Competing Economic Theories

Essays in memory of Giovanni Caravale

Edited by
Sergio Nisticò and Domenico Tosato

London and New York

Competing Economic Theories

Providing a contemporary overview of the debate amongst theoretical stands in economics, this book brings together contributions from a number of eminent scholars. It covers important issues in methodology and the history of thought, as well as economic analysis.

- Part I focuses on the relevance of the history of economic ideas for current economic analysis.
- Part II centres around the role of the classical theory of value and distribution. In the light of Sraffa's unpublished papers, new suggestions as to Sraffa's interpretation of Ricardo are posited.
- Part III contrasts the equilibrium approach with an out-of-equilibrium perspective. Keynes's concept of equilibrium is reconsidered, problems in the existence of monopolistic general equilibrium are reviewed, and the role of demand in classical versus neoclassical theories of value is analysed.
- Part IV evaluates the legacy of Keynes in the light of the recent development of macroeconomics, and tries to answer the question of how much of Keynes's theory remains in current macroeconomic analysis.
- Part V is dedicated to the issue of how institutions ought to be embedded in current economic theorising.

Providing up-to-date, fresh and detailed perspectives on economic theory, this book will prove invaluable for students and academics in the fields of the history of economics and contemporary economic theory.

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Introduction

Sergio Nisticò and Domenico Tosato

This volume originates from the conference held in memory of Giovanni Caravale at the University of Rome 'La Sapienza' on 4–5 June 1998, one year after his untimely death. For the occasion we invited eminent scholars, who had shared some of Caravale's scientific interests, to present papers on the themes that had characterised his research activity and that we thought could be arranged under the following five headings: Economic theory and its history; The classical school and the Ricardo debate; Models of prices and allocations in equilibrium and out of equilibrium; The legacy of Keynes; Economic theorising and institutions. The contributions included in this collection have, thus, been grouped into five parts which we have prefaced with significant passages from Caravale's own writings. The final part also includes the full text of Caravale's 1996 paper on the problem of economic theory and institutions. This paper, together with a contribution by Luigi Pasinetti (also reproduced in the volume), was the springboard for the round table discussion which, coordinated by Pasinetti himself, concluded the memorial conference.

The choice of title deserves some comment. If we look back at the development of economic theory, we find it is mainly characterised by intense and systematic debate between schools of thought as well as by strong rivalry over competing explanations of fundamental economic phenomena: in microeconomics, between the Sraffian and the marginalist theories of value and distribution; in macroeconomics, between the Keynesian and the monetarist theories of national income and employment.

Blanchard (2000) and Woodford (1999) have taken a different and possibly influential stand. They challenge the lack of communication that supposedly existed between schools of thought generally considered to be far apart. They detect, with specific reference to macroeconomics, the existence of objective scientific progress, to which various schools have, in different but significant ways, contributed. In brief, they seem to suggest that a widely accepted body of knowledge, which bears important policy implications, has finally emerged after the fierce debate which until recently characterised the evolution of economics, both on theoretical and empirical grounds.

The idea that in economic science we can perceive advances in the body of knowledge shared by a large part of the profession is, to our minds, an appealing intuition, which may be fruitfully used in the history of economic analysis as a second key of interpretation to be matched to that of opposition and debate. We believe, however, that Blanchard and Woodford's reading should not be mistakenly interpreted as supporting the view that theoretical homogenisation in economics should be looked on with favour. On the contrary, since 'debate' – bitter, tough and passionate debate – is the very source of those past achievements that Blanchard and Woodford bring to our attention, we should be wary of any possible drying up of the sources of discussion with regard to the new issues that continually arise from the observation of the real world, but also of the very 'fundamentals' of the discipline. Homogenisation would inevitably bring about a weakening of the urge to economic research and hence of the same source of scientific progress.

In line with this concern, right from its title this book emphasises the importance of open and transparent competition among different ideas and different schools. In particular, it aims to offer a new reading of some *old* controversial themes and to indicate *new* areas of potential debate among scholars. We are deeply convinced that this is the proper way to pay tribute to Giovanni Caravale, who firmly believed in the progressive nature of the economic debate and whose research activity in this connection will be briefly outlined below.

Part I focuses on the relevance of the history of economic ideas to current economic analysis. Walker warns against the damaging consequences of the widespread habit of separating the teaching of the history of economic thought from the teaching of current economic theory. The harmful effect of this mode of organising university curricula has been twofold: on the one hand, it has reduced the history of economic thought to a superficial review of a host of past theories and past economists, as such unsuitable to graduate programmes in economics; on the other hand, it has consequently impoverished the teaching of current economic theory, which risks missing the benefit of a rich background of ideas and past debates. Walker, however, 'without any hope that the state of affairs will change', conclusively argues that 'the part of past economic theory relevant to each specific modern theory should be taught in conjunction with that theory, in the regular macroeconomic or microeconomic course in current economic theory'. Graziani carries the argument a step further. He claims that 'there is no clear-cut distinction between current economic thought and the economic theorising of the past'. Thus, contrary to those who exclusively 'give weight to logical rigour and refined technicalities', Graziani believes that good theorising is possible only if we start by asking how the problems to be investigated ought to be selected and approached, so that the 'need will spontaneously emerge to know whether and in what manner similar problems were identified and solved in the past'.

