen', *Nationalokonomisk Tidsskrift* 73:176–91 (English translation: 'Price expectations, monetary disturbances and malinvestments', in F.A.Hayek. *Profit, Interest and Investment,* Clifton: Kelley, 1975).

——(1935c) 'The maintenance of capital', *Economica* (New Series) 2:241–76 (republished in F.A.Hayek, *Profit, Interest and Investment,* Clifton: Kelley, 1975).

——(1936) 'Technischer Fortschritt und Überkapazität', *Österreichisce Zeitschrift für Bankwesen,* 9–23 (English translation: 'Technical progress and excess capacity', in F.A.Hayek, *Money, Capital and Fluctuations. Early Esssays,* London: Routledge, 1984).

——(1937) 'Economics and knowledge', *Economica* 4:33–54 (reprinted in F.A. Hayek, *Individualism and Economic Order,* Chicago: University of Chicago Press, 1976).

——(1939) *Profits, Interest and Investment and Other Essays on the Theory of Industrial Fluctuations,* London: Routledge & Kegan Paul.

——(1941) *The Pure Theory of Capital,* Chicago: University of Chicago Press.

——(1945) 'Individualism: True and false', The Twelfth Finley Lecture Delivered at University College, Dublin, on December 17, 1945 (reprinted in F.A.Hayek, *Individualism and Economic Order,* Chicago: University of Chicago Press, 1976).

——(1948) 'The meaning of competition', in F.A.Hayek, *Individualism and Economic Order,* Chicago: The University of Chicago Press, 1970.

——(1968) 'Competition as a discovery procedure', in F.A.Hayek, *New Studies in Philosophy, Politics, Economics and the History of Ideas,* Chicago: University of Chicago Press, 1978.

——(1976) *Individualism and Economic Order,* Chicago: University of Chicago Press.

——(1977) *Forwort* (first edition: 1948) to L.von Mises, *Erinnerungen,* Stuttgart: fischer xi–xvi (English translation in *The Collected Works of F.A.Hayek,* vol. IV, London: Routledge).

——(1979) *Law, Legislation and Liberty: A New Statement of the Liberal Principles of Justice and Political Economy,* 3 vols, vol. 3, *The Political Order of a Free People,* London: Routledge.

––(1981) *Foreword* to L.von Mises, *Socialism: An Economic and Sociological Analysis,* Indianapolis: Liberty Classics (reprinted in *The Collected Works of F.A.Hayek,* vol. IV, London: Routledge).

––(1984) 'Intertemporal price equilibrium and movements in the value of money', in F.A.Hayek *Money, Capital and Fluctuations: Early Essays,* Chicago: University of Chicago Press.

––(1988a) *The Fatal Conceit. The Errors of Socialism. The Collected Works of F.A. Hayek,* vol. I edited by W.W.Bartley, London: Routledge.

––(1988b) *Conoscenza, mercato, pianificazione,* Bologna: II Mulino.

––(1992), 'Addendum to The Austrian School of Economics', in *The Collected Works of F.A.Hayek,* vol. IV, London: Routledge.

––(1994) *Hayek on Hayek,* edited by S.Kresge and L.Wenar, London: Routledge.

––(1995) *Contra Keynes and Cambridge: Essays, Correspondence, The Collected Works of F.A.Hayek,* vol. 9, London: Routledge.

Hempel, C.G. (1965) [1948] *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science,* New York: Free Press.

Hempel, C.G. and Oppenheim, P. (1948) 'Studies in the logic of explanation', *Philosophy of Science* (reprinted in C.G.Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science,* New York: Free Press).

Hey, J.D. and Winch, D. (eds) (1990) *A Century of Economics,* Oxford: Blackwell.

Hicks, J.R. (1939) *Value and Capital,* Oxford: Oxford University Press.

––(1967) 'The Hayek story', in J.R.Hicks, *Critical Essays in Monetary Theory,* Oxford: Clarendon Press.

Hirshleifer, J. (1985) 'The expanding domain of economies', *American Economic Review* 54:53–68.

Hodgson, G.M. (1993a) *Economics and Evolution: Bringing Life Back into Economics,* Cambridge: Polity Press.

––(1993b) 'The Mecca of Alfred Marshall', *Economic Journal* 103:406–15.

Hollander, S. (1979) *The Economics of David Ricardo,* Toronto: University of Toronto Press.

––(1998) 'Ricardo, Torrens and Sraffa: the untenability of de Vivo's "summing up"', *Cambridge Journal of Economics* 2:617–22.

Howson, S. (1985) 'Hawtrey and the real world', in G.C.Harcourt (ed.) *Keynes and his Contemporaries,* London: Macmillan.

Hutchison, T.H. (1953) *A Review of Economic Doctrines 1870–1929,* Oxford: Oxford University Press.

––(1981) *The Politics and Philosophy of Economics,* Oxford: Basil Blackwell.

Ingrao, B. and Ranchetti, F. (1996) *Il mercato nel pensiero economico,* Milano: Hoepli.

Jaffe, W. (ed.) (1965) *Correspondence of Leon Walras and Related Papers*. 3 vols, Amsterdam: North Holland.

Jenner, R.A. (1964) 'The dynamic factor in Marshall's economic system', *Western Economic Journal* 3:21–38.

Johnson, H.G. (1962) 'Review of *Production of Commodities by Means of Commodities*', *Canadian Journal of Economics and Political Science* 28:464–5.

Kahn, R. (1954) 'Some notes on liquidity preference', *Manchester School of Economics and Social Studies,* September: 229–57 (reprinted in R.Kahn, *Selected Essays on Employment and Growth*, Cambridge: Cambridge University Press, 1972).

