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General Equilibrium Theory and the Rationality of Economics*

Abstract: Most philosophers of economics are hostile towards neoclassical economics in general and general equilibrium theory in the vein of Arrow and Debreu in particular. Especially the latter's dismissal is justified by pointing out its lack of direct relevance for an understanding of real economies. Many recommend a more pragmatic approach along the lines of Keynes instead. The criterion of scientific legitimacy underlying this approach derives from a philosophy of science developed along the lines of Popper and Lakatos. They, however, neglect the importance of conceptual problems and of the choice of adequate 'language-systems' in science. Since these conceptual and 'linguistic' aspects may be able to explain and to justify the rationale of the Arrow-Debreu approach, I recommend the more balanced philosophies of Carnap and Laudan, in which conceptual as well as empirical problems are allowed for, as a framework for methodological appraisal. I explain why such a balanced view is obstructed for most philosophers of economics and advocate a moderate pluralism leaving room for different theories, methodologies and language-systems, depending on the scientific aims that are pursued.

0. What's Wrong with Economics?

Let me present you with a puzzle. Economics has often been regarded as being the best-developed discipline within the social sciences. It has been promoted as being the premier or even universal social science due to its presumed applicability to virtually all areas of human life (Hirshleifer 1985; Becker 1993; Lazear 2000; Frank 2008). At the same time, however, it has been regarded as warranting the most fundamental criticism and, particularly from the 1970's onwards, as being in a severe state of crisis due to the scientifically unjustified dominance of the neoclassical mainstream (cf., e.g., Robinson 1972; Phelps Brown 1972; Bell/Kristol 1981; Keen 2001). This belief in the critical status of economics has given rise to a whole industry of sceptical or even hostile literature, the only purpose of which is to point out, again and again, that something is wrong with

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economics and what exactly it is that is wrong. The phrase itself became kind of a slogan when Benjamin Ward published a widely received book under this heading (cf. Ward 1972), and it has been carried further through the years up to relatively recent collection of articles titled, in dead earnest, *A Guide to What's Wrong with Economics* (Fulbrook 2004).

Thus, the question of how the merits of economics as a science are to be assessed appears to be unsettled. Some critics like, for instance, Tony Lawson have even gone so far as to claim that “the continued failure of economics over the last 50 years or so is such that the existence of a social science of economics cannot be accepted as a premise.” (Lawson 1997a, 26) Since Lawson is the leading figure of the school of ‘critical realism’ currently influential among philosophers of economics, his judgment is more than just an isolated opinion that need not be taken seriously. Nevertheless, his bold verdict should strike us as surprising, given that we are talking about a period in which economics was already firmly established as an academic discipline and experienced several ‘scientific revolutions’, of which only the following shall be mentioned:

First of all, the ‘macroeconomic revolution’, starting with John M. Keynes’ *General Theory of Employment, Interest, and Money* in 1936, giving rise to macroeconomic and econometric model building and exerting a formerly unknown impact on economic policy making; second, more or less contemporary to the first, the ‘formalist revolution’, consisting in the axiomatic reformulation of Walrasian general equilibrium theory by Kenneth J. Arrow, Gerard Debreu, Lionel W. McKenzie, Frank H. Hahn and many others, and of the theory of individual decision-making under risk and uncertainty by John von Neumann and Oskar Morgenstern, Leonard J. Savage and again many others; third, as a reaction to the temporary dominance of Keynesian macroeconomics, the ‘monetarist counterrevolution’ in macroeconomics led by Milton Friedman and others, which rehabilitated certain tenets of pre-Keynesian economics concerning monetary policy, but was later superseded by, fourth, the ‘rational expectations revolution’, initiated by a paper by John F. Muth and mainly developed by Robert E. Lucas, Jr. and Thomas J. Sargent as an attempt to integrate what they regarded as being the most convincing components of both macroeconomic schools into a more coherent scheme by use of the general equilibrium framework and a concept of expectations consistent with it. Let me also mention, penultimately, the ‘institutionalist revolution’ deriving from Ronald Coase’s long neglected insights into the importance of transaction costs for the working of real economies and the existence of institutions; and, last but not least, the ‘informational revolution’, calling attention to the important consequences of incomplete and asymmetric distribution of information mainly associated with George A. Akerlof and Joseph E. Stiglitz. Many of these economists were awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel*, commonly called the ‘Nobel prize’ for economics, which does not of course prove their scientific merits but is at least evidence for the high esteem in which their peers held their work.

To be sure, not all of these so-called revolutions have gained universal assent among the profession, and some of them have even encountered trenchant resistance especially from the side of so-called heterodox economists. However,

one need not take sides with regard to the details of these controversial issues in order to conclude that a denial of the very existence of an economic science would be going much too far. When we compare the state of economics at the beginning of the past century with its state at the end of it, it is difficult to deny that considerable progress has been made regarding our understanding of the economy and our ability of managing it, although certainly not all of the high hopes that had been nourished, especially during the ‘Keynesian revolution’, have been fulfilled.

