

1 Introduction: A Non-linear Discourse

The idea of writing this book was dismissed as crazy by many of my friends. The book is here, now, but they may still be right, given the breadth and complexity of our field of enquiry: contemporary economic research, from the Second World War to the present. Consider the mass of economic journals (some thousands) and books (ditto) published yearly; consider the fact that economists active in the period under consideration by far outnumber the authors of economic writings of all the previous periods. Thus, although there are on average a thousand pages read behind every single page of this book, my ignorance remains pervasive and the account of each area of research will be scanty and simplified.

However, confronted with the fragmentation characterizing economic research today, in order to evaluate the present state of our science it may be useful to reconstruct its lines of development, inquiring into their interrelations and the underlying philosophies, or worldviews. My hope is that this will help explain why contrasting views abound in theoretical research. As Joseph Schumpeter (1954, p. 4) maintains, studying the (in our case, recent) history of economic thought may be useful to ‘prevent a sense of lacking direction and meaning from spreading’.

The task is clearly a very difficult one, and we cannot expect it to lead to a univocal solution. ‘There are more things in heaven and earth, Horatio, than in your philosophy’: Hamlet’s lesson holds in many research fields, including the economists’ search to understand the functioning of human societies. It also holds for the search to reconstruct the history of any field of human culture, including the history of economic thought. Indeed, in this case it holds perhaps even more than for other fields, owing both to the intersections between worldviews, analytical developments and political passions, and to the continuous evolution in the economic and social structure of human societies.

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As we shall see, the very definition of economics may take on different connotations;¹ above all, we are confronted with a multiplicity of worldviews logically preceding the multiplicity of theories. The different worldviews affect both the selection of the specific problems to be dealt with and the framing of the analysis. It is one thing to study the evolution of technology, but quite another to consider the motivations behind human actions. It is one thing to conceive economic theory as the way in which humans tackle the problem of scarcity, but again quite another to look at the set of economic relations from the point of view of the division of labour within a circular flow of production, distribution and consumption. In reconstructing the history of economic thought, different approaches (classical, marginalist, Keynesian and so on) intersect with different research fields (macroeconomics, the firm, public finance and so on).²

Within each approach, basic concepts and, in many instances, the theoretical schemes utilized for the analysis of different issues show significant affinities. Moreover, there are dialectical relationships of self-definition through opposition, between external and internal criticisms, theories and concept definitions shifting in response to difficulties emerging in analytical debate. Also, owing to the increasing specialization of researchers, over the past seventy years research in the field of economics has been characterized by far more fragmentation than hitherto, and increasing over time; this implies ever more frequently losing contact with the final goal of economics, namely interpretation of the economic reality confronting us and its evolution, which requires a multiplicity of competences on the part of the individual researcher.

All this means that recent developments in economic research cannot be illustrated by following the simple linear course of their development over time, nor by a catalogue of independent themes and theories. Often it will inevitably mean going back in time, as well as shifting from one approach or one field of research to another and back again. However,

¹ Economics, the term currently used today for our field of research, was introduced (by Marshall 1890) to mark a discontinuity with classical ‘political economy’, pointing to a stricter affinity with the ‘hard sciences’ such as physics or mathematics. However, as Alice in the Wonderland says, the meaning of the words is whatever we choose for them; thus, I shall use economics in a broad sense, possibly more akin to that of classical economists than to Samuelson’s 1948 notion.

² Both ‘research approach’ and ‘field/area of research’ are aggregate notions, with boundaries that are difficult if not impossible to define in a clear-cut way. Each economist may present more or less heterodox distinctive elements within his or her field of research; as for the research fields, it should suffice to recall the difficulty to define systems of classification for journal articles, like those appearing in the *Journal of Economic Literature*: commonly, more than one code is reported for each article; classification sets are never considered perfect, and are occasionally revised.

precisely as a reaction to the dangerous trend towards a fragmentation of economic research, we need to search for connections and lines of development and find justifications for them.