——(1972) *Selected Essays on Employment and Growth,* Cambridge: Cambridge University Press.
——(1984) *The Making of Keynes' General Theory,* Cambridge: Cambridge University Press.
——(1989) *The Economics of the Short Period,* Houndmills and London: Macmillan.
Kaldor, N. (1934) 'The equilibrium of the firm', *Economic Journal* 44:60–76.
——(1939) 'Keynes's theory of the own-rates of interest: III (reprinted in N.Kaldor, *Essays on Economic Stability and Growth,* London: Duckworth, 1980).
——(1958) 'Monetary policy, economic stability and growth, memorandum submitted to the Radcliffe Committee on the Working of the Monetary System', 23 June, *Principal Memoranda of Evidence,* Cmnd 827, 1960, 146–53, London: HMSO.
——(1972) 'The irrelevance of equilibrium economic', *Economic Journal* 82:1237–55.
——(1980) *Essays on Economic Stability and Growth,* London: Duckworth.
——(1984) 'Obituary. Piero Sraffa', *The Cambridge Review,* 105 (2281):149–50.
——(1985) 'Piero Sraffa', *Proceedings of the British Academy* 71:615–40.
——(1990) 'Notes on Allyn Young's LSE lectures, 1927–9', *Journal of Economic Studies* 17, 3–4:18–114.
Kant, I. (1929) *Critique of Pure Reason,* London: Macmillan.
Keynes, J.M. (1923) *A Tract on Monetary Reform,* London: Macmillan (reprinted in *The Collected Writings of J.M.Keynes (CWK),* vol. IV).
——(1924) 'Alfred Marshall, 1842–1924', *Economic Journal* 34 (reprinted in *Essays in Biography, CWK,* vol. X).
——(1930) *A Treatise on Money,* 2 vols. London: Macmillan (reprinted in *CWK,* vol. V).
——(1931) 'The pure theory of money. A reply to Dr. Hayek', *Economica* 11:387–97 (reprinted in *The Collected Works of F.A.Hayek,* vol. IX, London: Routledge, 1995).
——(1933) *Essays in Biography,* London: Macmillan (reprinted in *CWK,* vol. X).
——(1936) *The General Theory of Employment, Interest and Money,* London: Macmillan (reprinted in *CWK,* vol. VII).
——(1973a) *The General Theory and After, Part I: Preparation, CWK,* vol. XIII.
——(1973b) *The General Theory and After, Part, II: Defence and Development, CWK,* vol. XIV.
——(1979) *The General Theory and After. A Supplement, CWK,* vol. XXIX.
——(1971–89) *The Collected Writings of J.M.Keynes (CWK),* edited by D.E. Moggridge, vols I–XXX, London: Macmillan.
Keynes, J.M. and Sraffa, P. (1938) 'Introduction' to D.Hume's *Abstract of the Treatise on Human Nature,* Cambridge: Cambridge University Press.
Keynes, J.N. (1891) *The Scope and Method of Political Economy,* London: Macmillan.
Klamer, A. (1989) 'A conversation with Amartya Sen', *Journal of Economic Perspectives* 3:135–50.
Klein, P.G. (1992) 'Introduction' to *The Collected Works of F.A.Hayek,* vol. IV, London: Routledge.
Knight, F.H. (1921a) *Risk, Uncertainty and Profit,* Boston and New York: Houghton Mifflin.
——(1921b) 'Cost of production and price over long and short period', *Journal of Political Economy* 29:304–35.
——(1924) 'Some fallacies in the interpretation of social cost', *Quarterly Journal of Economics* 39:582–604.

––(1925) 'On decreasing cost and comparative cost. A rejoinder', *Quarterly Journal of Economics* 40:331–3.

––(1928) 'A suggestion for simplifying the statement of the general theory of price', *Journal of Political Economy* 36:353–70.

Kregel, J.A. (1976) 'Economic methodology in the face of uncertainty: the modelling methods of Keynes and the Post-Keynesians', *Economic Journal* 86: 203–25.

––(1983) 'Effective demand: origins and development of the notion', in J.A. Kregel (ed.) *Distribution, Effective Demand and International Economic Relations,* London: Macmillan.

––(ed.) (1991) *Nuove interpretazioni dell'analisi monetaria di Keynes,* Bologna: Il Mulino.

Kuenne, R.E. (ed.) (1967) *Monopolistic Competition Theory: Studies in Impact,* New York: John Wiley & Sons.

Kuhn, T.S. (1962) *The Structure of Scientific Revolutions,* Chicago: University of Chicago Press.

Kurz, H.D. (1994) 'Review of *Interpreting Ricardo*', *European Journal of the History of Economic Thought* 1:411–20.

––(1995) 'Sulle perturbazioni "naturali" e "artificiali" dell'equilibrio economico generale. La teoria monetaria del sovrainvestimento di Friedrich August Hayek in *Prices and Production*', *Studi Economici* 56:5–61.

––(1998) 'Il contributo di Sraffa alla scienza economica. Note sui suoi scritti inediti', *Il Pensiero Economico Italiano* 6:11–38.

––(ed.) (2000) *Critical Essays on Piero Sraffa's Legacy in Economics,* Cambridge: Cambridge University Press.

Kurz, H.D. and Salvadori, N. (1993) 'The standard commodity and Ricardo's search for an invariable measure of value', in M.Baranzini and G.C.Harcourt (eds) *The Dynamics of the Wealth of Nations. Growth, Distribution and Structural Change. Essays in Honour ofLuigi Pasinetti,* New York: St Martin Press.

––(1995) *Theory of Production. A Long-Period Analysis,* Cambridge: Cambridge University Press.

––(1997) 'On critics and protective belt', in A.Salanti and E.Screpanti (eds) *Pluralism in Economics. New Perspectives in History and Methodology,* Aldershot: Edward Elgar.

Lachmann, L. (1986) 'Austrian economics under fire: the Hayek-Sraffa duel in retrospect', in W.Grassl and B.Smith (eds) *Austrian Economics: History and Philosophical Background,* London: Croom Helm.

Lange, O. (1938) 'The rate of interest and the optimum propensity to consume', *Economica* 5:12–32.

Larizza Lolli, M. (1979) 'L'antifascismo democratico: vicende, figure, dibattito', in A. Agosti and G.M.Bravo (eds) *Storia del movimento operaio, del socialismo e delle lotte sociali in Piemonte,* Bari: De Donato.

Lawlor, M.S. (1994) 'The own-rates framework as an interpretation of *The General Theory:* a suggestion for complicating the Keynesian theory of money', in J.Davis (ed.) *The State of Interpretation of Keynes,* Norwell: Kluwer.

Lawlor, M.S. and Horn, B. (1992) 'Notes on the Hayek-Sraffa exchange', *Review of Political Economy* 4:317–40.

Lenti, L. (1984) 'Gli ottant'anni della Bocconi', *Quaderni della Nuova Autologia* 21: 23–65.

Lerner A.P. (1952) 'The essential properties of interest and money', *Quarterly Journal of Economics* 67:173–93.

Limoges, C. and Ménard, C. (1994) 'Organization and the division of labor: biological metaphors at work in Alfred Marshall's *Principles of Economics*', in P.Mirowski (ed.) *Natural Images in Economic Thought,* Cambridge: Cambridge University Press.