Part II centres around the role of Ricardo's contribution to classical economics and economics *tout court*. Through the chapters by Hollander, Garegnani, Blaug, Panico, Zagari, Zaghini, Nisticò, Porta and Mongiovi a lively and wide-ranging debate emerges which touches upon old and new controversies. In their chapters, Hollander and Garegnani offer an up-to-date presentation of the two established and conflicting *rational* reconstructions of the classical theory of value, distribution and growth. Hollander proposes new textual evidence supporting the so-called canonical interpretation according to which a line of continuity exists between the neoclassical theory and the preceding theoretical formulations of Smith, Ricardo, John Stuart Mill and even of Marx. Garegnani, on the other hand, provides new arguments in favour of the Sraffian interpretation which identifies in the classical school the roots of a theory of value and distribution alternative to the marginalist explanation of goods and factor prices. Blaug criticises the historical accuracy of the neo-Ricardian interpretation and in particular the idea of a classical 'core' defined by a given real wage as well as by given techniques and sectoral outputs on the grounds that it inevitably leaves out some important, fruitful elements in the thinking of the classical authors. He casts serious doubts also on the legitimacy of *rational* as opposed to *historical* reconstructions, thus indirectly questioning the validity of both Garegnani's and Hollander's interpretations. The issue of rational versus historical reconstructions is taken up by Panico and Zagari. Panico rejects Blaug's criticism of Sraffa's interpretation of the classical school. To begin with, he questions the very existence of pure historical reconstructions – as any interpretation is inevitably 'rational' in as much as it is influenced by the historian's present knowledge – and thus argues that any evaluation ought to be based on different criteria. He then goes on to praise Sraffa's interpretation as the outcome of an outstanding example of editorial scholarship, pointing to the accuracy in collecting and presenting the textual evidence. Zagari critically discusses the alternative rational reconstruction of the classical theory of growth and distribution, namely the canonical growth model. He claims that precise constraints ought to be imposed on rational reconstructions, i.e. limits within which theories formulated in the past can be reinterpreted and re-elaborated with the help of today's analytical tools; and that such limits have been passed by the canonical model. Zaghini deals with the analytical problem of the convergence of market prices towards natural prices, a key issue for the explanatory validity of the classical long-period positions. In particular, he critically examines Garegnani's (1997) ambitious attempt to offer a coherent and general solution to this problem. Zaghini observes that the classical approach builds on the assumption that 'agents, outside of equilibrium, know the economic system's equilibrium configuration' (that is, natural prices and effectual demands) 'and are guided by it – a variant of the perfect knowledge or foresight hypothesis that plagues a good part of modern theory'. He grants that Garegnani tries to go beyond this unsatisfactory approach, but shows that Garegnani's refusal to use the concept of demand function, not necessarily to be anchored to the neoclassical paradigm of utility maximisation, also makes his approach unsatisfactory and flawed. Nisticò joins in the debate on the interpretation of the classical theory, addressing two issues. The first concerns the differences between Smith and the later classicists in the approach to price theory, from the viewpoint of the choice of the distributive variable to be considered as given from outside the model: the rate of profit, according to Smith, the real wage, according to the later classicists. A rationale is thus provided for Smith's theory of the component parts of price, later so heavily criticised by Marx and Sraffa under the label of the *adding up* theory. Nisticò's second concern regards Ricardo's notion of natural equilibrium, which he argues should be viewed as a temporary centre of gravity rather than a long-period position capable of persisting through time. In this perspective, he maintains that the Sraffian assumption according to which, in each given situation, one distributive variable is exogenously given is not necessarily incompatible with Hollander's thesis, that there is room in Ricardo for a declining real wage throughout the accumulation process. Finally, the chapters by Porta and Mongiovi tackle the issue of the role played by Sraffa's editorial work in the Ricardo debate. Porta argues, also on the basis of Sraffa's papers in the Wren Library at Trinity College, that Sraffa's *interpretation* is, on the one hand, heavily conditioned by his own theoretical research programme and, on the other, no more than a restatement of Marx's reading of Ricardo. Mongiovi rejects Porta's aim to belittle Sraffa's contribution to the reconstruction of Ricardo's theory. He argues that the well known circumstance that Sraffa's work on Ricardo 'was part of a larger project that culminated in *Production of Commodities by Means of Commodities* . . . has no bearing on the soundness of his interpretation'. Moreover, as far as the alleged Marxian connection is concerned, he denies the existence of any significant textual support for Porta's thesis and claims, on the contrary, that evidence indicates that Sraffa came to Ricardo not through Marx but via Marshall.

The role of the concept of equilibrium is the main issue touched upon in Part III. Chick provides a sympathetic assessment of Caravale's contribution to the study of the concept of equilibrium with special reference to its usefulness in interpreting the *General Theory*. Chick emphasises the importance of Caravale's proposal to modify the classical-type concept of long-period equilibrium to include the equality of expected rather than actual rates of return and then discusses the role of his notion from the viewpoint of the 'logical' as opposed to the historical nature of equilibrium. Chick does not appear to be fully convinced by Caravale's attempt to identify a line of continuity between the classics and Keynes on the role of long-period equilibrium in a logical sense. Instead, following Joan Robinson's notion of *history*, she believes that the short period cannot be dismissed in the interpretation of Keynes's theory and of the working of actual economic systems. Kurz provides a brief overview of Sraffa's unpublished papers and correspondence with the aim of clarifying the importance of Sraffa's contribution to economics and in particular to show that Sraffa succeeded in rescuing from oblivion a 'distinct' classical theory of value and distribution that cannot be revisited in terms of the neoclassical demand and supply determination of goods and factors prices. Tosato and Nicola address the issue of the existence of monopolistic general equilibrium. Tosato's analysis shows the results and potentialities of those works that aim to foster the general equilibrium theory with greater realism, while retaining the idea of interdependence typical of the benchmark model. Whereas the conjectural approach, with its *ad hoc* assumptions, may produce useful results on specific issues, it is inevitably to the objective approach, with its emphasis on analytical rigour, that we have to turn if the aim of the analysis is to establish a comparison between monopolistic and competitive general equilibrium. This comparison shows that the existence of equilibrium in a monopolistic market regime (1) requires further assumptions (uniqueness of the price or demand correspondence and quasi-concavity of the profit function) not needed in the standard Arrow–Debreu model; and (2) may depend on the choice of normalisation rule. After noting that most of the difficulties occur also at a partial equilibrium level, Tosato concludes that 'some of the problems that preclude the possibility of establishing existence results of the same degree of generality as under perfect competition are to be considered inherent in the situation of direct interaction between firms not mediated by the market'. Nicola's chapter emphasises the importance of abandoning the assumption of an auctioneer capable of instantaneously driving the whole system towards its equilibrium configuration. Nicola proposes the notion of imperfect general equilibrium that should place the out-of-equilibrium dynamics at the heart of the analysis of the price system. The out-of-equilibrium perspective is also central in Amendola's chapter, which discusses the limits of the equilibrium analysis from a technological viewpoint. In particular, Amendola addresses the relation between technology and unemployment using a neo-Austrian approach; in this context he analyses the *vertical* dimension of technology and the problems connected with the phases of construction and utilisation of productive capacity. The chapter shows how the attempts by economic agents 'at re-establishing the consistency over time of construction and utilisation disturbed by the original shock stir an out-of-equilibrium process that propagates the initial distortion over time'. Boitani finally evaluates the main conclusions reached by the New Keynesian literature on macroeconomic equilibrium, stressing the specific role of the assumption of near rationality to corroborate the findings of that literature about the soundness of the Keynesian notion of unemployment equilibrium. In particular, he emphasises the fact that even 'a small proportion of near rational agents, under strategic complementarity, has a disproportionately strong impact on the macroeconomy'.

Part IV offers an evaluation of the legacy of Keynes in the light of recent developments in macroeconomics. Carabelli traces back the Keynesian notion of radical uncertainty in Keynes's early interest in dilemmas and tragic choices, which characterise situations of indecision and of irreducible conflict. She argues that Keynes's refusal to assume comparability of magnitudes such as probability and real income or general price level may derive from his youthful studies in Greek tragedy. De Cecco proposes a different reading of Keynes's thought and argues against the thesis that Keynes renounced tradition. According to De Cecco, the sometimes ambiguous way in which Keynes expresses his revolutionary ideas on macroeconomics clearly shows his urge 'to be taken seriously by his fellow economists and to avoid being lumped with the heretics'. Jossa provides an up-to-date state of arts of the New Keynesian approach and raises the question of whether and how it can offer further arguments in favour of the Keynesian view of macroeconomics. He emphasises the importance of the New Keynesian microfoundation of fixed prices and wages in strengthening Keynes's crucial finding that equilibrium is reached through changes in output. Jossa grants that some of the New Keynesian models draw heavily from the neoclassical toolbox, thus justifying an attitude of rejection on the part of the more radical Keynesians. He argues, however, against the idea that New Keynesian and Post (orthodox) Keynesian economics should be viewed as incompatible and competing lines of analysis, as many in the Keynesian camp hold them to be. A quite different evaluation of the New Keynesian approach is contained in Rotheim's chapter. According to Rotheim, New Keynesians 'are treading on thinner and thinner ice' and will not be able to stay much longer between neoclassicism and Keynesianism. Some of them will go back to the safe neoclassical metaphor according to which unemployment equilibrium occurs because of the existence of various rigidities in the aggregate labour, goods and capital markets. But, for those other 'strong' New Keynesians who think in terms of spill-over effects and strategic complementarities, the neoclassical 'ice [will be] finally cracking'. They will be forced, therefore, to embrace Keynes's idea that the organic interdependence which characterises the capitalist economy is at the root of the 'fluctuations in output, spending, and therefore employment *as a whole*'. Rodano's chapter provides further arguments to Woodford and Blanchard's thesis mentioned above, according to which during the last century macroeconomics has witnessed a progressive accumulation of scientific knowledge. Rodano's reading of the macroeconomic debate emphasises the circumstance that each period of juxtaposition between schools of thought has generated an agreed synthesis and that 'eventually, these achievements will be brought into question by new debates and controversies, which sooner or later will converge to a richer synthesis, although a temporary one'. A different position on this issue is finally provided by Musu, who argues that the macroeconomic debate between conflicting schools of thought, far from having produced a shared body of knowledge, has actually yielded the relevant outcome of sharpening the reasons and the nature of the irreducible distinction between neoclassical and Keynesian macroeconomics. In fact, Musu concludes, 'modern Keynesian macroeconomics implies a complex vision of economic policy, which . . . cannot accept the minimalist approach to economic policy entailed by the New Classical macroeconomics and deriving from theoretical premises too distant from the features of market economies in the real world'.

