1 The history of economic thought and its role

> To understand the others: this is the historian's aim. It is not easy to have a more difficult task. It is difficult to have a more interesting one.
> (Kula 1958, p. 234)

1. Introduction

The thesis advanced in this chapter is that the history of economic thought is essential for anyone interested in understanding how economies work. Thus economists, precisely as producers and users of economic theories, should study and practise the history of economic thought. While illustrating this thesis, we will examine some questions of method that, apart from their intrinsic interest, may help in understanding our line of reasoning in this book.

Our thesis is opposed to the approach now prevailing. Most contemporary economists, especially in Anglo-Saxon countries, are convinced that looking back may perhaps be of some use in training young economists, but is not necessary for the progress of research, which rather requires work on the theoretical frontier.

In the next section we will consider the foundations of this approach, also known as 'the cumulative view' of the development of economic thought. We shall see how, even in this apparently hostile context, a crucial role has been claimed for the history of economic thought.

The cumulative view has been opposed by other ideas on the path pursued by scientific research. In section 3 we take a look at the theses on the existence of discontinuities (Kuhn's 'scientific revolutions') or competition among different 'scientific research programmes' (Lakatos). As we shall see, they point to the existence of different views of the world, and hence of different ways of conceiving and defining the problems to be subjected to theoretical enquiry.

In section 4 we will recall the distinction, proposed by Schumpeter, between two different stages in the work process of the economic theorist:

first, the stage of construction of a system of concepts to represent the economy and, second, the stage of construction of models. In section 5, we then go on to see how this distinction points to an important, but generally overlooked, role for the history of economic thought within the very field of economic theory, as a way to investigate the conceptual foundations of different theories.

All this constitutes the background for discussing, in section 6, the kind of history of economic thought which is most relevant for the formation of economic theories. Obviously, this is not to deny that there is intrinsic interest in research into the history of ideas: far from it! Nor will we consider issues such as the autonomy of the history of economic thought or whether, in the division of intellectual work, historians of economic thought should be considered closer to the economists or to the economic historians. The point we wish to make is that economists who refuse to get involved in the study of the history of economic thought and to have some research experience in this field are severely handicapped in their own theoretical work.

2. The cumulative view

According to the cumulative view, the history of economic thought displays a progressive rise to ever higher levels of understanding of economic reality. The provisional point of arrival of today's economists – contemporary economic theory – incorporates all previous contributions.

The cumulative view is connected to positivism.[1] More specifically, the most widespread version of the cumulative view draws on a simplified version of logical positivism, the so-called 'received view', which found a considerable following as from the 1920s. In a nutshell, the idea was that scientists work by applying the methods of logical analysis to the raw material provided by empirical experience. To evaluate their results, objective criteria for acceptance or rejection can be established. More

[1] An illustrious and characteristically radical example of this position is represented by Pantaleoni 1898. According to him, the history of thought must be 'history of economic truths' (ibid., p. 217): 'its only purpose [. . .] is to relate the origins of true doctrines' (ibid., p. 234). In fact Pantaleoni held that a clear-cut criterion for judging the truth or falsehood of economic theories is available: 'There has been a troublesome search for hypotheses that are both clear and in conformity with reality [. . .] Facts and hypotheses have then been used, and what could be deduced from them has been deduced. The theorems have also been checked on empirical reality' (ibid., p. 217). Expressed in these terms, Pantaleoni's criterion mirrors a still rather primitive and simplistic version of positivism; the resolution with which it is stated probably stems at least in part from the harshness of the controversy between the Austrian marginalist school and the German historical school (cf. below, § 11.2).

precisely, *analytic statements*, namely those concerning abstract theoretical reasoning, are either tautological, i.e. logically implied in the assumptions, or self-contradictory, i.e. they contain logical inconsistencies; in the former case, the analytic statement is accepted, in the latter rejected. Similarly, *synthetic statements*, i.e. those concerning the empirical world, are either confirmed or contradicted by evidence, and hence accepted or rejected for 'objective' reasons. All other statements for which no analogous criteria of acceptance or rejection can be found are termed *metaphysical* and are considered external to the field of science.

This view has come in for severe criticism, discussed in the following section.[2] Nevertheless it remains the basis for the cumulative view of economic science, or, in other words, the idea that each successive generation of economists contributes new analytic or synthetic propositions to the common treasure of economic science, which – as a science – is univocally defined as the set of 'true' propositions concerning economic matters. New knowledge is thus added to that already available, and in many cases – whenever some defect is identified in previously accepted statements – substitutes it. Hence, study of a science must be conducted 'on the theoretical frontier', taking into consideration the most up-to-date version and not the theories of the past. Notwithstanding this position, it is granted that the latter may deserve some attention: as Schumpeter (1954, p. 4) says, studying economists of the past is pedagogically helpful, may prompt new ideas and affords useful material on the methods of scientific research in a complex and interesting field such as economics, on the borderline between natural and social sciences.