Lindahl, E. (1939) *Studies in the Theory of Money and Capital,* London: Allen & Unwin.

Lipietz, A. (1982) 'The so-called "Transformation Problem" revisited,' *Journal of Economic Theory* 26:59–88.

Loasby, B.J. (1978) 'Whatever happened to Marshall's theory of value?', *Scottish Journal of Political Economy* 25:1–12.

––(1989) *The Mind and Method of the Economist,* Aldershot: Edward Elgar.

Lucas, R.E. Jr (1977) 'Understanding business cycles', in K.Brunner and A.Meltzer (eds) *Stabilisation of the Domestic and International Economy,* Amsterdam: North-Holland.

Lunghini, G. (1975) 'Teoria economica ed economia politica: Note su Sraffa', in G. Lunghini (ed.) *Produzione, capitale e distribuzione,* Milano: Isedi.

––(1996) 'Marx before Sraffa', in P.Zarembka and A.Sinha (eds) *Research in Political Economy. Latest Developments in Marxist Theory,* vol. 15, London: JAI Press.

Lutz, F.A. and Hague, D.C. (eds) (1961) *The Theory of Capital,* London: St Martin's Press.

Macchioro, A. (1991) 'Da Marx a Sraffa', *Mondo Operaio* 7:128–33.

McCloughry, R. (1982) 'Neutrality and monetary equilibrium', in M.Baranzini (ed.) *Advances in Economic Theory,* Oxford: Blackwell.

––(1984) 'Introduction' to F.A.Hayek, *Money, Capital and Fluctuations. Early Esssays,* edited by R.McCloughry, London: Routledge.

Machlup, F. (1955) 'The problem of verification in economies', *Southern Economic Journal* 22:1–21.

Machovec, F.M. (1995) *Perfect Competition and the Transformation of Economics,* London and New York: Routledge.

McWilliams, R. and Tullberg, R. (eds) (1990) *Alfred Marshall in Retrospect,* Aldershot: Edward Elgar.

Majewski, R. (1988) 'The Hayek challenge and the origins of Chapter 17 of Keynes' *General Theory*', in H.Hagemann and O.Steiger (eds) *Keynes' General Theory nach Funfzing Jahren,* Berlin: Duncker und Humblot.

Mäki, U. (1994) 'Isolation, idealization and truth in economies', in B.Hamminga and N.de Marchi (eds) *Idealization VI: Idealization in Economics,* Amsterdam: Rodopi.

––(1998) 'Kinds of assumptions and their truth. Shaking an untwisted F-twist'. Erasmus University Rotterdam, unpublished.

Maneschi, A. (1986) 'A comparative evaluation of Sraffa's "The laws of returns under competitive conditions" and its Italian precursor', *Cambridge Journal of Economics* 10, 1:1–12.

Marchionatti, R. (1992) 'Marshall on increasing returns and competition', *Quaderni di Storia dell'Economia Politica* 10:553–84.

Marchionatti, R. and Fiorini, R. (2000) 'Between Walras and Ricardo. Ladislaus von Bortkievicz and the origin of neo-Ricardian theory', *Revue européenne des sciences sociales. Cahiers Vilfredo Pareto* 38:173–91.

Marchionatti, R. and Gambino, E. (1997) 'Pareto and political economy as a science: methodological revolution and analytical advances in economic theory in the 1890s', *Journal of Political Economy* 105:1322–48.

Marcuzzo, M.C. (ed.) (1976) *Problemi e storie delle teorie economiche,* Milano: Mazzotta.

––(1996), 'Joan Robinson and Richard Kahn. The origin of short period analysis', in M.C.Marcuzzo, L.Pasinetti and A.Roncaglia (eds) *The Economics of Joan Robinson,* London: Routledge.

––(1998) 'La collaborazione tra Keynes e Kahn dal *Treatise* alla *General Theory*', in N.De Vecchi and M.C.Marcuzzo (eds) *A Cinquant'anni da Keynes: teorie dell' occupazione, interesse e crescita*, Milano: Unicopli.

Marcuzzo, M.C. and Rosselli, A. (1991) *Ricardo and the Gold Standard. The Foundations of the International Monetary Order,* London: Macmillan.

––(1994) 'Ricardo's theory of money matters', *Revue Economique* 45:1251–67.

Marcuzzo, M.C., Pasinetti, L. and Roncaglia, A. (eds) (1996) *The Economics of Joan Robinson,* London: Routledge.

Marshall, A, (with M.P.Marshall) (1879) *Economics of Industry,* London: Macmillan.

Marshall, A. (1925) [1885] 'The present position of economies', in A.C.Pigou (ed.) *Memorials of Alfred Marshall,* London: Macmillan.

––(1961) [1st edn 1890; eighth edn 1920] *Principles of Economics,* ninth variorum edition, with annotations by C.W.Guillebaud, 2 vols. London: Macmillan.

Marx, K. (1985) *Das Kapital. Kritik der Politischen Ökonomie,* Dritter Band, Berlin: Dietz Verlag.

Maxwell, J.A. (1958) 'Alfred Marshall and the competitive firm', *Economic Journal* 67: 673–98.

Meldolesi, L. (1982) *L'Utopia Realmente Esistente. Marx e Saint-Simon,* Bari: Laterza.

Melitz, J. (1965) 'Friedman and Machlup on the significance of testing economic assumptions', *Journal of Political Economy* 73:37–60.

Menger, C. (1934–6) *The Collected Works,* with an Introduction of F.A.Hayek, 4 vols, London: Routledge & Sons.

Milgate, M. (1977) 'Keynes on the "classical" theory of interest', *Cambridge Journal of Economics* 1:307–15.

––(1979) 'On the origin of the notion of "Intertemporal equilibrium"', *Economica* 44:1–10.

––(1982) *Capital and Employment,* Cambridge: Cambridge University Press.

Minsky, H.P. (1990) 'Sraffa and Keynes: effective demand in the long run', in K. Bharadwaj and B.Schefold (eds) *Essays on Piero Sraffa. Critical Perspectives on the Revival of Classical Theory,* London: Unwin Hyman.

Michon, F. (ed.) (1998) *L'économie. Une science pour l'homme et la société. Melanges en l'honneur d'Henry Bartoli,* Paris: Publications de la Sorbonne.

Mirowski, P. (ed.) (1994) *Natural Images in Economic Thought,* Cambridge: Cambridge University Press.

