

ITALIAN-STYLE PLURALISM IN ECONOMICS¹

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1. Methodological pluralism, theory appraisal, and history in economics.

1.1 An interest in the methodology of a scientific subject may be justified on various grounds. First, and perhaps more naturally, it may be the result of a concern in the local epistemology of a specific branch of knowledge, by a practitioner of that science. Second, it may be legitimately linked to the search for information by a general epistemologist wishing to obtain evidence on the logical structure of a scientific subject, for cognitive purposes. A third and somewhat less legitimate reason for interest is that of the philosopher of science who ascribes himself the task of prescribing methodological rules to scientists; even if he knows that they may resent this attitude and regard it as a serious impediment in the search for methodological pluralism.

Some aspects of the methodological problem will be discussed in this paper, from a historical-critical perspective, with specific reference to economic science as practised in this country². The purpose is both to understand why there are different positions in the matter of method in the philosophy and in the historiography of economics, two scientific subjects which explore the same object of study from various points of view, and also to establish whether the contrast can be overcome and on which conditions. Our hope is to contribute to the transformation of the philosophy of economics into a non-prescriptive philosophy of history of economics.

1.2. Professional relationships between economists and philosophers have been up to now rather disappointing. The two areas of interests seem to have grown far apart from one another. The influence exerted on economists by philosophers' analytical work has indeed been minimal and limited to the domains of logics and epistemology. A similar remark may be made about the impact of economic thought on philosophical speculation.

¹ This paper is the result of joint research. Duccio Cavalieri wrote the first section, Riccardo Faucci wrote the second section. As the reader will note, there are slight differences in interpreting pluralism in Italian economics. Thanks are due to Tiziano Raffaelli for his useful comments.

² For a more extended treatment of the subject, see D. Cavalieri, *Per una critica dei fondamenti della conoscenza economica: note sull'epistemologia e la storiografia dell'economia politica*, in G. Becattini, D. Cavalieri e S. Zamagni, *Nuovi approcci nella ricerca economica*, Congedo, Lecce, 1991, pp. 19-75.

The wrong appears to be equally distributed on both sides. The unfortunate idea of a pre-eminence of science on philosophy, one of the main points of logical empiricism, has probably done as much harm to the establishment of good relationships between economics and philosophy as the attempt to re-establish the importance of ideological and sociological elements in scientific explanation.

Half a century ago Joseph Schumpeter maintained that economics owed very little to the great philosophical streams of the Nineteenth and Twentieth centuries³. This was not an accurate statement, as it neglected the influence exerted on economics by utilitarianism, rationalism and other philosophical doctrines; but was in itself an indication that the methodological debate in economics had remained up to that time internal to a limited sector of the economic profession, without drawing the attention of philosophers of science.

This unfortunate situation does not seem to have changed to a great extent in recent times, in spite of some meritorious attempts to fill the gap (as the appearance in 1985 of the half-yearly journal *Economics and Philosophy*). It is still true that only a small number of economists cultivate philosophical studies (most of them belong to a single school of thought: the neo-Austrian one) and that few philosophers take more than an occasional interest in economics.

As regards in particular the situation in Italy, let us recall the personal responsibility of a great philosopher, historian and man of letters, Benedetto Croce, for the long breakdown in dialogue between philosophers and economists which took place in the period 1930-1950. Croce believed in an ontological hierarchy of importance of the various subject matters which study the human world. At the very top of the scale he placed historical knowledge; at the bottom, abstract human sciences, like pure economics. In a debate which took place at the beginning of the century in the *Giornale degli economisti*, Croce, objecting to Vilfredo Pareto's assimilation of pure economics to rational mechanics, argued that economics is a practical activity and economists should not indulge in theorizing, but should rather engage in computation (a non-scientific activity)⁴. This ungenerous statement by one of the most influential philosophers of the time marked the breaking off in Italy of the incipient dialogue between epistemologists and economists. (For further details, see the second chapter).

The minimal conditions for a dialogue would not be re-established until half a century later, after the second world war, when Italian culture, in its broadest sense, succeeded in freeing

³ J.A. Schumpeter, *History of Economic Analysis*, Oxford Univ. Press, New York, 1954, vol. I, ch. 3.