Finally Part V is dedicated to the relationship between economic theorising and institutions. Pasinetti's and Caravale's chapters emphasise the limits of the neoclassical treatment of economic institutions, but express a different viewpoint as to how institutions ought to be embedded in current economic theorising. According to Pasinetti, the Keynesian and the classical-type formulations constitute a body of *pure* theory which might fruitfully be enriched, in a subsequent phase, with the findings of institutional economics. Caravale rejects this separation between a first phase of 'pure' theorising and a second phase in which institutions make their appearance on the stage. He thus praises the classical and Keynesian approaches precisely for their attempt to build theoretical models *starting from* realistic assumptions as to the institutional set-up: namely, the role of the social classes in explaining agents' behaviour as opposed to the assumption of atomistic utility and profit maximisation, the idea of exchange of commodities occurring at 'false' prices, the role of labour market institutions in determining unemployment equilibrium. Ciocca brings to the discussion the viewpoint of a central banker and deals with the role played by 'the *conventions* – what Keynes called changeable beliefs – of global finance' in constraining the economic policies of national governments and describes three possible ways out of this supremacy of finance over economic policy: the *enlightened* scenario, that envisages the introduction of a global currency; the *interventionist* scenario, based on the use of the Tobin tax to reduce speculative capital movements; and the *autarchic* scenario carrying with it the undesirable 'reaction by the [*local*] political sphere, claiming back its primacy over the economy'. Spaventa, on the one hand, accepts Pasinetti's and Caravale's starting point, according to which we should overcome the separation between 'a theory devoid of institutions and a prevalently descriptive analysis of institutions devoid of theory'. However, he is less pessimistic than Pasinetti and Caravale about the ability of recent mainstream economic research to deal with the problem of institutions, arguing that those who object to the mainstream treatment of institutions 'ought to propose and apply different tools of analysis' which are not yet available. Spaventa also expands on Caravale's criticism of Pasinetti's proposal of a two-phase economic theorising and points to 'the normative sterility that would result from the implicit suggestion that in an initial phase economic analysis should not be involved with studying institutions'. Sylos Labini provides three examples of the impact that historical and institutional changes may have on economic phenomena: the changing nature of the business cycle during the last two centuries; the relation between the structural changes both in the commodity markets and in the labour market on the growth rates of wages and prices; the effects of legislation on bankruptcy with regard to innovation and growth. The main upshot of the argument is that:

> the essence of economic analysis lies precisely in the harmonic integration between theory and history. The latter provides the factual premises which, when transformed into hypotheses, allow for the elaboration . . . of historically restricted theoretical models. It is not that their logical coherence is historically restricted; that coherence is on the contrary timeless. It is rather their interpretative capacity which is historically contingent.

In his conclusive remarks on the round table discussion on 'Economic theorising and institutions' Pasinetti, while minimising the differences between his stance and Caravale's, reiterates his criticism of mainstream economics as intrinsically unsuitable to accommodate realistic institutional features in the corpus of the theory.

As initially mentioned, this volume originated in a conference held in memory of Giovanni Caravale. We think it proper to conclude this introduction with a short profile of his career and with an evaluation of his scientific work which bears on the topics debated in this book and deserves the attention of those scholars who are actively engaged in the discussion of competing economic theories.

Giovanni Caravale was born on 18 August 1935. In 1953 he graduated from high school with highest marks, took up the study of law at the University of Rome 'La Sapienza' and began working in the research department of the Banca Nazionale del Lavoro, one of Italy's leading credit institutions. Work did not interfere with his studies, but it did influence his decision to write the thesis for his law degree in the field of economics. In 1958 he passed an extremely selective competitive examination and joined the technical staff of the Italian Senate, where, from 1965 to 1972, he served as Secretary to the Permanent Committee on Finance and Treasury Affairs, Industry and Foreign Trade. In 1960 and 1961, as a visiting student at Trinity College, Cambridge, he came in contact with Maurice Dobb, Piero Sraffa, Nicholas Kaldor and other great economists and began to accumulate notes on growth theory, particularly on the relationship between growth and cycles. In 1963 he qualified for university teaching and began lecturing on economics and fiscal policy at the University of Pescara while still holding his job at the Senate. From 1968 to 1971 he lectured on political economy at the University of Perugia. In 1972 he was awarded a full professorship at the University of Perugia and resigned from the staff of the Senate. He taught in Perugia until 1979, when he moved on to the University of Rome 'La Sapienza', in the Faculty of Political Sciences.

Among the prominent public positions that he held during his professional life, Giovanni was a member of Italy's Central Tax Commission, President of the Atlantic Economic Society, Minister of Transport in the Government of Prime Minister Lamberto Dini and a member of the board of directors of the *Italian Encyclopaedia*. Giovanni entered the government in January 1995, during one of the most delicate phases of post-war Italian history, the so-called 'fall of the first Republic'. The Dini government's mandate was to put Italy on track to financial stability. The difficulties that Giovanni faced at the Transport Ministry were compounded by the ending of public monopoly virtually throughout the transport sector, where previous governments had generally prized political consensus over technical and economic efficiency. The April 1996 general election brought the curtain down on Dini's 'government of technicians', but Giovanni's projects remained on the agenda of his successors at the Ministry. In June 1996 Giovanni returned to the University of Rome and his research work. He died unexpectedly on 29 May 1997.

Giovanni Caravale's scholarly contributions can be divided into groups corresponding to four phases in the development of his research work. Until the early 1970s he concentrated on formulating a comprehensive scheme for the joint interpretation of fluctuations and long-term growth in which oligopoly plays a major role. The principal results of this first research programme were *Cicli economici e trend* (1961), *Fluttuazioni e sviluppo nella dinamica di squilibrio di un sistema economico* (1967) and *Oligopolio differenziato e processo di sviluppo* (1973). The main threads running through all these publications are: (1) the role of structural unbalance, i.e. the divergence between the composition of demand and of output, in generating trade cycles; (2) the use of the 'equilibrium dynamic path' simply as a theoretical reference point to emphasise how non-fulfilment of the conditions defining that path may generate trade cycles with growth; (3) the study of possible disequilibrium dynamics, not constrained by the aim of proving convergence to the equilibrium path but rather as a theoretical tool with which to explore how systems evolve once they are displaced from the 'golden age' path.