Modigliani, F. (1944) 'Liquidity preference and the theory of interest and money', *Econometrica* 12:45–88.

Moggridge, D. (1992) *Maynard Keynes: An Economist's Biography,* London: Routledge.

Mongiovi, G. (1990) 'Keynes, Hayek and Sraffa: on the origin of Chapter 17 of the *General Theory*', *Economie Appliquée* 42:131–56.

––(1994) 'Misinterpreting Ricardo: a review essay', *Journal of the History of Economic Thought* 16:248–69.

––(1996) 'Sraffa's critique of Marshall: a reassessment', *Cambridge Journal of Economics* 20:207–24.

Mongiovi, G. and Petri, F. (eds) (1999) *Value, Distribution and Capital: Essays in Honour ofPierangelo Garegnani,* London: Routledge.

Monk, R. (1990) *Ludwig Wittgenstein,* London: Jonathan Cape.

Moore, H.L. (1906) 'Paradoxes of competition', *Quarterly Journal of Economics* 20: 211–30.

Mosca, G. (1901–2) 'Salvatore Cognetti de Martiis', *Annuario della Regia Università di Torino,* Torino: Università di Torino.

Moseley, F. (1993) 'Marx's logical method and the transformation problem', in F. Moseley (ed.) *Marx's Method in Capital: A Reexamination,* Atlantic Highlands (N.J.): Humanities Press.

––(ed.) (1995) *Heterodox Economic Theories. True or False?,* Aldershot: Elgar.

Moss, L.S. (1994) 'Hayek and the several faces of socialism', in M.Colonna, H. Hagemann and O.F.Hamouda (eds) *Capitalism, Socialism and Knowledge,* Aldershot: Edward Elgar.

Moss, S.J. (1978) 'The post Keynesian theory of income distribution in the corporate economy', *Australian Economic Papers* 38:303–22.

Musella, M. and Panico, C. (1995) *The Money Supply in the Economic Process,* Aldershot: Elgar.

Musgrave, A.E. (1981) '"Unreal assumptions" in economic theory: the F-twist untwisted', *Kyklos* 34:377–87.

Myint, H. (1965) [1948] *Theories of Welfare Economics,* New York: A.M.Kelley.

Mvrdal, G. (1933) 'Die Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse', in F.A.Hayek (ed.) *Beiträge zur Geldtheorie,* Vienna: Springer.

Naldi, N. (1998a) 'Piero Sraffa a Perugia: novembre 1923-febbraio 1926', *Il Pensiero Economico Italiano* 6:105–31.

––(1998b) 'Some notes on Piero Sraffa's biography: 1917–1927', *Review of Political Economy* 10:493–515.

––(1998c) 'Dicembre 1922: Piero Sraffa e Benito Mussolini', *Rivista italiana degli economisti* 3, 2:271–99.

––(2000) 'Piero Sraffa and Antonio Gramsci: their friendship between 1919 and 1927', *European Journal of the History of Economic Thought* 7:79–114.

Napoleoni, C. (1964) "Premesse al concetto della produzione come processo circolare", *La Rivista Trimestrale* 3:174–7.

––(1976) *Valore,* Milano: ISEDI.

––(1989) 'La teoria del valore dopo Sraffa', in L.Pasinetti (ed.) *Aspetti controversy della teoria del valore,* Bologna: Il Mulino.

––(1992) 'Essay on the theory of production as a circular process', in L.L. Pasinetti (ed.) *Italian Economic Papers,* vol. I, Bologna/Oxford: Il Mulino/Oxford University Press.

Negishi, T. (1989) *History of Economic Theory,* Amsterdam: North Holland.

Nell, E.J. (1988) 'Does the rate of interest determine the rate of profit?', *Political Economy: Studies in the Surplus Approach,* 4:263–7.

Newman, P (1960) 'The erosion of Marshall's theory of value', *Quarterly Journal of Economics* 74:587–601.

Newman, P and Vassilakis, S. (1988) 'Sraffa and imperfect competition', *Cambridge Journal of Economics* 12:37–42.

Newman, P. and Wolfe, J.N. (1961) 'A model for the long-run theory of value', *Review of Economic Studies* 42:51–61.

O'Brien, D.P. and Prisley, J.R. (eds) (1981) *Pioneers of Modern Economics in Britain,* London: Macmillan.

O'Driscoll, G. (1977) *Economics as a Coordination Problem: the Contribution of Friedrich Hayek,* Kansas City, Kan.: Sheed Andrews and McMeel.

Panico, C. (1987) 'The evolution of Keynes's thought on the rate of interest', *Contributions to Political Economy* 6:53–61.

––(1988a) 'Sraffa on money and banking', *Cambridge Journal of Economics* 12: 7–28.

––(1988b) *Interest and Profit in the Theories of Value and Distribution,* London: Macmillan; New York: St Martin's Press.

––(1991) 'Some notes on Marshallian supply functions', *Economic Journal* 101: 557–69.

––(1993) 'Two alternative approaches to financial model building', *Metroeconomica* 44:99–133.

––(1997) 'Government deficits in the post-Keynesian theories of growth and distribution', *Contributions to Political Economy* 16:61–86.

––(1998) 'Le questioni monetarie negli scritti di Sraffa', *Il Pensiero Economico Italiano* 6:155–87.

––(1999) 'The government sector in the post Keynesian theory of growth and personal distribution', in G.Mongiovi and F.Petri (eds) *Value, Distribution and Capital: Essays in Honour of P.Garegnani,* London: Routledge.

Panico, C. and Salvadori, N. (1994) 'Sraffa, Marshall and the problem of returns', *European Journal of the History of Economic Thought* 1:323–43.

Pasinetti, L.L. (1974) *Growth and Income Distribution,* Cambridge: Cambridge University Press.

––(1979) 'Sraffa Piero', *International Encyclopaedia of Social Sciences, Biographical Supplement,* Vol. 18, London and New York: Macmillan.

––(1985a) *Due scritti bio-bibliografici su Piero Sraffa,* Bank of Italy Studies Service, *Term di discussione,* n. 50.

––(1985b) 'In memoria of Piero Sraffa: economista italiano a Cambridge', *Economia Politica* 2:315–32.

––(1988) 'Sraffa on income distribution', *Cambridge Journal of Economics* 12: 135–8.

––(ed.) (1989) *Aspetti controversi della teoria del valore,* Bologna: Il Mulino.

––(1990) 'Capital theory: paradoxes' (with R.Scazzieri), in J.Eatwell, J.Milgate and P.Newman (eds) *Capital Theory,* New York and London: Norton.