Similar arguments are proposed by various other historians of economic thought, often in a simplistic way and with rhetorical overtones. However, as Gordon (1965, pp. 121–2) points out, the fact that the history of economic thought may help in learning economic theory is not a sufficient reason to study it. Given the limited time available to human beings, one would also have to show that a course of lectures dedicated to the history of economic thought contributes more to the formation of an economist than an equal amount of time directly dedicated to economic theory. Clearly, if we accept a cumulative view of economic research, this would be rather difficult to maintain. As a consequence, according to Gordon (1965, p. 126), 'economic theory [. . .] finds no necessity for including its history as a part of professional training' (which does not mean that the history of economic thought should be abandoned: 'We study history because it is there').

[2] For a survey of this debate, see Caldwell 1982 and, more recently, Hands 2001; for the link between the 'received view' in epistemology and the cumulative view in the history of economic thought, see Cesarano 1983, p. 66.

Interest in the history of economic thought, when justified by its pedagogical usefulness, is reduced whenever the development of economics sees discontinuity in the analytical toolbox. This is how some authors explain the waning interest in the history of economic thought as from the 1940s.[3] However, we may recall that as early as the 1930s economists such as Hicks and Robertson were arguing that there was no reason to waste time reading the classical economists;[4] their attitude is explained not so much by change in the analytical toolbox as by change in the very conception of economics, from the classical (surplus) approach to the marginalist (scarcity) view.

Among adherents of the cumulative view, Viner proposes a subtle defence of the history of economic thought, only apparently modest. Viner points to 'scholarship', defined as 'the pursuit of broad and exact knowledge of the history of the working of the human mind as revealed in written records'. Scholarship, although considered inferior to theoretical activity, contributes to the education of researchers, being 'a commitment to the pursuit of knowledge and understanding': 'once the taste for it has been aroused, it gives a sense of largeness even to one's small quests, and a sense of fullness even to the small answers [. . .] a sense which can never in any other way be attained'.[5]

Education in research, Viner seems to suggest, is a prerequisite for exploitation of the knowledge of analytical tools.[6] Thus, even if the history of economic thought is considered to be of little use in learning modern economic theory, a crucial role is attributed to it in the education of the researcher. The importance of this wider perspective becomes much clearer, however, outside a strictly cumulative view of economic research, as we shall see below.

First, however, it is worth stressing that the cumulative view of the history of economic thought considered in this section is the modern one, which reached a commanding position in the twentieth century parallel with the marginalist approach. A somewhat different kind of cumulative view can be found in the brief digressions on the history of economic thought made by certain leading economists such as Smith and Keynes, who use them to highlight their own theories, contrasting them to those prevailing previously. Thus Smith, in book four of *The wealth of*

[3] Cf. Cesarano 1983, p. 69, who also refers to Bronfenbrenner 1966 and Tarascio 1971.
[4] Letter by Robertson to Keynes, 3 February 1935, in Keynes 1973, vol. 13, p. 504; and letter by Hicks, 9 April 1937, in Keynes 1973, vol. 14, p. 81.
[5] Viner 1991, pp. 385 and 390.
[6] Schumpeter (1954, p. 4; italics in the original) says something similar when stating that the history of economic thought 'will prevent a sense of *lacking direction and meaning* from spreading among the students'.

nations, criticises the 'commercial or mercantile system' and the 'agricultural system' (namely the physiocrats). The critique of the mercantilists – an abstract category, devised in order to place under a single label a long series of writers who are often quite different from one another (cf. below, § 2.6) – goes hand in hand with Smith's liberalism, illustrated in other parts of his work; the critique of the physiocrats serves to stress, by contrast, his own distinction between productive and unproductive workers and his tri-partition of society into the classes of workers, capitalists and landowners. Similarly, Marx contrasts his 'scientific socialism' to 'bourgeois' economics (that of Smith and Ricardo) and 'vulgar' economics (that of Say and of Bastiat's 'economic harmonies'); Keynes creates a category – the 'classics' – in which he includes all previous authors who, like his Cambridge colleague Pigou, exclude the possibility of persistent unemployment that is not reabsorbed by the automatic forces of competitive markets. Clearly, we are not confronted here with instances of cumulative views stressing the gradual accumulation of economic knowledge, but rather with historical reconstructions by means of which certain protagonists of economic science stress the leap forward accomplished by their discipline thanks to their own theoretical contribution. Obviously, recalling this fact is not to deny the validity of such historical reconstructions, since in the case of protagonists like Smith or Keynes these reconstructions do identify key steps in the path of economic science.