⁴ See B. Croce, "Sul principio economico. Lettera al prof. Vilfredo Pareto", *Giornale degli economisti*, 1900-II, pp. 15-26; V. Pareto, "Sul fenomeno economico. Lettera a Benedetto Croce", *ibidem*, 1900-II, pp. 139-62; and their rejoinders, *ibidem*, 1901-I, pp. 121-30 and 131-38.

itself from the Crocian strait jacket. A decisive impulse in this direction came, on the one hand, from resumption of an old tradition of critical studies in the Marxian theory of value (that of Loria, Labriola, Graziadei), with their twofold economic and philosophical characterization, and on the other from the diffusion in Italy of the philosophical works by Karl Popper, whose interpretation of science as an open critical process helped to reject the attempt to reduce economics to a simple technique of computation and prediction.

1.3. Turning now to pluralism in economics, let me first try to define this concept and to distinguish between methodological and theoretical pluralism. Methodological pluralism is the idea that there is no single infallible method for doing scientific research. As such, it should not be confused either with theoretical pluralism, which may be understood as a theorist's willingness to accept criticism and dialectical competition among theories, or with theoretical eclecticism, the tendency to draw one's views from various systems of thought.

Methodological pluralism is simply the opposite of methodological monism, the view that there is just one correct scientific methodology. It implies a refusal to recognize two mystifying conceptions held by neopositivistic epistemology: the existence of a prescriptive scientific methodology, of general application, and of an objective criterion suited to demarcate science from non-science. From a positive heuristic point of view, however, it is not easy to characterize methodological pluralism as a definite proposal. The point is that pluralism is not a programme for action, but a quest for tolerance, for the abandonment of a single prescriptive methodology.

As regards the relationship of methodological pluralism with methodological individualism and methodological holism, it is perhaps less easy to take a definite position. In principle pluralism might be reconciled with non-prescriptive versions of both such conceptions. But it is somewhat problematic to think of non-prescriptive formulations of an epistemic view according to which all economic theories should be grounded on the analysis of an individual behaviour subject to deterministic laws. The antithetic "gestaltic" conception which assumes that the behaviour of aggregate variables cannot be reduced without residuals to those of their alleged individual components seems more acceptable. Both holists and pluralists thus reject the neoclassical tenet by which only those macroeconomic propositions which are suitable to be aggregatively founded on microeconomics should be validated. But in a social science like economics the unit components of an aggregate do not behave in a deterministic way, because of the absence of a teleologic or mechanistic order. Economic aggregates cannot therefore be reduced without residuals to an analysis of the underlying microeconomic variables.

1.4. The fundamental question, for pluralists, is not who should fix the methodological rules of the game, but which rules, if any, should regulate the growth of a critical knowledge. By critical knowledge I mean consciousness that economic phenomena are not invariant with respect to time and to the different forms of institutional organization.

Empirical evidence is conclusive in showing that any kind of economic knowledge is historically relative. Thus the rejection of the neoclassical faith in the general applicability of a "pure" economic theory is not an ontological thesis, an undemonstrable a priori synthetic proposition, but a legitimate conclusion, derived from economic experience.

When associated, two of the basic premises underlying the neoclassical idea of a pure economic theory - the utilitarian hypothesis and the rationality assumption - provide a powerful analytical tool, because they imply a maximizing individual behaviour. This is not simply a prescribed behavioural law in normative economics, but also a testable prediction, which can easily be checked. Everyone can thus realize that man is not a calculating machine, but an unpredictable mixture of situational rationality and passion.

An additional difficulty derives from a third neoclassical assumption: that of a perfect market structure, capable of supplying through a competitive price system all the information economic agents need for their calculus, at zero cost. The trouble is that market prices offer no valid support for strategic decisions in the face of uncertainty. Because of the presence of imperfections, indivisibilities and externalities, real markets are not reliable indicators of relative scarcities.