The attempt to provide a historically accurate analytical reconstruction of Ricardo's theory of value and distribution occupied Caravale's intellectual energies in the second phase of his research, which roughly spanned the years from 1973 to 1980. The result was the influential Ricardian growth model that Caravale, together with Domenico Tosato, developed in those years. The essential elements of the model were first published in Italian in 1974 as *Un modello ricardiano di sviluppo economico*, and then in an expanded English version, published in 1980 as *Ricardo and the Theory of Value, Distribution and Growth*. The book provides an analytical solution of general validity – i.e. not subject to the limitations of the labour theory of value – of Ricardo's central problem: the relationship between diminishing agricultural returns and the general rate of profit. It also contains an analysis of the role that the 'standard commodity' played in the evolution of Ricardo's thought, an analysis that the authors had previously published in Italian in an article, 'Saggio di profitto e merce tipo nella teoria di Ricardo', which appeared in 1978 in *Rivista di politica economica*. Giovanni's reconstruction of Ricardo's theory rests on the following building blocks: (1) the identification of the natural, as opposed to the market, path of rent, profits and wages as the 'core' of Ricardo's scientific programme; (2) the key role consequently played by the natural wage, as defined by Ricardo in the *Essay on Profits*, in determining the natural equilibrium path; (3) the ensuing link, given the natural wage, between diminishing returns in agriculture and changes in distribution; in other words the inverse relation between the dynamics of rent, driven by the need to extend cultivation to less and less fertile land, and the rate of profit. The simultaneous inclusion of these three blocks within a unifying interpretative scheme marks the proposed reconstruction as substantially innovative with respect to the other main interpretations of Ricardo's theory. Caravale's emphasis on the role played by the natural wage constitutes a clear departure from the 'new view', which asserts the existence, within Ricardo's theory, of anticipation of the neoclassical determination of goods and factors prices on the basis of demand and supply functions. Yet his reconstruction markedly differs also from the neo-Ricardian interpretation in as much as it strongly rejects the idea of attributing to Ricardo a Sraffa-type inverse relationship between alternative levels of the real wage and the profit rate with given technological conditions.

From the early 1980s until 1990 Caravale took up the difficult task of providing a critical presentation of the most important interpretative strands of Ricardo's and Marx's thoughts. Prominent in this third phase was Caravale's efforts to counter, in a clear and constructive fashion, the various attempts to establish a direct link or line of descent from Ricardo to Marx primarily on the basis of a common appeal to the labour theory of value. *The Legacy of Ricardo* (1985) and *Marx and Modern Economic Analysis* (1991) offer the reader a comprehensive view of the positions held on this issue by distinguished contributors such as John Hicks, Samuel Hollander, Mark Blaug, Pierangelo Garegnani, Paul Samuelson, William Baumol and Caravale himself.

According to Caravale, the crucial divide between Ricardo and Marx regards the role played by the labour theory of value in their analytical constructions. In Marx's work, the labour theory of value is essential to proving the existence of exploitation in a capitalistic economic system; the transformation of labour values into prices of production is the analytical instrument through which such a result is allegedly achieved. The demonstration by Dmitriev, Bortkiewicz and Sraffa of the absence of a necessary causal relation going from labour values to production prices, which can be determined from the mere knowledge of the technical coefficients of production, deprives Marx's theory of exploitation of its claimed scientific basis. Nor, as Caravale repeatedly emphasised, is the argument acceptable that grounds the theory of exploitation in Sraffa's inverse relationship between wages and profits, since this relation is determined by the technology and has nothing to do with the gap between the value of labour power and the value of its product. In Ricardo, by contrast, the labour theory of value is an analytical tool that serves the role of eliminating the interdependence between prices and distribution and thus of showing the basic relation between rents and profits in a dynamic context in which the wage rate is supposed to be determined and to evolve independently. Notwithstanding the limitation of the labour theory of value, of which Ricardo was well aware, the attainment of his goal of determining the natural laws that govern the path of wages, profits and rents does not depend on the validity of that theory. In fact, that aim can be achieved on the basis of a theory of value not suffering from the limitation of Ricardo's tool, as Caravale and Tosato have shown, building on Sraffa's *Production of Commodities by Means of Commodities*.

In his last years Caravale was drawn to work on some unresolved Keynesian questions and on developing a novel research programme reflecting his long-standing view that economic analysis, economic policy and the history of economic thought were closely interconnected. To this end, Giovanni was forced to grapple with thorny methodological and analytical questions, beginning with the very possibility of adopting a new, more constructive, notion of equilibrium. His main contribution on this matter is included in the collection of essays he edited, *Equilibrio e teoria economica* (1994), subsequently translated into English as *Equilibrium and Economic Theory* (1997).

Caravale thought that the literature offered two opposing and equally unacceptable views: the apologetics of neoclassical theory, which holds that equilibrium is a desirable state of general market clearing spontaneously attained in decentralised market economies; and the critical Marxian view that equilibrium requires the fulfilment of such demanding conditions that its attainment is utterly unrealistic, so that the market system can produce only inefficiencies and conflict. Caravale proposed a notion of equilibrium of a classical and Keynesian flavour, capable of representing – naturally, in a stylised and simplified way – the potential centre of gravity of the economy (the classical component) and the not necessarily optimal outcome of the relevant and dominant forces (the Keynesian component).

However, it is not enough to prove the existence of equilibrium; it is at least as important to show that the economy tends to converge on it. But even proof of convergence may not be sufficient, according to Caravale, to settle the question of the relevance of the equilibrium position. A further issue must be faced: it concerns, from one point of view, the speed of convergence and, from another, the persistence of exogenous data. The Walrasian approach tends to promote the idea of rapid convergence of disequilibrium to equilibrium prices, so that one can argue as if markets were continuously in or close to equilibrium. The neo-Ricardian school addresses this issue from a different angle and looks at the relation between market and long-period prices by assuming that market forces continuously drive the former towards the latter; this is the classical notion of gravitation as distinct from that of stability typical of neoclassical theory. The gravitation of market prices towards the long-period prices is then considered as occurring in historical time, given the assumption of persistence of the data. In Garegnani's view, these data include the technology, the real wage rate and the level and composition of output (see Chapter 4 of this volume).

Caravale's notion of long-period positions tried to cope with the problem of the speed of convergence on equilibrium in the presence of ever changing exogenous data. He set himself the aim of avoiding both the Walrasian instantaneous way of conceiving the reaching of equilibrium and the neo-Ricardian hypothesis of data persistence. He resorted to a notion of equilibrium as a potential centre of gravity that can be defined only in logical rather than historical time. In his view, equilibrium is thus an analytical tool that one cannot do without in order to establish a logical correspondence between exogenous data and endogenous variables. This tool should be capable of offering a scientific explanation that can be applied in policy making. A typical example, frequently mentioned by Caravale, is Keynes's theory of effective demand, which sets a logical correspondence between a given amount of investment, substantially taken to be exogenous, and the equilibrium level of income. No one can deny that investment changes over time:

> However, this circumstance does not prevent each equilibrium position from exerting a strong attraction over the real variables in each period of time. The fact that the system will never be able to reach the equilibrium position (which is in continuous movement, through actual calendar time) does not deprive this position of its importance as a meaningful point of reference, as a centre of gravity for the system. On the contrary, it represents a mirror of the actual reality of contemporary economies, in their perennial unrest, in their continuous process of change – a reality which does not convey the image of a state of rest.
>
> (Caravale 1997b: 23)

Caravale's position thus appears to be close to Marshall's. From Marshall Caravale draws the basic idea that economic 'laws' and prescriptions are bound to the *ceteris paribus* clause; at the same time he differs from Marshall in thinking that the application of the *ceteris paribus* clause should not be confined to the historical short term, but could be used to tackle any logical problem independently of the time span involved. His approach breaks the classical and neoclassical connection between the long run and the notion of perfect adjustment of the economy to changing conditions. The equilibrium position may never be attained because conditions do in fact change in historical time, but it remains a fundamental benchmark of the analysis.