3. The competitive view

Over the past few decades a number of economists have referred to Kuhn's (1962) 'scientific revolutions' or Lakatos's (1970, 1978) 'scientific research programmes' in support of the idea that it is impossible to choose among competing theoretical approaches with the 'objective' criteria indicated by logical positivism (logical consistency, correspondence of assumptions to empirical reality).

These criteria had already been the object of debate. Some criticisms specifically concerned the clear-cut distinction between analytic and synthetic statements. Indeed, analytic statements, if interpreted as purely logical propositions, are devoid of any reference to the real world; as a consequence, they are empty from the point of view of the interpretation of real-world phenomena.[7] Synthetic statements in turn necessarily embody a large mass of theoretical elements in the very definition of the

[7] In other terms, observations are necessarily 'theory-laden'; cf. Hands 2001, pp. 103 ff. It is on this ground, for instance, that Dobb (1973, ch. 1) develops his critique of the excessively clear-cut distinction, proposed by Schumpeter, between history of economic analysis and history of economic thought, to which we will come back later on (§ 5).

categories used for collecting the empirical data and in the methods by which these data are treated; as a consequence, the choices of acceptance or rejection of any synthetic statement cannot be clear-cut, but are conditioned by a long series of theoretical hypotheses that cannot, however, be subject to separate evaluation.[8] It is precisely the impossibility to have neatly separate evaluations based on univocal objective criteria for analytic and synthetic statements that constitutes a crucial difficulty for the positivistic view discussed in the previous section.

Another important critique of the criterion for acceptance or rejection proposed for synthetic statements – their correspondence or non-correspondence to the real world – is developed by Popper (1934). No matter how many times a synthetic statement is corroborated by checking it against the real world, says Popper, we cannot exclude the possibility that a contrary case will eventually crop up. Thus, for instance, the statement that 'all swans are white' may be contradicted by the discovery of a single new species of black swans in Australia. The scientist cannot pretend to verify a theory, that is to demonstrate it to be true once and for all. The scientist can only accept a theory provisionally, bearing in mind the possibility that it may be falsified, or, in other words, that it be shown to be false by a new-found empirical event contradicting it. Indeed, in a subsequent book (1969) Popper maintains that the best method for scientific research consists precisely in the formulation of a potentially never-ending series of 'conjectures and refutations'. In other words, the scientist formulates hypotheses and then, rather than looking for empirical confirmation – which in any case could not be definitive – should rather seek out refutations. These, by stimulating and guiding the search for better hypotheses, make a crucial contribution to the advancement of science.[9]

A number of leading figures of positivistic epistemology maintain that it is not applicable to the field of social sciences. The influence of some historians and philosophers of science, such as Kuhn, Lakatos and Feyerabend, contributed then, in the last decades of the twentieth century, to abandonment of the positivistic methodology in the field of economic theory. Let us briefly recall their theories and the competitive view of science that follows from them.

In a few words, according to Kuhn, the development of science is not linear, but can be subdivided into stages, each with its own distinctive

[8] This criticism is known as the 'Duhem–Quine underdetermination thesis' (cf. Quine 1951); according to it, 'no theory is ever tested in isolation', so that 'any scientific theory can be immunized against refuting empirical evidence' (Hands 2001, p. 96).

[9] For debate on the utilisation of Popper's ideas in the field of economic theory, cf. De Marchi 1998.

characteristics. In each period of 'normal science', a specific point of view (paradigm) is commonly accepted as the basis for scientific research. On such a basis, an ever more complex theoretical system is built, capable of explaining an increasing number of phenomena. This process of growth of normal science, however, is accompanied by the accumulation of anomalies, namely of phenomena that are either unexplained or that require for their explanation an increasingly heavy load of ad hoc assumptions. A growing malaise derives from this, which favours a 'scientific revolution', namely the proposal of a new paradigm. This marks the beginning of a new stage of normal science, within which research proceeds without calling into question the underlying paradigm.