Obviously, the reconstruction proposed in this volume, though meditated at length and founded on a vast mass of material (the bibliography falls far short of accounting for all the books and articles consulted over more than half a century of study of our subject), may be criticized on many counts. In this Introduction I shall try to justify some of my choices, although there are bound to be disagreements, in particular on the weight attributed to authors, schools, approaches and research fields.³

As far as this latter aspect is concerned, let me point out immediately that in a book like this, focused on a survey of the troops and illustration/interpretation of the different research approaches (hence, focused on concepts and ideas more than on models or individual economists), it may be useful to allot more space than usual to developments outside the mainstream, even independently of the partisan propensity of the author. In the long run, as a rule the more innovative heterodox thinkers may receive more attention while the orthodox thinkers dominating in a given historical period may recede to a secondary position.

In any case, I can well imagine that each reader will find the treatment of their specific research fields far too brief and simplified. Some may even view some topics bordering on other social sciences as external to the field of economics. The former criticism might indeed hold for the whole of the book, despite the need to select and simplify the material to be considered; the reader should also keep in mind that my objective is a reconstruction, albeit a concise one, and not representation on a reduced scale, as in the case of geographical maps. As far as the latter criticism is concerned, however, I hereby notify my dissent. Refusal to consider so many aspects of social life as an integral part of the issues tackled by economists obviously may simplify our research work, but it also implies a loss of depth that may turn out to be very dangerous, especially when we expect the results of our research to influence economic policy.

Quite often, those working within a given approach and tackling specific issues decide to ignore the multifaceted nature of economic research, or fail to perceive it. Whatever (often very little) is gained in

³ Some studies (e.g. Kosnik 2015) offer data on the percentages of articles or pages published in the different research fields in a more or less extensive and more or less representative selection of journals. These data have a certain relevance for an understanding of what economists are doing; however, in a work like ours even considerable deviations between these proportions and the space allotted to the different research areas are – I feel – justified.

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depth is more than offset by a loss in general vision and critical capability. Economic research thus risks getting lost in a meander of blind alleys. The danger is even more serious for those conceiving of research as a guide to action. Here, connections between different aspects of the most complex issue of how to pursue the common weal may be lost sight of. Also, the opposition between different theses may be perceived as a clash of *a priori* tenets, thus barring mutual understanding in the open debate between opinions based on contending approaches, the characteristics of which should be set out and discussed. Each economist engaged in a specific field of research may find it advantageous to consider that field from outside, thereby realizing that certain traditional axioms may actually be questionable.⁴

The historian of economic thought tries to build a bridge between the different approaches, by reconstructing their foundations and illustrating their internal evolution. Obviously, historians of thought are entitled to hold a personal opinion: as economists, they are part of the debate being illustrated to their readers. We are in any case confronted with a constraint: respect for the scientific criteria of the history of thought, which we may summarize as philological respect for text and context. The possibility of dissent remains open, indeed much wider than in the case of mathematical theorems; however, the debate may take place in an open way if the approach is to search for textual or contextual elements in support or refutation of each interpretative thesis. As Kula (1958, p. 234) writes: ‘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.’

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Once we accept as a fact of life that there are different approaches to economic theory,⁵ interpretative work needs to pay attention to an aspect quite often overlooked in current debate. As Schumpeter (and before him Max Weber) observed, theoretical models are grounded on a web of concepts; each concept, though retaining the same name, may differ even radically from one approach to another. The theoretician in his or

⁴ Let us recall what John Stuart Mill wrote (in his essay ‘Auguste Comte and positivism’, 1865, quoted by D’Ippoliti, 2011, p. 106): ‘A man’s mind is as fatally narrowed, and his feelings towards the great ends of humanity as miserably stunned, by giving all his thoughts to the classification of a few insects or the resolution of a few equations, as to sharpening the points or putting on the heads of pins.’ Mill may possibly appear too severe on the need for specialization present in every field of scientific research, though such a need is not denied: what is being criticized is the exclusive focus on a specific theme of research, without ever raising the eyes to observe what surrounds it.