All this does not mean that the neoclassical theory, in its "purest" form, has to be set aside as a wholly irrelevant doctrine; rather, it has limited explanatory and predictive power. Economic purism and marginal analysis may however retain an important role to perform in normative microeconomics, where both methodological individualism and the rationality assumption make more sense.

1.5. A compromising epistemic solution, suited to reconcile to some extent the different points of view, is offered by the particular type of methodological individualism which holds to the neoclassical hypothesis of complete individual rationality, but interprets it in a non deterministic sense, by admitting the possibility that the real behaviour of economic agents may exhibit deviations from the rigid model postulated by the theory. There is at present a definite trend in this anti-scientist direction. Examples of this tendency are Karl Popper's "logic of the situation" and the methodological positions held by those authors who identify in a "situational determinism" the Lakatosian basis of the degenerating

research program of neoclassical economics, as concerns the theory of the firm.

These positions testify that the old presumption of an absolute rationality has been gradually substituted by a more flexible concept - Herbert Simon's innovative notion of "bounded rationality"⁵ - which accounts for the scarce capacity of individual economic agents to collect and elaborate all relevant market information, and pays a greater attention to the decision-making process. The rapid growth of new theories of the firm which assume only a procedural rationality (no substantive rationality) testifies this change of methodological attitude.

The traditional assumption of rational economic behaviour thus now appears to have been reduced to one of simple conformity to a situational logic. This relaxation of the basic premises of the neoclassical model has undoubtedly increased its degree of realism and its predictive power. Recent neoclassical models incorporate costly and incomplete information, stocastic expectations, learning by doing and endogenous innovations. A revision of the most critical attitudes towards neoclassical theory therefore seems both possible and desirable.

1.6. A further point which may be worth discussing is the relationship between methodological pluralism and the logic of theory appraisal. The prevalent opinion on this problem seems to be that methodological pluralism in economics should not imply absence of internal control. There is wide concordance on the fact that some conventional rule has to be respected in appraising an economic theory, or a whole research programme in economics. The only question is: which rule? An absolutely general one, covering all sciences, as advocated by positivists? Or a more specific one, valid only for social sciences, or for economic science alone? The answer to these questions is a long and instructive story, which goes back to nearly two centuries ago, when the Ricardian hypothetico-deductive approach established itself as a standard method of economic reasoning.

I have no intention to recall that story. But let me note some specific points of interest. Inside the classical school, which regarded political economy as a deductive scientific subject, there was no prejudicial denial of the utility of inductive inference. John Stuart Mill maintained that inductive methods should be employed in economics - not for discovering truth, but for verifying truth. Verificationism was therefore, in this sense, the logical method the late classical economists, such as Cairnes and Mill, supported to establish the truth or falseness of a specific hypothesis.

Unfortunately, verificationism - though an essential component in the epistemological statute of every subject matter which refers directly to the real world - is not applicable to the type of non-

⁵ H.A. Simon, "Theories of Bounded Rationality", in C.B. MacGuire and C. Radner (eds.), *Decision and Organization*, North-Holland, Amsterdam, 1972, pp. 161-76.

analytic propositions frequently used by economists. Two reasons may be adduced in support of this statement. First, no single criterion meets the need of verifying apriori synthetic sentences founded on intuitive premises, such as those which affirm the transitivity of individual preferences, or the impossibility of interpersonal comparisons of utility. Second, verification in economics is bound in any case to be highly problematic, both for the non-experimental nature of this science and because economic laws are empirical correlations devoid of any character of necessity and subject to various counteracting influences.

At the end of the Nineteenth century, the spread of determinist positivism brought a revival of verificationism, in a more rigid form. The positivistic conception of objective knowledge, modeled on natural sciences, was in open contradiction with the historical relativity and simple plausibility of economic reasoning. Political economy was not the "social physics" positivists had in mind. There could be however some room for the temperate kind of economic positivism which was repropounded in England at the time of the stormy Austro-German *Methodenstreit*. John Neville Keynes, by inclination a methodological pluralist, did much to reaffirm the classical vision of the problem and to reconcile it with the positions of the American institutionalists and of the German historical school, who were resolutely in favour of an inductive method.