Let us stress here that Kuhn does not consider the succession of different paradigms as a logical sequence characterised by a growing amount of knowledge. The different paradigms are considered as not commensurable among themselves; each of them constitutes a different key for interpreting reality, necessarily based on a specific set of simplifying assumptions, many of which also remain implicit. No paradigm can encompass the whole universe in all its details. Strictly speaking, it is incorrect both to say that the earth goes round the sun and that the sun goes round the earth: each of the two hypotheses corresponds to the choice of a fixed point as reference for the study of the universe, or better a part of the universe that is in continuous movement relative to any other possible fixed point. In other words, since both the earth and the sun move in space, those of Copernicus and Ptolemy are but two alternative theoretical approaches which explain in more or less simple terms a greater or smaller number of phenomena.[10] We may also recall in this respect that a heliocentric view had already been proposed by Aristarchus of Samos in the third century BC, nearly five centuries before Ptolemy: thus, paradigms do not necessarily follow each other in a linear sequence, but can reappear as dominant after even long periods of eclipse.

[10] Among Kuhn's predecessors in this respect we may recall Adam Smith with his *History of astronomy* (Smith 1795). A connecting link between Smith and Kuhn might be located in Schumpeter, who sets apart the *History of astronomy* as 'the pearl' among Smith's writings (Schumpeter 1954, p. 182), and further on considers the same historical case that was later to be studied by Kuhn: 'The so-called Ptolemaic system of astronomy was not simply "wrong". It accounted satisfactorily for a great mass of observations. And as observations accumulated that did not, at first sight, accord with it, astronomers devised additional hypotheses that brought the recalcitrant facts, or part of them, within the fold of the system' (Schumpeter 1954, p. 318 n.). Kuhn, like most of the protagonists of the epistemological debate, originally developed his ideas as an interpretation of the history of natural sciences, specifically astronomy and physics, and not as a methodological recipe for the social sciences. However, some at least among his ideas can be readily utilised in the field of economic theory. For an attempt in this direction, cf. the essays collected in Latsis 1976.

Kuhn presents his idea of scientific revolutions as a description of the path actually followed by the different sciences rather than as a normative model of behaviour for scientists. In opposition, a normative attitude is adopted by Lakatos (1978).

Lakatos's 'methodology of scientific research programmes' consists in a set of working rules for both critique and construction of theories (negative and positive heuristic), organised around a 'hard core' of hypotheses concerning a specific set of issues and utilised as foundations for the construction of a theoretical system. The hard core remains unchanged even when anomalies arise, thanks to a 'protective belt' of auxiliary hypotheses, and is abandoned only when the scientific research programme based on it is clearly recognised as 'regressive', or in other words when it is clearly recognised that going ahead with it is most likely a waste of time and effort. The acceptance or rejection of a scientific research programme is thus considered by Lakatos a complex process, and not an act of judgement based on a crucial experiment, or in any case on well-defined, univocal, objective criteria.

Thus interpreted, Lakatos's view is not very different from – although admittedly less radical than – that proposed by Feyerabend (1975) with his 'anarchistic theory of knowledge'. Feyerabend stresses the need for the utmost open-mindedness towards the most disparate research approaches; at the same time he is far from accepting without qualification his own motto: 'Anything can go'. Critique of the idea that there exist absolute criteria of truth (or better of acceptance and rejection of theories) may coexist with the idea of the practicability of a rational debate between different, even conflicting, points of view. Obviously, when debating the different viewpoints the advocates of each should be ready to drop the pretence of using as absolute the criteria of judgement based on their own world-view. On the contrary, provisionally adopting the rival viewpoint to criticise it from inside may constitute an element of strength in the debate. We are thus confronted with a procedure for scientific debate analogous to that commonly followed in legal proceedings, where prosecutor and defence each brings the most disparate arguments in support of their positions.

Feyerabend's views were brought into the economic debate by McCloskey (1985, 1994), albeit with some changes. McCloskey speaks of a 'rhetorical method of scientific debate' that rejects neat, monodimensional criteria for the evaluation of theories, and stresses, in contrast, the role of their relative power of persuasion.[11] This does not mean

[11] Within the field of the natural sciences, well-conducted experiments as a rule constitute decisive proof of the superiority of one theory over other theories. In the field of the social sciences, however, experiments performed in controlled conditions (that is,

to deny any value to the theoretical debate: far from it, the main message given by this methodology is the need for tolerance in the face of different views of the world and hence of different theoretical approaches. We may also recall that thus interpreted the rhetorical method in economics can be traced back to Adam Smith.[12]

In the case of Kuhn and Lakatos alike, economists have been attracted by the role attributed to the existence of alternative approaches, deduced from the succession of different paradigms or from the coexistence of different scientific research programmes.[13] Obviously Feyerabend's ideas lead in the same direction.