––(ed.) (1992) *Italian Economic Papers,* vol. I, Bologna/Oxford: Il Mulino/Oxford University Press.

––(1994) 'Economic theory and institutions', in R.Delorme and K.Dopfer (eds) *The Economics of Diversity,* Aldershot: Edward Elgar.

––(1997) 'The marginal efficiency of capital', in G.C.Harcourt and P.A.Riach (eds) *A 'Second Edition' of The General Theory,* London: Routledge.

––(ed.) (1998) *Italian Economic Papers,* vol. III, Bologna/Oxford: Il Mulino/Oxford University Press.

Patinkin, D. (1965) *Money, Interest, and Prices,* New York: Harper and Row.

Patinkin, D. and Leith, J.C. (eds) (1977) *Keynes, Cambridge and the General Theory,* London: Macmillan.

Paulesu Quercioli, M. (ed.) (1977) *Gramsci vivo nelle testimonianze dei suoi contemporanei,* Milano: Feltrinelli.

Peach, T. (1993) *Interpreting Ricardo,* Cambridge: Cambridge University Press.

––(1998) 'On *Interpreting Ricardo:* a reply to Sraffians', *Cambridge Journal of Economics* 22:597–616.

Pene Vidari, G.S. (1980) 'Cultura giuridica', in *Torino città viva. Da capitale a metropoli. 1880–1980. Cento anni di vita cittadina. Politica, economia, società, cultura,* vol. 2, Torino: Centro Studi Piemontesi.

Pigou, A.C. (1907) 'Review of the fifth edition of Marshall's *Principles of Economics*', *Economic Journal* 17:532–5.

––(1912) *Wealth and Welfare,* London: Macmillan.

––(1920) [2nd edn 1924] *The Economics of Welfare,* London: Macmillan.

––(1922) 'Empty economic boxes: A reply', *Economic Journal* 32:458–65.

––(ed.) (1925) *Memorials of Alfred Marshall,* London: Macmillan.

––(1927) 'The law of diminishing returns and increasing cost', *Economic Journal* 37:188–97.

––(1928) 'An analysis of supply', *Economic Journal* 38:238–57.

Pino Pongolini, F. (1995) 'Note sulla cultura bancaria a Milano nei primi anni' 20: Cabiati, Mattioli e la *Rivista bancaria*', *Rivista di storia economica* 12:1–55.

Pivetti, M. (1988) 'On the monetary explanation of distribution: a rejoinder to Nell and Wray', *Political Economy: Studies in the Surplus Approach* 4:275–83.

––(1991) *An Essay on Money and Distribution,* London: Macmillan.

Pogliano, C. (1979) *'Mondo accademico, intellettuali, professione sociale dall'Unità alla guerra mondiale',* in A.Agosti and G.M.Bravo (eds) *Storia del movimento operaio, del socialismo e delle lotte sociali,* vol. 1, Bari: De Donato.

––(1993) *Cognetti de Martiis. Le origini del Laboratorio di Economia Politica,* Torino: Università degli Studi di Torino–Dipartimento di Economia.

Pollitt, B.H. (1990) 'Clearing the path for *Production of commodities by means of commodities:* Notes on the collaboration of Maurice Dobb in Piero Sraffa's edition of the Works and Correspondence of David Ricardo', in K.Bharadwaj and B.Schefold (eds) *Critical Perspectives of the Revival of the Classical Theory,* London: Routledge.

Porta, P.L. (1992) 'Piero Sraffa, "superb editor"'. *Revue Européenne des Sciences Sociales, Cahiers Vilfredo Pareto* 30, 92:25–34.

––(1995) 'How Piero Sraffa found Ricardo's letters to Mill', *Cambridge Journal of Economics* 19:353–7.

––(1996) 'Piero Sraffa and the Mill-Ricardo papers: a rejoinder', *Cambridge Journal of Economics* 20:641–3.

Porta, P (2000) 'Sraffa's Ricardo after Fifty Years. A Preliminary Estimate', in S. Peart and E.Forget (eds) *Reflections on the Classical Canon in Economics. Essays in honor of Samuel Hollander,* London: Routledge.

Potestio, P (1986) 'Equilibrium and employment in the *General Theory*', *Giornale degli economist,* July-August: 363–88.

Potier, J.P. (1991) *Piero Sraffa. Unorthodox Economist (1898–1983),* London: Routledge.

Pownall, T. (1776) *A Letter from Governor Pownall to Adam Smith, L.L.D. F.R.S., being an Examination of Several Points of Doctrine, laid down in his 'Inquiry in to the Nature and Causes of the Wealth of Nations',* London (reprinted in A.Smith, *Correspondence,* ed. by E.C.Mossner and I.S.Ross, Oxford: Oxford University Press, 1977).

Pradella, P. (1915a) *Algebra ed Aritmetica: ad uso dei licei,* Turin: G.B.Paravia.

––(1915b) *Geometria: ad uso dei licei,* Turin: G.B.Paravia.

Prendergast, R. (1992) 'Increasing returns and competitive equilibrium–the content and development of Marshall's theory', *Cambridge Journal of Economics* 16:447–62.

––(1993) 'Marshallian external economies', *Economic Journal,* 103:454–8.

Pugh, P. (1987) *Educate, Agitate, Organize. 100 Years of Fabian Socialism,* London: Methuen.

Quinton, A. (1968) 'The later philosophy of Wittgenstein', in G.Pitcher (ed.) *Wittgenstein: A Collection of Critical Essays,* London: Macmillan.

Raffaelli, T. (1996) 'La formazione di Keynes nel laboratorio marshalliano', *Rivista Italiana degli Economists* 1:137–58.

Ragionieri, E. (ed.) (1967) *Gramsci,* Roma: Editori Riuniti.

Ranchetti, F. (1986) 'Mattioli "economista critico"', *Rivista di storia economica* 3: 229–36.

––(1996) 'Piero Sraffa', in F.Ranchetti and B.Ingrao, *Il mercato nel pensiero economico,* Milano: Hoepli.

––(1998) 'Sraffa e Keynes. Note per una critica della teoria keynesiana dell'interesse e della moneta', in N.De Vecchi and M.C.Marcuzzo (eds) *A cinquant'anni da Keynes. Teorie dell'occupazione, interesse e crescita,* Milano: Unicopli.

––(1999) 'Introduction' to P.Sraffa (1999) *Produzione di merci a mezzo di merci* (new edition), Torino: Einaudi.