In the history of Italian economic thought, the highest point achieved by positivism may be identified with Pareto's attempt to free economics from psychological influences. Pareto did not deny the logical priority of psychics on economics, but tried to transform the latter into a pure theory of choice: the study of those "logical actions" which consist in selecting the means most suited for the pursuit of given ends. For this purpose, he abandoned the assumption of hedonistic behaviour and substituted the traditional individual utility function, which implied cardinal measurability, with a more palatable "ophelimity" function, involving only an ordinal preference scale⁶.

On the whole, the influence exerted by utilitarianism upon economic culture was somewhat less effective in Italy than elsewhere. Though utilitarianism had been readily assimilated, through the works of Pantaleoni, De Viti De Marco and other exponents of economic hedonism, it remained essentially an imported cultural phenomenon, filtered from the writings of foreign authors, such as Gossen, Jevons

⁶ Thirty years later, both the classical mixture of abstraction, deductivism and verificationism and Pareto's suggestion of a pure logic of rational choice and for a scientific knowledge free from value judgements and neutral with respect to single aims of economic policy were successfully repropounded in England by Lionel Robbins. But this time in a different apriorist mood, derived from the neo-Austrian school. See L. Robbins, *An essay on the Nature and Significance of Economic Science*, Macmillan, London, 2nd ed., 1935.

and Walras. The critical attitude taken with respect to the hedonimetric approach to the theory of value by Antonelli, Pareto, Barone, and by the philosopher Antonio Labriola, started an intensive season of scientific debates. Altogether, the discussion did not show violent tones and did not result in a rejection of the utility theory of labour. But the anti-hedonistic tendency was largely followed, especially by general equilibrium theorists of the Paretian school. A number of Italian economists gave evidence that they regarded the "hedonistic postulate" as an undemonstrable premise, an unverifiable behavioural hypothesis, or an ethical proposition unsuited for distinguishing value from pleasure⁷.

1.7. The real turning point in the methodology of scientific research was Karl Popper's "critical epistemology". Without questioning the possibility of objective knowledge and the need of a demarcation criterion, it substituted the previous search for truth by a search for facts and observations capable of disproving a conjecture, thus providing a brilliant, reverse solution to the old problem of induction, raised by David Hume. Truth could not be unambiguously ascertained, but falsity could be rationally criticized and successfully refuted. It was a signal that the retreat from positivism had begun. Freed from the boundary of the relevance of assumptions, the growth of scientific knowledge could at last proceed by conjectures and refutations and become a great critical process, a continuous adventure of the reason.

Some doubts were however raised on the possibility of applying the falsificationist methodology to a social science such as economics, where the *ceteris paribus* condition which makes theories unfalsifiable does not represent, as elsewhere, a simple immunizing strategem, purposely adopted to protect a theory from refutation. For this reason, Popper himself considered economics a tautological subject, devoid of any empirical significance. Its practical function could not go beyond measurement of the deviations of real economic behaviour from the pure rationality model on which it was founded.

Popper was not a methodological pluralist. He severely censured as historicism any methodological position in the field of social sciences which was not in line with his conception. Yet his fallibilist perspective was less unilateral than the neopositivist pretension to assign a sort of monopoly in the search for truth to purely empirical methods. Moreover, at a certain stage of his intellectual itinerary, Popper revised his initial position and accepted the idea that no economic theory might be conclusively discredited by a failure of the empirical testing of its predictions,

⁷ On this point, see D. Cavalieri, "Contributi italiani alla critica della concezione edonistica dell'economia", in R. Faucci (ed.), *Gli Italiani e Bentham: dalla "felicità pubblica" all'economia del benessere*, Angeli, Milano, 1982, vol. II, pp. 17-27.

so long as another theory with a better predictive power was not provided. This view represented a "modified" solution to the problem of theory control, which accounted for the difficulty of making crucial experiments in the presence of prescribed side conditions.