Caravale's most recent articles touched on the importance of uncertainty and expectations in the notion of Keynesian unemployment equilibrium ('Keynes, equilibrium and modern economic systems', in *Perspectives on the History of Economic Thought*, edited by R. F. Hébert, 1993), on the role of demand in the classical context ('Demand conditions and the interpretation of Ricardo', *Journal of the History of Economic Thought*, 1994; 'Prices and quantities: Walras, Sraffa and beyond', *Studi economici*, 1994), and on the issue of incomes policy ('On a recent change in the notion of incomes policy', in *New Keynesian Economics/Post-Keynesian Alternatives*, edited by R. J. Rotheim, 1998). All these papers express his conviction that the future of economics depends on the positive solution of a fundamental problem: the integration within the discipline of analytical rigour and relevance of the models. In line with this tenet, he addressed the general aspects of the relations between institutions and economic theorising ('Economic theory and institutions: an introductory note', *Rivista di politica economica*, 1996) and inaugurated a doctoral course on 'Political Economy and Institutional Reality' in his own Faculty of Political Science. This represented the final endeavour of a distinguished representative of the profession; one who was profoundly convinced that economics cannot escape the fascination of being technical and controversial at the same time, with the roots of controversy lying predominantly in the pre-analytical dimension. While he was aware that he himself was constrained by his own *Weltanschauung*, Caravale always sought to make analytical rigour and intellectual honesty prevail when tackling any economic issues. Faced with the main existing 'competing economic theories', he could not perfectly fit any of them. Though respectful of the neoclassical concern for analytical rigour, he felt that any convincing theoretical explanation of production, distribution and exchange had to abandon the assumption that human economic behaviour can be modelled exclusively by means of an objective function to be maximised under constraints. He was convinced that conventions and institutions are fundamental in shaping individuals' behaviour, and that the notion of social classes should have pride of place over that of self-interested individuals in economic theory. This is the main reason why he was in sympathy with Ricardo, Sraffa and Keynes, though always ready to recognise their ambiguities and logical inconsistencies.

Caravale felt that the debate among different schools of thought on what he considered the most fascinating and intricate phenomena to be explained – namely value, income distribution and unemployment – was marked by both apologetic attitudes and preconceived criticisms. He believed that the best way to help economic theory progress in understanding how actual economic systems work was to compare the competing theoretical strands in an open, transparent way. We thought it appropriate to apply his method to this memorial volume.

Competing Economic Theories is a tribute by friends and colleagues of different persuasion to a brilliant, open-minded economist and to an extraordinary teacher.

Part I

Economic theory and its history

> the economist . . . cannot afford to ignore the existence of different lines of economic thought running parallel in the history of our discipline, often conflicting with one another, at times intersecting each other . . .
>
> being a historian of economic thought and an economist (or political economist), are to a very large extent the complementary sides of the same coin. It is no accident that in the greatest economists of the past and of the present – from Ricardo to Marx, from Marshall to Pareto, from Schumpeter to Keynes, from Sraffa to Samuelson, to name but a few – the profound knowledge of the history of thought goes hand in hand with their capability to supply lasting contributions to economic analysis.
>
> (Caravale 1992d: 206)

1 The relevance for present economic theory of economic theory written in the past

Donald A. Walker

Introduction

Terminology

In order that this chapter may be clear to the reader, some understandings about terminology must be established. First, if we speak of 'theories that were formulated in the past', ordinarily we mean to leave open the question of whether they are still valid, and if we speak of 'past theories', ordinarily we imply that they have been discarded and that implies that they were defective and were supplanted by current theories. Nevertheless, in this chapter the latter term, used for the sake of brevity, will have the same meaning as the former expression. If a past theory is considered to be invalid, that will be stated explicitly.

A second terminological consideration is the meaning of 'the past' and 'the present' in relation to economic theorising. There is no sharp line drawn between them in this study. There is a temporally spacious moving present which is not just the knife edge of the present instant. In the wide sense, 'the present' is today and a recent past period in which intellectual activity directly related to present concerns occurred. The view has been expressed in relation to J. A. Schumpeter's inclusion of the work of Paul Samuelson in his *History of Economic Analysis* (1954) that 'Samuelson was a contemporary and therefore not legitimate material for the history of economic thought in the sense that, because of the problem of inadequate perspective, one does not do current history of economic thought' (Samuels 1988: 25). That view is not shared in this chapter, nor, of course, was it shared by Schumpeter. One could argue that understanding anything, including the most recent economic theory, requires adequate perspective, and, in fact that is precisely the position taken here. The perspective in the latter case is given by a knowledge of the history of economic thought.

All writings on economics become part of the history of economics from the moment that they are produced. There is therefore no particular age that a theory must have for it to be considered a part of the history of economic thought. What distinguishes current theorising from the study of the history of economic thought is the approach taken by the investigator. The historian of economic thought, whether dealing with the work of Adam Smith or with an article published yesterday in the *Journal of Economic Theory*, is concerned with describing and viewing it in the light of other parts of the author's work, with finding the relationship of the material to the work of other past and contemporary writers, with interpreting it in the light of other doctrines, with revealing the methodology, preconceptions and biases of the author. The economic theorist, on the other hand, is concerned with rectifying a logical problem in a current theory, extending a theory or developing a new theory, arguing against another competing current view, or developing a new model of an empirical situation (Walker 1988: 99–100).

Third, this chapter does not refer to 'economic analysis' as something different from 'economic thought'. There is, of course, a distinction between rigorous analysis and opinions about appropriate policies and other distinctions that some writers mean to evoke when they use the two terms. In this chapter, however, those distinctions will be described directly, and 'economic analysis', 'economic thought', 'economic doctrine', 'economics' and similar terms will all be used to mean economic ideas and constructions.

Distinctions between theories, models, methods, tools

The differences among theoretical concepts or abstractions of a general nature, theories or theoretical models (the two terms are generally used interchangeably in this chapter), econometric theory, methods of theoretical and empirical economic investigation, analytical tools, and models of empirical situations are important for an understanding of the relation between past and present economics. Much of the confusion and controversy surrounding the question of the value of the study of the history of economic thought stems from a failure to discriminate between those different aspects in discussions of the topic. It has been argued, for example, that when a change in the tools used by economists occurs, past economic thought – that is, thought dealing with the displaced set of tools – becomes uninteresting for current theory (Cesarano 1983: 64). In fact, most tools, such as a formula for the elasticity of demand or the definition of the marginal rate of technical substitution, and many methods, such as the *ceteris paribus* technique, are not tied to a particular phase of economic history and are always useful in their proper domain of application. The same is true of economic concepts such as 'labour', 'capital', 'profits', 'entrepreneurship' and 'marginal product'.