Ricardo, D. (1951–73) *Works and Correspondence,* edited by P.Sraffa with the collaboration of M.Dobb, vols I–XI, Cambridge: Cambridge University Press.

Richardson, G.B. (1975) 'Adam Smith on competition and increasing returns', in A.S.Skinner and T.Wilson (eds) *Essays on Adam Smith,* Oxford: Oxford University Press.

Robbins, L. (1928) 'The representative firm', *Economic Journal* 38:287–304.

Robertson, D. (1924) 'Those economic boxes', *Economic Journal* 34:16–31.

––(1930) 'The trees of the forest', *Economic Journal* 40:80–9.

––(1936) 'Some notes on Mr. Keynes' general theory of employment', *Quarterly Journal of Economics* 51:168–91

––(1937) 'Alternative theories of the rate of interest: rejoinder. II', *Economic Journal* 47:428–36

––(1938) 'Mr. Keynes and "Finance"', *Economic Journal* 48:314–18.

––(1940) *Essays in Monetary Theory,* London: King.

––(1951–2) 'Interchange with H.Johnson', *Review of Economic Studies* 49:90–110.

Robinson, E.A.G. (1977) 'Keynes and his Cambridge colleagues', in D.Patinkin and J.C.Leith (eds) *Keynes, Cambridge and the General Theory,* London: Macmillan.

––(1990) 'Fifty years on the Royal Society Council', in J.D.Hey and D.Winch (eds) *A Century of Economics,* Oxford: Blackwell.

––(1994), 'Richard Kahn in the 1930s', *Cambridge Journal of Economics* 18:7–10.

Robinson, J.V. (1933) 'A parable on savings and investment', *Economica* 13:75–84.

––(1951) *Collected Economic Papers,* vol. I, Oxford: Blackwell.

––(1953) 'The production function and the theory of capital', *Review of Economic Studies,* 21:81–106.

––(1961) 'Prelude to a critique of economic theory', *Oxford Economic Papers,* 13: 53–8.

––(1969) [1933], *The Economics of Imperfect Competition,* 2nd edn, London: Macmillan.

––(1978) *Contributions to Modern Economics,* Oxford: Basil Blackwell.

Roemer, P.M. (1986) 'Increasing returns, specialisation and external economies: growth as described by Allyn Young', Working Paper no. 64, Rochester, New York.

——(1987) 'Growth based on increasing returns due to specialisation', *American Economic Review* 77:56–62.

Roll, E. (1973) *A History of Economic Thought,* London: Faber & Faber.

Romani, M.A. (1993) '«Contro Angelo Sraffa, il despota dell'Università Bocconi»', *Rivista delle società* 38:709–22.

——(1997) '"Bocconi über alles!": l'organizzazione didattica e la ricerca (1914–1945)', in M.Cattini, E.Decleva, A.De Maddalena and M.A.Romani (eds) *Storia di una libera università. L'Università Commerciale Luigi Bocconi dal 1915 al 1945,* Milano: Egea.

Roncaglia, A. (1980, 2nd edn) *Sraffa e la teoria dei prezzi,* Roma-Bari: Laterza (English translation: *Sraffa and the Theory of Prices,* London and New York: Wiley, 1978; 1981, 2nd edn).

——(1983) 'Piero Sraffa: una bibliografia ragionata', *Studi Economici* 38:137–66.

——(1984) 'Sraffa e le banche', *Rivista Milanese di Economia* 3:104–12.

——(1985) *Petty: The Origin of Political Economy,* Cardiff: University College Cardiff Press.

——(1990a) 'Le scuole sraffiane', in G.Beccatini (ed.) *Il pensiero economico: temi, problemi e scuole,* Torino: Utet.

——(1990b), 'Is the notion of long-period positions compatible with classical political economy?', *Political Economy* 6:103–11.

——(1991) 'Sraffa's 1925 article and Marshall's Theory', *Quaderni di Storia dell'Economia Politica* 9:373–97.

——(1993) 'Toward a post-Sraffian theory of income distribution, *Journal of Income Distribution,* 3:3–27.

——(1994) *Piero Sraffa,* Roma: Mimeo.

——(1995a) 'Introduzione', in A.Smith, *La ricchezza delle nazioni,* Roma: Newton Compton.

——(1995b) 'On the compatibility between Keynes's and Sraffa's viewpoints on output levels', in G.Harcourt, A.Roncaglia and R.Rowley (eds) *Income and Employment in Theory and Practice,* London: Macmillan.

——(1999) *Sraffa: la Biografia, l'Opera, le Scuole,* Bari: Laterza.

Runde, J. (1994) 'Keynesian uncertainty and liquidity preference', *Cambridge Journal of Economics* 18:129–44.

Salanti, A. (1997) 'Comment', in A.Salanti and E.Screpanti (eds) *Pluralism in Economics. New Perspectives in History and Methodology,* Aldershot: Edward Elgar.

Salanti, A. and Screpanti, E. (eds) (1997) *Pluralism in Economics. New Perspectives in History and Methodology,* Aldershot: Edward Elgar.

Salvadori, N. (1985) 'Switching in methods of production and joint production', *Manchester School* 53:156–78.

——(1998) 'Sraffa sulla "domanda": un'analisi testuale', *Il Pensiero Economico Italiano* 6:243–62.

Samuelson, P.A (1966) 'A summing up', *Quarterly Journal of Economics* 80:568–83.

——(1967) 'The monopolistic competition revolution', in R.E.Kuenne (ed.), *Monopolistic Competition Theory: Studies in Impact,* New York: John Wiley & Sons.

——(1987) 'Sraffian economies', in P.Newman, M.Milgate and J.L.Eatwell (eds) *The New Palgrave Dictionary of Economics,* vol. 4, London: Macmillan.

——(1990) 'Revisionists Findings on Sraffa' and 'Reply', in K.Bharadway and B. Schefold (eds) *Essays on Piero Sraffa: Critical Perspectives on the Revival of Classical Theory,* London: Unwin Hyman.

——(1991) 'Sraffa's other leg', *Economic Journal* 101:570–4.

Schiavone, L. (1993) 'Dati statistici e grafici sugli student!', in F.Traniello (ed.) *L'Università di Torino. Profilo storico e istituzionale,* Torino: Pluriverso.

Schumpeter, J.H. (1908) *Das Wesen und der Hauptinhalt der theoretischen National-okonomie,* Munchen and Leipzig: Duncker & Humblot

––(1912) *Theorie der wirtschaftlichen Entwicklung,* Berlin: Duncker & Humblot.