The above qualification came as a corollary of Popper's response to the so-called "Duhem-Quine" irrefutability objection, according to which the failure of a theory to meet an empirical test could be simply due to the absence in the real world of some of the required side conditions⁸. Quine had argued, against what he called the "dogma of reductionism", that no controlled experiment was really "crucial", in the Baconian sense of providing conclusive empirical evidence in support of a theory and against some other one, since a theory always states something more than one can observe or predict.

This "modified solution" to the problem of theory appraisal was a significant change of attitude in the direction of methodological pluralism, as it involved a comparison of alternative theories, and not only a comparison of a theory's predictions with the available empirical evidence. However, it still left open the Quinian question of what to do in the presence of conditional predictions made by a theory which assumed untestable side conditions, so that it could neither be confirmed nor disconfirmed.

1.8. For a long period, in the past, methodological pluralism was not very popular among Italian economists. The autocratic head of the liberal school in Italy, Francesco Ferrara (1810-1900), and three leading figures of economic purism, Vilfredo Pareto (1848-1923), Maffeo Pantaleoni (1857-1924) and Luigi Einaudi (1874-1961), were against methodological pluralism. Ferrara, who held a narrow conception of economics as a purely theoretical science and followed an "absolutist" approach to the history of economic ideas, took an active part in the battle of method, where he opposed the historical school and the spread of "economic Germanism". Pareto, who began his scientific activity as a positivist, thought that there was but one legitimate method of doing research in economics, which he regarded as a natural science. Pantaleoni was only slightly more tolerant: he maintained that a single school of thought - the school of those who know the subject - is fully justified in economics and that any analytical progress in this science is bound to follow a linear trend. Einaudi favoured the idea of the existence of an economic "dogma" and thought that the history of economics should only deal with the internal developments of this tenet. (For a more detailed account, see chapter two).

It took quite some time for the small group of methodologically pluralist social scientists in Italy to overcome this unfavourable situation and to re-establish legitimacy of attention to the history

⁸ See W.V.O. Quine, *From a Logical Point of View*, Harvard Univ. Press, Cambridge, Mass., 1953, 2nd ed. 1963.

of relationships between economic science and the great philosophical and political theoretic systems.

In this variegated Italian context, fairly open to the international flow of new ideas, Popper's critical epistemology came as a truly innovative event. It was welcomed in the late Fifties by Ferdinando Di Fenizio, who edited the translation of some of Popper's methodological works, and by a small number of Keynesian economists. The critics of the dominant neoclassical paradigm were ready to recognize in Popper an alternative to scientism, a methodology strictly linked with important currents of the marginalist doctrine. But some economists educated in the Paretian tradition also seemed to appreciate the attempt to remove from science any residue of psychologism, by distinguishing the logic of scientific research from the psychology of knowledge.

1.9. Elsewhere, the success of the new critical epistemology had encouraged economists to adopt different blends of logical positivism and falsificationism, running from the methodological position held in the late Thirties by Terence Hutchison, according to which both theory assumptions and theory predictions had to be tested by the falsificationist criterion (with serious consequences for the validity of the neoclassical theory), to more conventionalist positions held in the Fifties by other economists. The main reference is to Milton Friedman and Fritz Machlup, who maintained that the premises of economic reasoning were not required to be realistic, but only to be capable of generating testable predictions, so that the credibility of the neoclassical theory could be re-affirmed, in spite of the scarce realism of its premises.

The underling trend was to release economic theories from metaphysical notions and to regard them as simple instruments of inquiry, endowed with an explanatory power measured by the success or failure of their predictions. Theories could no longer be considered true or false, but simply adequate or inadequate to a particular task. A typical example of this instrumentalist conception was the operationalist-descriptivist approach suggested by Paul Samuelson, who denied any possibility of explanation in economics, but took description and prediction as admissible.

On the whole, these empiricist methodologies have never been very popular among Italian economists, who show a propensity to internalize theoretical research, to reject factual myths and to welcome any attack on empiricism which casts doubts on the existence of an "objective knowledge".