⁵ That is, when accepting a ‘competitive’ view of the history of thought, rejecting the ‘cumulative’ view that assumes continuous progress within the same basic view of the economy: cf. Roncaglia (2005), pp. 2 ff.

her work often pays scant attention to this aspect, assuming as given the approach dominating his or her research field. As a matter of fact, however, construction of such a web of concepts – and the associated choice, often only implicit, of a web of simplifying assumptions – constitute from the logical viewpoint the first, far from simple, step on the path of any research: the ‘stage of conceptualization’. Building formal models and comparison with empirical reality are only the second and third steps: they, too, are quite complex, especially the third, but never so much as the first. In order to compare theories grounded on different approaches, it is crucial to consider first of all the first step, namely the web of concepts, and only subsequently to consider the strictly analytical issues. This is why in these pages greater attention is paid to concepts than to theoretical models; a few analytical elements are briefly illustrated in a couple of appendixes. Often, to make the going easier for readers who have not had a university education in economics, analytical issues are relegated to footnotes or bibliographical references.

We can begin by taking not one but two steps backwards. Part I of the book is devoted to recalling the premises of the recent economic debate. Chapter 2 offers an overview of the main approaches adopted in the history of economic thought: the classical one, with Adam Smith and David Ricardo (and, in some respects, Karl Marx); the marginalist one, with William Stanley Jevons, Carl Menger and Léon Walras.⁶ We shall also recall the distinction between the monodimensional (felicific calculus) and the pluridimensional (passions and interests) notions of the motivations of human action. Chapter 3 considers the main protagonists in the period going from the end of the nineteenth century to the middle of the twentieth century: Knut Wicksell, Thorstein Veblen and Max Weber; John Maynard Keynes and Joseph Schumpeter. The latter two in particular had a notable influence in the period following the end of the Second World War, namely the period on which we focus attention, but died when this period had just begun.

Part II constitutes a bridge between the debates of the inter-war period and those of the subsequent period. It is devoted to two authors: Friedrich Hayek (Chapter 4) and Piero Sraffa (Chapter 5), who published important works both before and after the Second World War, and hence belong to the period we are considering. At the level of the conceptual foundations, these two scholars represent to the full the two main

⁶ Labels such as classical, marginalist, mainstream and heterodox economics obviously constitute simplifications, and cannot be given clear-cut definitions. They are useful as general indications, but leave ample room for internal differences and difficulties of classification.

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competing approaches active in the European culture of the ‘short century’, namely the period running from the beginning of the First World War to the fall of the Soviet Empire:⁷ the conservative view, extolling the invisible hand of the market, and the revolutionary one bringing to the fore the distributive conflict between wages and profits. This explains why more space has been allotted to them rather than to other economists – such as Sir John Hicks, Milton Friedman or Paul A. Samuelson – whose contributions rest on already well-established basic conceptual foundations.

As to the period directly addressed in this book, the choice of the line of exposition constitutes a serious problem, open to different solutions, each with its negative implications. The solution adopted here highlights the cultural–analytical debate, devoting ample room – as noted earlier – to heterodox views (which in fact are such only in the decades considered here, but were not necessarily such in earlier periods and may turn out not to be such in the future). As we shall see, some chapters are devoted to research fields, others to schools or streams of thought, while still others combine these characteristics.

Thus in Part III we consider the development of the dominant research approaches – the so-called mainstream – in the main research areas (micro, macro, applied economics), together with the main neo-liberal streams, quite different from one another but sharing the thesis of the efficacy of the invisible hand of the market at the political-cultural level.⁸

Part IV is devoted to those fields of debate – the axiom of rational behaviour and its limits, finance and crises – that show clear-cut opposition between different research approaches, with the consequence that the analytical results, even when accepted as such, are given conflicting interpretations.

Part V is mainly devoted to heterodox research streams: post-Keynesians (Chapter 12), Marxists, institutional and evolutionary economists (Chapter 13); post-utilitarians and the capabilities theory (Chapter 14).

⁷ The notion of the ‘short century’ was utilized by the British historian Eric Hobsbawm (1917–2012; 1994, p. 3), in opposition to the notion of the ‘long century’, the nineteenth century covering the period from the French Revolution in 1789 to the beginning of the First World War in 1914; it is now widely accepted. As a matter of fact, the fall of the Berlin Wall in 1989 and the fragmentation of the Soviet Union in the immediately subsequent years mark a cultural break, blurring the opposition between socialism and capitalism, market and planned economies. Thus, our book covers two historical periods (pre- and post-1989) significantly different on the political plane if not on the plane of economic theorizing.