––(1928) 'The instability of capitalism', *Economic Journal* 38:361–86.

––(1954) *History of Economic Analysis,* New York: Oxford Univeristy Press.

Servadio, G. (1993) *Incontri,* Catanzaro: Abramo.

Shackle, G.L.S. (1967) *The Years of High Theory,* Cambridge: Cambridge University Press.

––(1981) 'F.A.Hayek, 1899-', in D.P.O'Brien and J.R.Presley (eds) *Pioneers of Modern Economics in Britain,* London: Macmillan.

Shove, G.F. (1930) 'The representative firm and increasing returns', *Economic Journal* 40:94–116.

Signorino, R. (1998) 'La critica di Piero Sraffa alla teoria marshalliana del valore negli anni venti. Una prospettiva metodologica', *Il Pensiero Economico Italiano* 6: 189–230.

Silberling, N.J. (1924) 'Graphical illustrations of the law of price', *American Economic Review* 14:417–42.

Skidelsky, R. (1986) 'Keynes e Sraffa: un caso of non-comunicazione', in R. Bellofiore (ed.) *Piero Sraffa: tra teoria economica e grande cultura europea,* Milano: Angeli.

––(1992) *John Maynard Keynes. The Economist as Saviour 1920–1937,* London: Macmillan.

Skinner, A.S. and Wilson, T. (eds) *Essays on Adam Smith,* London: Oxford University Press.

Smith, J. (1998) 'An archivist's apology: the papers of Piero Sraffa at Trinity College. Cambridge', *Il Pensiero Economico Italiano* 6:39–54.

Spriano, P. (1967) *Storia del Partito comunista Italiano,* vol. I, Torino: Einaudi.

––(1972) *Storia di Torino operaia e socialista. Da De Amicis a Gramsci,* Torino: Einaudi.

––(1977a) *Profilo di Antonio Gramsci,* in P.Spriano, *Gramsci e Gobetti. Introduzione alla vita e alle opere,* Torino: Einaudi.

––(1977b) *Gramsci e Gobetti. Introduzione alla vita e alle opere,* Torino: Einaudi.

Sraffa, P. (1920) *L'Inflazione Monetaria in Italia,* Milano: Premiata Scuola Tipografica Salesiana. Translated into English by G.C.Harcourt and C.Sardoni (1993), 'Piero Sraffa, monetary inflation in Italy during and after the war', *Cambridge Journal of Economics* 17:7–26.

––(1921a) 'Open shop drive', *L'Ordine Nuovo,* 5 Luglio.

––(1921b) 'Industriali e governo inglese contro i lavoratori', *L'Ordine Nuovo,* 24 Luglio.

––(1921c) 'I labour leaders', *L'Ordine Nuovo,* 4 Agosto.

––(1922a) 'The bank crisis in Italy', *Economic Journal* 32:178–97.

––(1922b) 'Italian banking to-day', *The Manchester Guardian Commercial, Reconstruction in Europe, Supplement,* 7 December: 675–6.

––(1923) 'Opinioni', *La Rivoluzione Liberale,* Torino, Anno II, numero 31, 16 ottobre, 128.

––(1924) 'Obituary–Maffeo Pentaleoni, *Economic Journal* 34:648–53.

––(1925a) 'Sulle relazioni tra costo e quantità prodotta', *Annali of Economia* 2: 277–328. Reprinted in Sraffa (1986). English translation by J.Eatwell and A. Roncaglia, 'On the relations between cost and quantity produced', in L.L. Pasinetti (ed.) (1998)

Italian Economic Papers, vol. III, Bologna: Il Mulino; Oxford: Oxford University Press, pp. 323–63.

––(1925b) 'Una breve recensione of H.B.Hastings, *Cost and profits: their relation to business cycles,* Boston: Houghton Mifflin, 1923', *Giornale degli Economisti* 66: 389–90.

––(1926a) 'The laws of returns under competitive conditions', *Economic Journal,* 36:535–50.

––(1926b) 'Una breve recensione di R.A.Lehfeldt, *Money,* London: Oxford University Press, 1926', *Giornale degli Economisti* 67:230.

––(1926c) 'Una breve recensione di M.Segre, *Le banche nell'ultimo decennio, con particolare riguardo al loro sviluppo patologico nel dopo guerra,* Milano: La Stampa Commerciale, 1926', *Giornale degli Economisti* 67:230.

––(1927a) 'Una breve recensione di H.W.Phillips, *Modern foreign exchange and foreign banking,* London: Macdonald and Evans, 1926', *Giornale degli Economisti* 68:610.

––(1927b) 'Politica monetaria. Due lettere sull'articolo di Angelo Tasca «La rivalutazione della lira e la crisi dell'economia italiana»', *Lo Stato Operaio,* I, 9–10: 1089–95.

––(1930a) 'A criticism–A rejoinder', *Economic Journal* 40:89–93.

––(1930b) 'An alleged correction of Ricardo', *Quarterly Journal of Economics* 44: 539–44.

––(1932a) 'Dr. Hayek on Money and Capital', *Economic Journal* 42:42–53 (reprinted in *The Collected Works of F.A.Hayek,* vol. 9, London: Routledge).

––(1932b) 'Rejoinder', *Economic Journal* 42:249–51 (reprinted in *The Collected Works of F.A.Hayek,* vol. 9, London: Routledge).

––(ed., with the collaboration of M.Dobb) (1951–5) *The Works and Correspondence of David Ricardo,* vols. I–X, Cambridge: Cambridge University Press (vol. XI, indexes, 1973).

––(1960a) *Production of Commodities by Means of Commodities. Prelude to a Critique of Economic Theory,* Cambridge: Cambridge University Press.

––(1960b) *Produzione di merci a mezzo di merci. Premesse a una critica della teoria economica,* Torino: Einaudi (1999 new edition edited by F.Ranchetti).

––(1961) 'Comment to Hicks's paper', in F.A.Lutz and D.C.Hague (eds) *The Theory of Capital,* London: St Martin's Press.

––(1962) 'Production of commodities: a comment', *Economic Journal* 72:477–9.

––(1986) *Saggi,* edited by P.Garegnani, Bologna: Il Mulino.

––(1991) *Lettere a Tania per Gramsci,* edited by V.Gerratana, Roma: Editori Riuniti.

Steedman, I. (ed.) (1988) *Sraffian Economics,* 2 vols, Aldershot: Edward Elgar.