The strongest of these attacks - after that of Willard Quine, already mentioned - came from Russell Hanson's contention that no confrontation with empirical data could be accorded a decisive role in verifying or falsifying single theories, because factual observations are themselves "theory-laden", in the sense that they

cannot be ascertained independently of a prior theoretical frame⁹. This amounted to a denial of the existence of a pretheoretic observational language, an opinion shared by several philosophers of science. In a similar epistemological perspective, Stephen Toulmin pointed out the importance of the phenomenon of the meaning variance, which prevents different paradigms from having common theoretical terms¹⁰.

It was probably this unsatisfactory state of affairs which induced Imre Lakatos to distinguish between "naive falsificationism" - the unduly destructive notion that a theory must be discarded if it has been shown to be inconsistent with even a single piece of empirical evidence - and a "sophisticated falsificationism", a more acceptable notion that requires a number of negative tests to reject a theory, or a research programme¹¹.

An even stronger recently recorded reaction to prescriptive methodologies is the irrationalistic view which maintains that economic science is simply what economists do and that economic truth consists in what economists believe. This is a sterile position. The idea of justifying economic methodology by appealing simply to the current scientific practice of economists amounts to denying the very existence of a methodological problem.

1.10. Modern epistemology has thus succeeded in dismissing any general principle proposed for the construction of a scientific methodology in economics. Feyerabend's methodological anarchism - summarized in his famous dictum: "anything goes"¹² - and Lakatos' theoretical pragmatism, both of which reject single prescriptive rules and the very necessity of theory choice, are highly rated by a number of economic methodologists. The case for methodological pluralism therefore seems to be reinforced. But it has to be stressed that methodological pluralism does not involve aprioristic rejection of any methodological rule (*i.e.*, methodological nihilism, or anti-methodology). It has nothing to do with the idea that methodology has come to an end, or that it ultimately reduces to an astute use of rhetorical devices, a style of reasoning typical of a non-demonstrative science. Simple abolition of any methodological rule

⁹ See N.R. Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundation of Science*, Cambridge Univ. Press, Cambridge, 1958.

¹⁰ S. Toulmin, *The Philosophy of Science*, Hutchinson, London, 1953.

¹¹ I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in I. Lakatos e A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge Univ. Press, Cambridge, 1970, pp. 91-116.

¹² P.K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge*, NLB, London, 1975, p. 23 and 302.

would leave an absolute vacuum, on which nothing could be edificated. It would mark the passage from Popper's critical epistemology to a wholly unconstructive critique of epistemology as such.

Methodological pluralism cannot mean that any method of research is equally acceptable, as in art, or that all comparative merits and demerits between methods ultimately cancel out. It involves consciousness that the standards of evaluation of scientific research are bound to change over time and that truth should therefore be intended as a relative concept, similar to correspondence, congruence or coherence (three fundamental concepts in philosophic realism, pragmatism and in Nicholas Rescher's logic of plausible reasoning¹³).

A distinctive feature of pluralism is its willingness to draw materials for methodological reflexion from the history of science, which testifies to the presence at any moment of a plurality of alternative paradigms and scientific research programs. The philosophy and historiography of economics are two distinct subjects with a common object of inquiry, which they study from different points of view. A large part of the philosophy of economic science has an essentially prescriptive intent, whereas the history of economic thought pursues reconstructive and critical aims. But I can see no reason for an opposition between the two approaches. Every interpretation of history rests on a definite methodological conception, as summarized by Imre Lakatos' famous Kantian paraphrase according to which philosophy of science without history of science is empty and history of science without philosophy of science is blind¹⁴.

History of science supplies philosophy of science with historical research materials, so that the various contrasting conceptions on the nature and the evolution of a positive science can be appropriately tested. Philosophy of science, besides shedding intellectual light on the history of science, provides the latter with a whole spectrum of candidate criteria for theory appraisal.

Without entering a full discussion of the *vexata quaestio* of the growth of knowledge, which still places believers in a Popperian "continuist" model in opposition to followers of a Kuhnian "discontinuist" conception, let me point out that if one puts the real relevant question - namely, what determines the appearance, growth and crisis of a theoretical system in economics - the existence of a strict connection between the methodology and the historiography of economics becomes absolutely evident. There are, of course theories which collapse when a certain type of institutional

¹³ See N. Rescher, *Plausible Reasoning*, Van Gorcum, Assen, 1976.