There are theories or models that do not deal directly with a particular set of real economic variables and these may also be regarded as true at some time subsequent to the period when they were developed. Examples are the theories of the source of interest, of the nature and determinants of the rent of land, of the role of the entrepreneur, of choice, of the relation of the income generated by capital goods to their prices, of profit, and so forth. That type of theory becomes incorporated into one or another of the present-day theories dealing with that particular subject.

Interests for which past economic thought is relevant

There are as many respects in which past economic doctrine is relevant to present-day concerns as there are topics which involve it. It depends upon the sort of activity that is undertaken. For example, if the objective of a researcher is to discover how an economic theory developed into its present form, the study of the history of economic thought is not merely relevant but is the essence of the subject matter under consideration. It is relevant for people who desire a more complete picture of the past or richer understandings and modern interpretations of past intellectual endeavours achieved through contrasts and comparisons of the work of past writers, and so forth in endless variations. For the scholar who wishes to understand the rationale behind the economic policies followed by a past government, the economic theories that were accepted by the politicians of the time are relevant. Some obvious examples of policies related to economic doctrines are the repeal of the Corn Laws, Russian economic policy in the early 1920s, and the decisions of the US Federal Reserve system during the decade of the 1930s. For the scholar who derives satisfaction from learning about the intellectual life of George Bernard Shaw, the Fabian tracts of the 1920s are relevant. For the scholar who wants to write a history of modern Chile, the teachings of the Chicago school in the 1970s and 1980s are relevant.

Another motive at work in many instances of research into the history of economic thought is a desire to learn more about human behaviour:

> the highest claim that can be made for the history of any science or of science in general is that it teaches us much about the ways of the human mind. To be sure, the material it presents bears only upon a particular kind of intellectual activity. But within this field its evidence is almost ideally complete. It displays logic in the concrete, logic in action, logic wedded to vision and to purpose.
>
> (Schumpeter 1954: 5)

The work of Adam Smith, for example, has been used as a laboratory for enquiring into behaviour, morality and mores. We find research papers on Smith's treatment of the virtues which are really inquiries into the criteria that can be used to judge human actions and into the psychological make-up of humans as manifested in economic settings (see Pack 1997). A study of F. A. Hayek's thought on spontaneous socio-economic orders is an enquiry into how human societies form institutions, how cultural evolution occurs and transmits orderly behaviour, and whether humans do these things by rational economic calculation (Sabooglu 1996). The work of past economists may be used to study the use of metaphors and to learn about and evaluate techniques of persuasion (see McCloskey 1985).

There is, of course, no question about the relevance of current economic theories for the study of past economic theory. The activity of describing, interpreting and evaluating past theory is undertaken under the powerful influence of current economic thought. Indeed, it is the most important influence in the construction of our view of past economic thought. The ability of a historian of economic thought also influences his account of past economic theory. Historians of economic thought have different ideas about present-day economics and some are more capable than others. Since there is disagreement over which current ideas are worthwhile, there is no unanimous judgement on the value of the doctrines that were formulated in the past. For example, an economist of the modern Austrian school may believe that a particular past idea is worthwhile, but a modern neoclassical economist may believe it is not, as is the case in the evaluation of the ideas of F. A. Hayek on the efficiency of a socialist economy (Caldwell 1997: 1876–86). These considerations are of vital significance in understanding the work of any particular historian of economic thought. Knowledge about the historian helps us to understand why he develops the analysis that he does of a past doctrine, why he selects and emphasises some elements of a writer's work, why he interprets them as he does, and why he chooses particular criteria by which to evaluate it.

The supply of articles and books on the history of economic thought is forthcoming because individuals want to write them and educational institutions, foundations, government agencies, and publishers are willing to subsidise or reward their research directly or indirectly. Some universities believe that it adds to their lustre to have individuals in their departments of economics who publish research in the history of economic thought. In Europe and Japan, for example, some universities grant tenure and promotion as a reward for articles, books and conference activity on the subject. People are interested in the subject and are therefore willing to pay for journals and books on the topic. Students take courses in the history of economic thought in order to obtain a well rounded liberal education. Thus the history of economic thought is relevant to whoever is interested in the topic, for whatever reason the person finds it interesting. There are many such persons and reasons, so the supply of research will be forthcoming and the demand for research, if not for courses in the history of economic thought, will continue to flourish. The question at issue here, a question that has not been addressed in the foregoing remarks, is whether past economic theory is in fact relevant for current economic theory and, if so, in what ways, and this study now turns to those issues.

Two views of past economic theory

One opinion about the history of economic thought is that it is irrelevant for current economic theory and therefore that the study of the history of economic thought is unnecessary for someone who wishes to be a competent economist. Regarding economic theory, there is 'no necessity for including its history as a part of professional training' (Gordon 1965: 126). It is irrelevant, the argument goes, because in the process of the construction of economic theory, ideas that are worthwhile are incorporated into it, and ideas that are false or useless are discarded (see Schumpeter 1954: 4). The meaning of 'worthwhile' is usefulness in solving current economic problems or in understanding the behaviour of the modern economy. On this view, current economic theory incorporates all that is worthwhile that has been discovered in the past and what has been discarded is not worth studying for the purpose of attaining the best possible current theory. Additional worthwhile material would be useful new constructions.

It is true that economists do many things very competently that do not require a knowledge of the history of economic thought. Examples are studies of the standard of living of the population of California classed by ethnic origin, of the history of aluminium prices and production since World War II, of the savings rate of the Japanese, of the impact of the North American Free Trade Agreement on employment in the participating countries, of the costs and benefits of government policies and programmes, and the thousands of studies done by consulting firms and the economic research departments of banks and insurance companies. The history of economic thought is – up to a point – irrelevant for the new econometric field of simulation-based estimation. The great minds of the past never thought of methods of approximating high dimensional integrals to obtain unbiased estimators of economic variables and have nothing to add to the many empirical studies that use simulation methods (see Stern 1997). The same is true of many other new fields of economic studies.

Practitioners in empirical and technical fields like economic statistics and econometric theory have thought of the type of work that they themselves undertake when exhorted to recognise the current value of past doctrine and have therefore regarded the claims of historians of economic thought as being without merit. Economists are educated in graduate schools to perform studies like those mentioned above and to teach about them and the realisation of the irrelevance of the history of economic thought for such studies has contributed greatly to driving its study from lists of required graduate courses in the United States. Although a course in the subject might help a practitioner of applied economics to be a more interesting person and enrich his knowledge of human achievements, historians should recognise the irrelevance of discarded past doctrine for the empirical and technical aspects of current economics. They should base their argument in favour of its relevance upon a careful delimitation of the type of modern studies which do in fact benefit from it or can benefit from it.

Nevertheless, many things are wrong with the point of view expressed above. In the first place, although writers remote in time such as Sir William Petty, Adam Smith and David Ricardo are irrelevant for the development and understanding of new fields, especially highly technical ones such as simulation-based estimation, as time passes the new field of study accumulates its own very relevant history, a knowledge of which will be necessary in the ways discussed in the next section. Second, the point of view does not discriminate between the different types of economic thought. It does not recognise that empirical models and statistical studies are only a part of past economics, and that economic concepts, methods and theories or theoretical models are less time-bound and may therefore be useful in the present although they were produced many years ago. That matter will also be examined in the next section.