––(1991) 'Negative and positive contributions: Appraising Sraffa and Lakatos', in N.de Marchi and M.Blaug (eds) *Appraising Economic Theories: Studies in the Methodology of Research Programs,* Aldershot: Edward Elgar.

––(1994) 'On the pure theory of capital by F.A.Hayek', in M.Colonna, H. Hagemann and O.F.Hamouda (eds) *Capitalism, Socialism and Knowledge. The Economics of F.A.Hayek,* vol. II, Aldershot: Edward Elgar.

––(1995) 'Sraffian economics and the capital controversy', in F.Moseley (ed.) *Heterodox Economic Theories. True or False?,* Aldershot: Edward Elgar.

Steedman, I. and Metcalfe, J.S. (1981) 'On duality and basic commodities in an open economy', *Australian Economic Papers* 20:133–41.

Stigler, J. (1951) 'The division of labour is limited by the extent of the market', *Journal of Political Economy* 59:185–93.

––(1953) 'Sraffa's Ricardo', *American Economic Review* 43:586–99.
––(1957) 'Perfect competition, historically contemplated', *Journal of Political Economy* 45:1–17.
Talamo, O. (1976) 'Piero Sraffa: su alcuni problemi d'interpretazione', in M.C. Marcuzzo (ed.) *Problemi e storia delle teorie economiche,* Milano: Mazzotta.
Tobin, J. (1958) 'Liquidity preference as behavior towards risk', *Review of Economic Studies* 25:65–86.
Togliatti, P. (1967) [1949] 'Pensatore e uomo d'azione', in E.Ragionieri (ed.) *Gramsci,* Roma: Editori Riuniti.
Tonveronachi, M. (1983) *J.M.Keynes,* Roma: La Nuova Italia.
––(1991) 'Alcune considerazioni in tema di teorie fmanziarie e processi di valorizzazione', in J.A.Kregel (ed.) *Nuove interpretazioni dell'analisi monetaria di Keynes,* Bologna: Il Mulino.
Tosato, D. (1997) 'Equality of rates of return in models of general economic equilibrium with capital accumulation', in G.Caravale (ed.) *Equilibrium and Economic Theory,* London: Routledge.
Townshend, H. (1937a) 'Liquidity-premium and the theory of value', *Economic Journal* 43:157–69.
––(1937b) 'Review of Hawtrey, *Capital and Employment*', *Economic Journal* 43: 321–26.
Tranfaglia, N. (1968) *Carlo Rosselli. Dall'interventismo a* Giustizia e Liberià, Bari: Laterza.
Traniello, F. (ed.) (1993) *L'Università di Torino. Profilo storico e istituzionale,* Torino: Pluriverso.
Trevithick, J.A. (1992) *Involuntary Employment,* Hemel Hempstead: Harvester Wheatsheaf.
Turati, F. and Kuliscioff, A. (1977) *Carteggio,* 6 vols, edited by A.Schiavi, Torino: Einaudi.
Vianello, F. (1998) *On Ricardo's Principle that the Profits of the Farmer Regulate all Other Trades,* Rome, mimeo.
Vicarelli, F. (1977) *Keynes,* Milano: Etas Libri.
Viglongo, A. (1978) *Almanacco Piemontese,* Torino: A.Viglongo and C.Editori.
Viner, J. (1921) 'Price policies: the determination of market price', in C.Marshall (ed.) *Business Administration,* Chicago: University of Chicago Press.
––(1931) 'Cost curves and supply curves', *Zeitschrift für Nationalökonomie* 3:23–46.
Vita-Finzi, P. (1989) *Giorni lontani. Appunti e ricordi,* Bologna: Il Mulino.
Vittoria, A. (1988) 'Cosmo Umberto', *Dizionario Biografico degli Italiani,* vol. 34, Rome: Istituto dell'Enciclopedia Italiana.
Vivanti, G. (1903) *Complement di matematica ad uso dei chimici e dei naturalisti,* Milan: Hoepli.
Whitaker, A.C. (1904) *History and Criticism of the Labor Theory of Value in English Political Economy,* New York: Columbia University Press.
Whitaker, J.K. (ed.) (1990) *Centenary Essays of Alfred Marshall,* Cambridge: Cambridge University Press.
––(ed.) (1996) *The Correspondence of Alfred Marshall, Economist,* 3 vols, Cambridge: Cambridge University Press.
Wicksell, K. (1934) *Lectures on Political Economy,* London: Routledge.
Wieser, F. (1893) *Natural Value,* London: Macmillan.

Winslow, T. (1995) 'Uncertainty and liquidity preference', in S.Dow and J.Hillard (eds) *Keynes, Knowledge and Uncertainty,* Aldershot: Edward Elgar.

Wittgenstein, L. (1922) *Tractatus logico-philosophicus,* London: Kegan Paul.

––(1945), *Philosophical Investigations,* Oxford: Oxford University Press.

Wolfe, J.N. (1954) 'The representative firm', *Economic Journal* 64:337–49.

Wolff, R.D., Roberts, B. and Callari, A. (1982) 'Marx's (not Ricardo's) "transformation problem": a radical reconceptualization', *History of Political Economy* 14: 564–82.

Wray, L.R. (1988) 'The monetary explanation of distribution: a critique of Pivetti', *Political Economy: Studies in the Surplus Approach* 4:269–73.

Young, A.A. (1913) 'Pigou's *Wealth and Welfare*', *Quarterly Journal of Economics* 28: 672–86.

––(1928) 'Increasing returns and economic progress', *Economic Journal* 38: 527–42.

Zamagni, S. (1991) 'Comment', in N.de Marchi and M.Blaug (eds) *Appraising Economic Theories: Studies in the Methodology of Research Programs,* Aldershot: Edward Elgar.

Zanni, A. (1991–2) 'Pareto's monologue with Marshall', *Quaderni di Storia dell'Economia Politica* 9–10:399–423.

Zappia, C. (1996) 'The notion of private information in a modern perspective. A re-assessment of Hayek's contribution', *European Journal of the History of Economic Thought* 3:107–31.

––(1999) 'Equilibrium and disequilibrium dynamics in the 1930s', *Quaderni del Dipartimento di Economia Politica,* n. 243, Università di Siena (forthcoming in *journal of the History of Economic Thought*).

Zarembka, D. and Sihna, A. (eds) (1996) *Research in Political Economy. Latest Developments in Marxist Theory,* vol. 15, London: JAI Press.