¹⁴ I. Lakatos, *The Methodology of Scientific Research Programmes. Philosophical Papers*, J. Worral and J. Currie (eds.), Cambridge Univ. Press, Cambridge, 1978, vol I, p. 102.

organization of society is substituted by a different one. But other theories enter a similar crisis for philosophical rather than factual reasons. They fail when their logical inconsistency is definitely ascertained, as in the case of the classical and Marxian labour theories of value, or the neoclassical theory of income distribution. Still other theories are dismissed when they are shown to be founded on false philosophical premises (see Carey's and Bastiat's doctrines of economic harmonies). Philosophy of science may thus perform an important role in the development of the history of economic thought. And both of them may help economic research, wherever economics is regarded as a non-nomothetic theoretical subject, undistinguishable from its internal history and from its critique.

1.11. At the present time, methodological pluralism is fairly deeply rooted in the critical approach to economic science in Italy. This depends in part on broader intellectual horizons than in the past and on a relatively recent tradition of thought which sets high value on heterodoxal styles of scientific reasoning; in part also on the type of education given in Italian schools and universities, where curricula generally do not privilege purely professional training and encourage personal ability to formulate critical judgements. Average middle-aged Italian economists are not simply experts in the use of analytical techniques, but also social scientists sensitive to the political significance of their studies and interested in methodological and historical-critical questions. They feel personally involved in the solution both of the great social problems of their time and the major theoretical questions of their subject matter.

This kind of representative economist was formed in the Sixties and early Seventies, when the revival of a critical interest in the Ricardian and Marxian theories of value and distribution successfully questioned the dominance of the received monistic methodological view, according to which all economic phenomena obey a single logic, unaffected by changing institutional circumstances and by "antropomorfic prejudices".

But the present situation is perhaps less satisfactory as regards the methodological and thematic preferences of the new generations of economic scholars. Younger economists appear to be more sensitive to further refinement of analytical techniques and purely formal exercises in model building than to recovery of the social responsibility of political economy. There is therefore the risk of a new epistemological fracture which could draw the center of gravity of the economists' theoretical interests nearer to those exclusively internal problems which have in recent years been an increasing part of current academic practice.

1.12. As a specific, though not exclusive, feature which characterized the evolution of the Italian economic science in the

last thirty or forty years, it may be mentioned that a number of Italian economists who were not prepared to accept either the neoclassical or the Marxian concept of value stuck to the Sraffian theory of prices, in which they saw a more promising approach to some of the most controversial problems in the theory of capital and income distribution and a key for overcoming the mainstream tendency to keep the theoretical and political debates separate.

Two different interpretations of the impact of the Sraffian approach on the Marxian theoretical system were offered. According to the prevailing one, the so-called "neo-ricardian" interpretation (shared by Pasinetti and, initially, by Napoleoni), the failure of the Marxian labour theory of value to explain relative prices implied the breakdown of Marx's basic concept of capitalistic exploitation and of the whole of his economic theory. On the contrary, the "neo-marxian" interpretation (held by Garegnani) maintained that the Sraffian correct solution to the problem of price determination should not mark the end of Marx's theoretical system, but should rather be seen as a decisive factor of improvement in explanatory power of Marxian theory. Sraffa's solution was indeed seen by neomarxists as the decisive element which helped to free the Marxian system from the unnecessary burden of a labour theory of value, now regarded only as the instrument employed by Marx to avoid the risk of falling into circular reasoning in determination of the uniform profit rate and of relative prices.

In the late Seventies, with the rapid decline of Sraffian economics - recognized as a devastating critique of both the neoclassical theory of value and distribution and the Marxian labour theory of value, but imputed either to be only half a general equilibrium system, irretrievably deficient on the demand side¹⁵, or to be completely unaware of the real features of capitalism (class conflicts, disequilibria, crises) - the ambitious project of recovering into a great theoretical synthesis what was still valid of the tradition of economic thought running from Ricardo to Marx and Sraffa (with the possible inclusion of part of the Keynesian doctrine) definitely collapsed. The Sraffian "prelude to a critique of economic theory" thenceforth started to be considered for what it probably was: a perceptive introduction to a radical critique of economic thought, taken as a whole, and not the proposal of a new theoretical approach to the old problems of value and distribution, or an analytical description of the working of a capitalist economy.