The next consideration is that there is no such thing as a generally accepted single body of doctrine or knowledge that can be called current economic theory. The diversity of theoretical views is reflected in the diversity of analyses of empirical problems (Swartz and Bonello 1997). Thus by 'current economic theory' in this chapter is meant simply one or another of the theories held today, with no implication that there is a single body of received doctrine. Economics is a vast discipline and is divided into fields of interest. To discuss the point of view that past economics is irrelevant today because current theory incorporates all worthwhile economic thought, therefore, it is first necessary to specify the part of economic theory that is under consideration.

For example, macroeconomic theory can be considered. That immediately leads to the observation that instead of presenting a unified body of generally accepted doctrine, economic theory in regard to macroeconomics has for many years been divided into a number of differing streams. The different and largely incompatible macroeconomic theories express different views of how the economy behaves. There are Keynesians, post-Keynesians, neo-Keynesians, New Keynesians, new classicals, monetarists, rational expectations theorists, real business cycle theorists, supply siders, and so forth. Given that situation, it is not an accurate reflection of the state of economic learning to say that there is a body of current theory which incorporates all good ideas into a generally accepted body of macroeconomic thought. Moreover, there have always been objections to macroeconomic theory itself on the part of some neoclassical economists (for example, Marget 1938–42; Knight 1937) and of others who do not believe that macroeconomic functions can represent economic reality because of the problem of aggregation.

Similarly, with respect to microeconomics, it is incorrect to say that there is a single accepted body of current economic theory into which all valid contributions have been gathered. It is often said that 'the' theory of general equilibrium is the core or centrepiece of microeconomic theory, but in fact there is no such thing. There are many different incompatible general equilibrium models, each portraying a different hypothetical economy, and none of them portraying the actual economy. There are models of perfectly competitive economies, and models in which the firms are imperfectly competitive. In some models, the economy is assumed to be always in equilibrium. In some models, there is disequilibrium but no disequilibrium transactions or disequilibrium production. In others, there are those disequilibrium phenomena. There are models in which there is no money, and models in which money is used. Some theorists believe there are no models which treat money and markets adequately (Clower and Howitt 1996). For each of those general types of models there are a wide variety of special models that differ in significant detailed respects (see Walker 1997).

The Italo-Cambridge school does not accept the validity of general equilibrium theory as a method, replaces it with sequence analysis, rejects marginalism, makes no use of supply and demand functions, and regards many of the modern neoclassical analyses as erroneous (Garegnani 1990). There is hardly any need to add that the theories of the behaviour of the economy held by heterodox economists (see Prychitko 1977), such as institutionalists, Marxists and modern Austrians, differ greatly from the microeconomic general equilibrium theories and have nothing in common with the macroeconomic dynamics that appear, for example, in the journal devoted to that subject (Barnett 1977). Mention of their ideas emphasises the diversity of views that exists about the behaviour of consumers, firms, markets, and the role of government. Moreover, for those types of economists, the influence of economics written in the past upon their present ideas about economic behaviour is abundantly evident.

The fact that there is no monolithic ‘current economic theory’, but rather ‘current economic theories’ about any given subject profoundly affects the terms of the discussion of the relevance of past economic theories to present economics. Since there is no generally accepted body of present doctrine, no general agreement on what ideas are worthwhile, it cannot accurately be said that a body of current economics, generally regarded as the received state of knowledge, incorporates all worthwhile economic ideas.

A second point of view is that the different economic theories that have been held at different times, including the present, are not intrinsically right or wrong. At some time in the past a problem was identified, either an aspect of the real economy for which an explanation was desired or a problem in the logical structure of an economic theory. Analysis of the problem led to a new or modified theory. It may be that the situation to which the theory was a response no longer exists, but the theory is not thereby invalidated. A theory may not be espoused by anyone currently, but that is simply because attention has shifted to subjects other than those with which it deals:

> what, in economics, to select for special attention partly depends on circumstances – on the world environment, factual knowledge and our particular interests at any time (Hicks 1976). From this it follows that an economic theory may be ‘rejected’ because it is inappropriate for the particular problems with which we are currently concerned, rather than because it is ‘wrong’. It may often appear when there is a change in economic thought that one theory has displaced another because the first has somehow been proved, in some definitive sense, faulty, when nothing more than a change in ‘concentration of attention’ is involved.
>
> (Hollander 1987: 1)

According to this view, the past is a storehouse of different theories, each of which is potentially useful. It is a repository of the economic reasoning of many capable minds and some brilliant minds. Present-day economists can draw upon that rich heritage. The theory or model developed about a past situation is put aside until the time when it will be used as a source of inspiration or as the beginnings of a body of thought that will be constructed to deal with a similar sort of present situation.

One merit of the second point of view is that it does not postulate the existence of a single body of accepted economic theory. There are, however, some problems with the point of view if it is interpreted as maintaining that all past theories are potentially of some value. Is it true that those models are put on the shelf in the storehouse of economic theories awaiting a time when the situation with which they deal reoccurs and they once more become applicable? The answer depends partially on the type of model under consideration. Empirical models become outmoded when the economy changes in the respects that they model. A model which was directed at the understanding of a specific past empirical situation and correctly explains it does not become wrong in relation to that situation after it passes but it becomes largely irrelevant to explain new situations. Some of the elements of the model may be reused, but they cannot be applicable without changes in the overall model in which they appear because the economy does not recreate past situations. For example, the structure of the economy that existed during the 1929–36 depression in the United States and the events that took place will never occur again. If there were a correct model of that depression, it would never again be applicable because it would be a model of the structural features and economic behaviour of that particular time. W. S. Jevons's estimation of the periodicity of business cycles, for example, was strongly influenced by the data that he collected on prices and the way he analysed them. His data, it can now be shown, was inadequate as a representation of economic activity and his statistical techniques were comparatively primitive and will never be used again.

The further back in time that an empirical model was developed, the less relevant it is. An example is Léon Walras's model of the behaviour of a bimetallic monetary system (L. Walras 1881, 1889: 387–450). He used it to analyse a variety of monetary questions in an economy in which the circulating media were silver and gold. That may be a valid model of the monetary aspects of the economy of his time, but not of the modern economy. The successive monetary models that were constructed after Walras's time are increasingly relevant as explanations of the modern economy. That is not to say that one can identify a particular past monetary model which was the immediate progenitor of modern monetary modelling, because there are a number of different current theories of the role of money in the modern economy. Nevertheless, the most relevant empirical models are fairly recent. This is not only because they relate to an economy that is structurally and behaviourally substantially like the modern economy, but also because the modellers who have worked in recent years have been able to choose among a wider range of modelling techniques, economic concepts and tools. Moreover, they have been able to rely on better empirical information and to use computers to deal with large amounts of information and complex equation systems.

It may be argued that even though past situations are not exactly duplicated in the present, there are similarities between past and present empirical situations. Inflation has occurred many times in the past and may occur in the present. It may be argued, therefore, that a model that attempted to explain inflation in the United States in 1865 could be useful in explaining inflation in 1919, 1940–1, 1946–8, 1950–1, 1958–9, 1974–5, 1978–80 and in some subsequent period. The difficulty is, again, that for a theory to explain an empirical situation, the theory must contain structural and behavioural information drawn from the economy of the time. Otherwise, a general statement like '*ceteris paribus*, inflation is caused by increases in the quantity of money that are excessive in relation to increases in the quantity of commodities' is useless.