⁸ Once again, the term ‘mainstream’ is meant generically, pointing to the (internally quite varied) set of theories that dominated economics teaching in the period under consideration.

Apart from a few broad allusions, the possibility of convergence on the conceptual if not on the analytical level of these streams is left open.

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In the post–Second World War scenario, the barycentre of the economic debate moved from the East to the West of the Atlantic: from Europe, winner or loser but in both cases in ruins, to the triumphant United States.

Precisely as a collateral product of war research, not widely known to the public at large but an important pillar to a newly emerging dominant economic culture, we have the developments in the theory of general economic equilibrium connected to game theory and expected utilities. The polyhedral von Neumann was at the centre of these developments, together with the network of economists and mathematicians grouped in various research centres, such as the Cowles Foundation and the Rand Corporation, and in the most important universities. Game theory and expected utilities imply a reconstruction of the microeconomic foundations of the marginalist approach, and find their place side by side with the old Marshallian tradition still holding out in major centres like Chicago and a number of minor universities.

Power relations at the political and cultural level, beyond the strictly economic sphere, led to Keynes's partial defeat at Bretton Woods when confronted with the US ideas for the reconstruction of the post-war international economic system. The same constellation was among the factors behind the rise of a new cultural hegemony, which absorbed and annihilated the main elements of novelty of Keynesian theory within a 'neoclassical synthesis' with the marginalist approach to value and distribution. The East Coast of the United States, particularly MIT with Paul Samuelson, Franco Modigliani and Robert Solow, contributed the essential elements of this hegemony, both at the level of pure theory and at the – also quite important – level of their incorporation into a well-structured textbook vulgate.

However, at the theoretical level the compromise between the marginalist and the Keynesian approaches is of necessity fragile: with rational expectations theory, the marginalist foundations tend to overcome the Keynesian residuals, also at the level of policy. Thus, also due to the evolution of the international economic situation (crisis of the Bretton Woods system, then the two oil crises of 1973–4 and 1979) and then of the political situation (with Margaret Thatcher's victory in Great Britain and Ronald Reagan's victory in the United States), neo-liberalism in its various versions took the lead

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over progressive liberalism, increasingly concentrated in the US universities of the East Coast.⁹

At the political level, but not on that of theoretical construction (although both extolled the invisible hand of the market), the neo-liberal positions of monetarists and rational expectation economists found an ally in a rejuvenated Austrian school that gave central importance to the problems of knowledge and uncertainty and of the time dimension. The new Austrian school was supported, albeit with some autonomy, by Hayek's research and his political-cultural initiatives, such as the foundation of the Mont Pèlerin Society. We cannot place ordoliberalism within this school, but it is in many respects connected with it; it developed in Germany and remains important there, as well as – albeit in a modified form – in the European Union's economic policy.

Econometrics was born well before the beginning of the Second World War; the foundation of the Econometric Society dates back to 1933. On the borderline between applied economics, statistics and probability theory, it developed rapidly in the post-war period, with the construction of large econometric models utilized for forecasting and policy purposes. Developments in econometrics gave rise to intense research activity and the construction of an a-theoretical economics that claims to deal with specific economic issues on the sole basis of statistical inference. Wassily Leontief's input–output models and linear programming, although in many respects connected to general equilibrium theory, are also considered as analytical tools for applied research. A recent development in applied research, closer to descriptive than to inferential statistics, consists in research aimed at building synthetic indexes for socio-economic phenomena: human development indexes, indexes of well-being, of economic freedom, of democracy. Such indexes allow us to quantify intrinsically qualitative variables, so as to enable use of them to study their impact on economic variables such as the rate of economic growth or per capita income. On the borderline between theoretical and applied economics we then have some research fields of great practical relevance, such as market regulation and auctions, the economics of energy sources (a field in which I did some work in the past) and environmental issues.