A less eclectic and more genuinely pluralist methodological climate then followed. It was due to the assumption of the illegitimacy of relying on illusive attempts to arbitrarily decompose and recompose heterogeneous theoretical systems into grand synthetic constructions differing in their premises and in their objectives,

¹⁵ This is Joan Robinson's definition of the Sraffian price system, in "Prelude to a Critique of Economic Theory", *Oxford Economic Papers*, vol. XIII, 1961, p. 9.

for this results in a loss of both their internal links and their general visions¹⁶. This uncompromising methodological position, whose chief exponent is Giorgio Lunghini¹⁷, seems to have marked an important turning point in the "post-Sraffian debate" in Italy, by making evident the illusiveness of getting significant results by a search for grand but innatural syntheses.

1.13. Needless to say, the attention Italian economists pay to the construction of a critical knowledge has some practical disadvantage. In to-day's technocratic societies, where employers look for competent people in the perspective of their full integration in the productive system, wide cultural curiosity beyond the strict sphere of professional interests is not particularly appreciated. It is a risk factor, as from curiosity to critique there is but a small step. It is therefore easy to realize that it may involve a discriminatory behaviour in the demand for economists. In the search for a qualified job, graduates from a business school are usually preferred to "political economists". Cultural myopia and absence of theoretical-critical attitudes seem to be highly appreciated qualities for a rapid career as a business executive, as the most recent history teaches.

In such a difficult climate, it is comforting to see that a number of Italian economists reject the idea of limiting the basic methodological choice to a rigid alternative between adoption of a single "objective" criterion of theory appraisal and the opposite choice in favour of free personal experimenting. They seem to favour a sort of middle-way methodological position, roughly equidistant from the codified rationality of science and the illimited freedom of art. It is maintained that such a solution is particularly suited for overcoming the traditional distinctions between values, facts and theories and for linking knowledge, expectations and actions. The idea of appealing to conventional rule for theory control is thus accepted as a pure matter of practical convenience, with no claim to impose a uniform model of search.

The historiographic implications of this methodological standpoint move in the direction of acknowledging the temporal coexistence of several theoretical paradigms, in discontinuous evolution, whose relative success or failure depends on their respective ability to give significant answers to the major problems of the moment; not on their capacity for "puzzle-solving". Hence the Kuhnian distinction between the "normal", acritic activity of scientific research and the "extraordinary" activity, which consists

¹⁶ For an example of these attempts, see A. Ginzburg and F. Vianello, "Il fascino discreto della teoria economica", *Rinascita*, 1973, n. 31, repr. in *Marxismo ed economia: un dibattito di "Rinascita"*, Marsilio, Padova, 1974, pp. 15-26. See also a comment by B. Ingrao and M. Lippi, *ibidem*, pp. 125-31.

¹⁷ See G. Lunghini, "Il posto di Sraffa", *Alfabeta*, 1980, n. 13.

in submitting the dominant paradigm to critical analysis in periods of scientific revolutions, is in a sense reversed: critical activity is assumed to be the normal situation, not the exception, and is therefore practiced with continuity by a large number of theoretical economists. The extent and persistence of the phenomenon may suggest that the positivist tendency to privilege science over culture did not leave lasting traces in Italy.

Another distinctive feature of the methodological debate in economics which has taken place in Italy in the most recent years has been its tendency to be closely linked to the discussion of theoretical and historical-critical problems, rather than to be pursued as something interesting in its own right. Contrary to developments in other countries, where the methodological debate was to a large extent an epistemologists' affair disjointed from the problems usually discussed in economic journals, the tendency in Italy has been towards stricter association of the methodological discussion with substantial economic problems and with appraising specific economic theories or research programmes. This explains why the debate has taken place almost exclusively on journals of economic theory and history of economic thought.