At any time in the past, there have been different theories about a given subject matter, just as at present. What is under discussion is not theories that differ because they have different subject matters, but theories about a particular aspect of economics. For example, the different theories of the source of interest that existed during the time of Eugen von Böhm-Bawerk and which were described and evaluated by him (Böhm-Bawerk 1884), and the theory that he proposed, were all different explanations of the same phenomenon. Thomas Malthus and David Ricardo held opposing theories on many topics. The controversy regarding the wage-fund doctrine in the third quarter of the nineteenth century is another example of conflicting views. In the 1920s and 1930s there were many incompatible theories of the depression that was occurring, among which were the theories that its cause was purely monetary, monetary over-investment, non-monetary over-investment, capital shortages, disproportionate investment, cost changes, horizontal maladjustments, over-indebtedness, under-consumption, psychological, and harvest variations (see Haberler 1937; Hutchison 1953: 334–408). Irving Fisher (1932, 1936) and J. M. Keynes (1936), for example, developed different theories about the great depression. To evaluate the competing theories, each represented by their supporters as being the exclusive explanation of one and the same set of economic events, the historian of thought has to study and evaluate the late 1920s and the 1930s in the United Kingdom and the 1930s in the United States – he has to study economic history or he has to leave that job to an economic historian.

Contradictory theories cannot all be true. In short, some past theories were erroneous. Some of the erroneous theories were stepping-stones to reach a better theory and some were stepping-stones to increased confusion. For example, the labour theory of value was wrong and led to confusion. Like many other past doctrines, it will never be adopted again and used to explain the pricing of commodities. W. S. Jevons's sunspot model of business fluctuations was wrong, and it will never become relevant for an explanation of them. The virtual perfectly competitive general equilibrium models and proofs of existence of the period 1936–71 had no relation to the real economy, were never useful for understanding or affecting it, and will never be used to explain its behaviour. In ardently supporting the study of the history of economic thought, one does not want to be thought to be arguing that the different incompatible theories were all equally valid regarding the same subject matter. The storehouse of the past contains many notions that will be left there.

Why the history of economic thought is relevant for the content of current economics

First, economists can learn useful information from a study of past doctrine. In the words of J. R. McCulloch (1845: vi–vii), 'whether a writer or a speaker undertakes to unfold principles, to set them in a novel and more striking light, or to recommend their application, he should know what has been already undertaken, what has been accomplished, and what remains for discovery and elucidation'. McCulloch went on to give 'sundry examples of the inconveniences resulting from the want of this information, by exhibiting able men engaged in the investigation of principles and the development of laws which had been previously established and traced, and putting forward speculations as original which had been long before the public' (ibid.). Edgeworth argued the point this way:

> I am unable to devote proportionate attention to the advantages of historical studies. But you will not expect me to expatiate upon advantages which are known to most of you from personal experience. I will only advert to a secondary and less obvious benefit attending historical researches. To trace the affiliation of ideas in the progress of science is calculated to correct our estimates of authority: to reduce in general the extravagant regard which the youthful student is apt to entertain for contemporary leaders, and to assign due weight to real originality.
>
> (Edgeworth 1891: 11)

J. A. Schumpeter emphasised some other kinds of useful information that are obtained by the study of the history of economics: 'We learn about both the futility and the fertility of controversies; about detours, wasted efforts, and blind alleys; about spells of arrested growth, about our dependence on chance, about how not to do things, about leeways to make up for. We learn to understand why we are as far as we actually are and also why we are not further. And we learn *what succeeds and how and why*' (Schumpeter 1954: 5).

Second, it has sometimes happened that ideas that were superior to the state of knowledge in the received doctrine were neglected. A failure to study the history of thought can cause

> oblivion of an older and a higher theory; so in political economy the theory which explains value by utility – utility in the sense defined by Galvanize – has so fascinated by no means the worst sort of economists, that they have almost forgotten, or at least degraded, the older, and in some respects more important theory which connects value with sacrifice and labour. There is ever a danger that, as we press on to seize new conceptions, we should lose the positions which have been already won. Hence the history of theory is particularly instructive in dialectics, and all that the Greeks comprehensively called *words*, seem the best corrective of the narrow prejudices and deceptive associations which are sure to be contracted by those who have been confined to a single school or system.
>
> (Edgeworth 1891: 6)

Theories of a general nature that have been neglected, as distinct from empirical models, may become useful again, inevitably with some modification:

> It is important, therefore, if this is so, to try to discover whether, how and why changes in concentration have occurred over time. Not to do so is to remain under the possibly false impression that what one has learned in basic theory courses is all there is to know; that earlier perspectives have been definitively disposed of as erroneous, when there is much that remains 'valid' albeit forgotten or neglected by the profession.

> There is good reason to support this view. A number of instances illustrate that theoretical constructs, lost from sight, have indeed proved to be of permanent usefulness as old problems re-emerge.
>
> (Hollander 1987: 1)

Economists who have some knowledge of its history do not accept the idea that the entire work of evaluation of a theory was done at the time of its original dissemination because the record shows that has not been true. The best theory is often not incorporated into the most recent theory. It may be that economists – either those who held professorships in prestigious universities or heterodox economists – wrongly rejected a theory, so that what was at that time the most recent doctrine received by one or another school of thought did not in fact contain all that was worthwhile. One type of neglected economist is 'the man whose ideas, no matter how original and brilliant judged by the standards of today, had little if any impact on the thought of his own day. Not time but his contemporaries neglected him' (Fetter 1965: 138). Of course, there were also economists who influenced their contemporaries and who were subsequently forgotten but their situation is not relevant for the present chapter. Of interest here are ideas that were neglected for a period of time but subsequently were used in the construction of economic theories. In fact, the record shows that it has been through studying the history of thought that neglected ideas have been uncovered, understood, and evaluated.

An example is the historical studies that led to an appreciation of the neglected ideas of Antoine Augustin Cournot in the book that he published in 1838. It was only many years after that date that his innovations in method and in regard to demand, pricing, and market structures were studied by Alfred Marshall (1890: x, 1920: ix–x) and Léon Walras (1900: VIII, 1905, 1988: 5, 1965, I: 5), recognised as being valuable, and used in their theoretical work. Similarly, after many years of virtually total neglect, Irving Fisher's macroeconomics and monetary ideas began to be studied and utilised by modern economists (Dimand 1998). The constant interplay between past and present economic studies becomes evident. Past ideas come to be used in the formulation of current theory and the understanding of past ideas requires the application of present economics to achieve a clear expression and satisfactory evaluation of them. The latter process occurred, for example, when J. M. Keynes reinterpreted and re-evaluated mercantilist doctrines (Keynes 1936: 333–50). It is a process that may be repeated many times with different results in reference to a particular set of past writings because current theories change and so our perception of the character of past theory and our judgements on it change. Thus Keynes's views on mercantilist doctrine have themselves in turn been recently scrutinised (Walker 1986: 15–29), and the doctrines have subsequently been interpreted and evaluated again (Magnusson 1993).

Neglect of valuable ideas is what Keynes argued was the case in the rejection of Thomas Malthus's theory that general over-production can occur and the acceptance of David Ricardo's contrary view (Keynes 1922/33, X: 98–102; see Walker 1986: 4–5). Another example is that today we do not believe that Keynes