It is here that the history of economic thought comes into play. Those who accept a competitive view of the development of economic thought and participate in a debate between contending approaches are induced to investigate the history of such a debate, looking for the points of strength and weakness which explain the dominance or decline of the different approaches.

In particular, those who support approaches competing with the dominant one may find the history of economic thought very useful.[14] First, analysis of the writings of economists in the past often helps in clarifying the basic characteristics of the approach being proposed and the differences between it and the dominant one.[15]

Second, the history of economic thought helps in evaluating theories based on different approaches, by bringing to light the world-views, the content of the concepts and hypotheses on which they are based. Often this helps in retrieving the notes of caution and the qualifications originally accompanying the analysis, subsequently forgotten in unwarranted generalisation of the field of application of the theory.[16]

Third, recalling illustrious cultural roots sometimes serves a tactical purpose, in order to counter the inertia that constitutes such a strong

ceteris paribus) are practically impossible. Hence the greater complexity in this latter field for comparison between different theories.

[12] We refer here not only to the *Lectures on rhetoric and belles lettres* (Smith 1983), but also to the *Glasgow lectures* (the so-called *Lectures on jurisprudence*: Smith 1978). On this subject, cf. Giuliani 1997.

[13] See, for instance, the essays collected in De Marchi and Blaug 1991. For a note of caution, see Steedman 1991, who notes that Lakatos's programmes refer to specific issues rather than to broad views of the world.

[14] Cf. Dobb 1973, Meek 1977 and Bharadwaj 1989 as examples of this interest following the Sraffian revival of the classical approach.

[15] An illustrious example is Sraffa's edition of Ricardo's *Works and correspondence* (Ricardo 1951–5).

[16] An example is the assumption of market clearing. It implies markets that work in a very specific way, either like the 'call bid' markets of old-style continental stock exchanges, or like the 'continuous auction' markets of Anglo-Saxon stock exchanges. Kregel 1992 considers the former in relation to Walrasian general equilibrium theory, and the latter referring to Marshallian theory. Cf. below, chapters 12 and 13.

advantage for the prevailing mainstream. Obviously an appeal to authority does not constitute a good scientific argument; this is also true for the appeal to a majority rule, a proclamation of intellectual laziness so often repeated in defence, for instance, of the persistent use of one-commodity models in theories of employment and growth, or of U-shaped curves in the theory of the firm.

It may be useful to stress here that the competitive view implies neither an equivalence between competing approaches, nor the absence of scientific progress.[17] It simply implies recognition of the existence of different approaches based on different conceptual foundations. Each researcher generally follows the line of research which he or she considers the most promising one.[18] Such a choice, however, is extremely complex, because of the incommensurability of the different conceptual systems. In particular, the claim of the mainstream approach to impose evaluation criteria derived from its own views must be rejected.

What the competitive view specifically rejects is the idea of a monodimensional process of scientific advance. There can be progress both within each approach (where indeed it is the general rule, in terms of both greater internal consistency and higher explanatory power) and along the historical sequence of research paradigms or programmes. In the latter case, however, the idea of progress is more imprecise and greater caution is required. An undeniable element of progress is provided by the increasing number of ever more sophisticated analytical tools made available by developments in other fields of research (new mathematical tools, better and more abundant statistical material, higher computing power from new computers). But between successive research paradigms or programmes there are commonly crucial differences in the underlying worldview. Some aspects of reality (cause and effect relationships included) are given greater prominence, others less, so that there are differences in the set of (explicit or implicit)[19] assumptions on which theories are

[17] This opinion – the rejection of any idea of scientific progress – is sometimes attributed to Feyerabend's 'anarchistic theory of knowledge' and, within the economic field, to McCloskey's (1985) 'rhetoric'. However, this opinion does not necessarily follow from their main points, the rejection of clear-cut and univocal criteria for assessment of different theories and research programmes, and the proposal of an open – and morally serious – 'conversation' among differently oriented researchers.

[18] That is, if we exclude instances of career-oriented opportunistic choices, which sometimes explain adhesion to the mainstream.

[19] The assumptions will necessarily remain at least in part implicit: a full list of the elements of reality abstracted away in the process of building a theory (that is, elements not taken into account in the theory because they are considered not important for the issue under examination) is impossible. In this sense, axiomatic models rely on a limited number of explicit assumptions but – a fact all too often overlooked – they crucially imply a large, potentially unlimited, number of implicit simplifying assumptions when an attempt is made to relate them to the economic reality which they set out to interpret.