Mainstream economic theory has dominated for decades, but not without opposition. A fundamental battlefield lies in the theory of value. Devastating criticisms by marginalist economists of the labour

⁹ In Italian I was able to differentiate between 'liberale', a political category, and 'liberista', an economic category focused on free trade and free entrepreneurship. The term 'neo-liberal' is utilized here, following current usage, to designate currents of thought better depicted as 'liberisti'.

theory of value upheld by the classical economists and Marx were already looming large by the end of the nineteenth century (and the Marxian approach, though surviving, has been deeply affected by them), to be followed in the second half of the twentieth century by equally devastating criticisms of the traditional marginalist theory of value and distribution. Consensus on the analytical results of these criticisms is not accompanied by corresponding consensus on their implications, due also to some misunderstandings which we shall try to clarify. Concerning these aspects there are in fact a variety of views within both the marginalist–neoclassical and the classical–post-Keynesian fields; moreover, the separation prevailing between research fields fosters widespread ignorance of these analytical findings and especially of their wide-ranging implications.

Gradually, from within the very new mainstream microeconomics based on expected utilities came to light the anomalies, mostly reabsorbed within the dominant approach but accompanied by the collateral development of alternative research lines. Such is the case of behavioural economics, which has as its starting point analysis of the cases (viewed as paradoxes, but as a matter of fact quite general) where the agent's behaviour does not follow the paradigm of rationality, identified with the maximization of expected utility. From here to the development of notions such as bounded rationality (Simon) is a short step. The emergence of behavioural economics was accompanied by the development of experimental and cognitive economics, with bioeconomics and other streams of research such as Daniel Kahneman and Amos Tversky's 'prospect theory', commonly embraced within, but not always fully compatible with, the mainstream.

Incredible as this might seem, not even the recent world financial crisis gave rise to a new interest in the foundational themes of the theory of value, also concerning the ability of market mechanisms to ensure optimal equilibrium conditions. Within the monetary field, the renewed debate was livelier, more or less parallel to debate on the theory of value: the mainstream thesis of efficient financial markets (reinforced by consideration of rational expectations) was opposed to Hyman Minsky's thesis of the intrinsic instability of market economies and the unavoidable return of crises of ever greater dimensions.

This latter idea fell within the stream of post-Keynesian macroeconomics, a wide field rich in internal differences which constitutes one of the pillars of a possible approach alternative to the mainstream one. The centre of these analyses was in Cambridge, UK, where Richard Kahn and Joan Robinson, Nicholas Kaldor and others were at work; here we find in transit such an anomalous character as Michał Kalecki, while an

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Anglo-Italian school developed around Sraffa. The US post-Keynesians as well (Jan Kregel, Hyman P. Minsky, Sidney Weintraub), albeit with their specific ideas and their internal differentiations, had a reference point in Cambridge.

Another field concerns the study of institutions, and the focus on change: a field where Marxism had been active since its origins. Authors like Michael Polanyi and John Kenneth Galbraith constitute a link to more recent research. More directly connected to the marginalist approach, on the other hand, we have neo-institutionalism. Confronting it, partly innovating and partly connected to the Marshallian tradition, we have an evolutionary-institutional stream that presents itself as heterodox as compared to the dominant marginalist approach. On the borderline between growth theory and the analysis of economic-social institutions lies an extensive research area on development economics. Already briefly dealt with by Hayek, competition between different institutions came to the fore with increasing economic globalization.

Finally, an important current of researches concerns different aspects of ethics in the economic field, with the developments of utilitarianism and the ethics of consequences. The ethics debate is relevant to, among other things, the issue of income distribution, long at the centre of economic research, and welfare economics. On the borderline between economics and philosophy, Amartya Sen has provided important contributions, such as the notion of capabilities. We are confronted here with the non-neutrality of the economists' work, connected to the weighty and complex theme, all too often overlooked, of power in the broadest sense of the word: economic, political, social and cultural.

Paraphrasing Gramsci, we might conclude that within today's economic culture the US mainstream approach dominates but without necessarily retaining hegemony. Knowing how varied and complex the economic debate of the last few decades in the economic field has been may help us understand (and question) the more or less solid foundations of the different theoretical approaches, but also of economic policy choices, and hence in improving our ability to tackle the difficult situation confronting us, with the devastating crises and prolonged periods of stagnation it entails.