This view is confirmed by the conclusions of several prominent reviewers at the turn of the last century (cf. Baumol 2000; Blanchard 2000; Heckman 2000; Lazear 2000; Stiglitz 2000). None of them has denied that there is still much work to be done, but this is trivially true of every science since there is always more to be learned. Nevertheless, their reviews have sketched, by and large, a rather positive picture of the way in which the discipline developed during the past century. Such a positive view, however, is absent from most of the writings by philosophers of economics. Even if they do not question the very existence of an economic science as such, they do insistently question whether the development of the discipline after World War II has been to its advantage.

In short, there is a striking tension between the way in which most economists assess their discipline and the critical, even hostile view that is held by most philosophers of economics. To be sure, there is also a considerable number of economists who are far from being content with the present state of their discipline. The so-called heterodox economists already mentioned above identify themselves mainly by their opposition to what they regard as being a ‘neoclassical’ orthodoxy, i.e. the mainstream. Now, it should, of course, certainly not surprise us that heterodox economists are opposed to the mainstream of their discipline since this is the very definition of heterodoxy. It should, however, give us pause that most philosophers of economics, who are expected to hold a better-balanced view, seem to share this judgment. So this is my puzzle: why are most philosophers of economics so critical, to the point of hostility, about most of economics?

The most straightforward answer is of course: because economics deserves it. And it is not very difficult to find evidence confirming this belief. It can hardly be denied that economics has failed on numerous occasions when its expertise was needed most urgently. Have economists not been surprised, time and again, by new trends and events like the emergence of stagflation in the early 1970s, the breakdown of the socialist economies in the 1980s, or the current financial crisis? Indeed, the inability of economists to predict even the next two quarters reliably is notorious. Economists are at their best when they are permitted to hedge their predictions using *ceteris paribus* clauses. But what the public calls for are reliable predictions as such, not predictions amounting to the assertion that something will definitely happen unless some unforeseen event prevents it from actually doing so. While the rise of econometrics accompanying the ‘Keynesian revolution’ seemed to promise precisely such reliable predictions and, derived from them, scientifically controllable interventions into the economy,

later developments have sometimes dramatically belied this optimistic belief. Therefore, some disappointment should be quite understandable.

That said, we should nevertheless resist the temptation to judge the success of economics solely by means of an absolute standard. It is always easy to dismiss a certain endeavour simply by setting the standards too high. We should instead be more interested in the rationality of the way in which this endeavour was pursued, which means that we should compare its result with those that would have been possible under realistic conditions. It is then far from obvious that economics could have done better than it actually has. Such a claim would in fact be difficult to confirm. But the opposite is also true. Proving or disproving assumptions of rationality is never straightforward since, to a large extent, rationality seems to lie in the eye of the beholder. The rationality of a certain action depends on the logic of the situation, and this logic is not something that can simply be deduced from the facts. It is something which must be used to organize and interpret them in the first place.

It is for this reason that I will simply assume, as a kind of working hypothesis, a basic rationality of economics, and I will try to understand what this rationality has actually consisted in and why so many philosophers of economics have failed to acknowledge it. In the end, it might well be more appropriate to ask what is wrong with the philosophy of economics, given that it has failed for such a long time to provide a balanced account of the achievements and shortcomings of economic science.

1. The Critique of General Equilibrium Theory

The tension between the self-appraisal of many economists and the appraisal of philosophers of economics is most striking when we turn to neoclassical general equilibrium theory that has often been regarded as defining the very core of modern economic theorizing. It has already been above mentioned under the heading ‘formalist revolution’. This theory—or paradigm (Kuhn 2004) or scientific research programme (Lakatos 1970) or research tradition (Laudan 1977), name it as you like—originated with the foundational writings of Léon Walras (1954) and others published at the beginning of the last century (cf. Walker 2003) and found at least a temporary summit in the celebrated works of the abovementioned theorists Arrow, Debreu, McKenzie, Hahn and others after World War II (cf. Debreu 1959; Arrow/Hahn 1971; Weintraub 1983).

The distinguishing feature of this theory is its ambition to give a mathematically precise description of the whole economy, building on the concepts of individual utility maximizing choice behaviour and its mutually consistent interaction resulting in general market equilibrium. While many regard the development of this theory, and particularly its axiomatic treatment in Debreu’s paradigmatic monograph, as one of the major achievements of 20th century economics, there are others who have dismissed the whole endeavour as having been, at best, a considerable waste of time and energy or, at worst, the primary cause of the aforementioned detrimental development of economic science as a whole. According to the critics, there has actually been not progression but regression,

implying that economics is now in a less healthy state than it was in the pre-war age of theoretical and methodological pluralism (cf., e.g., Morgan/Rutherford 1998).

Note, however, that one might agree with the observation that pluralism in economics has decreased since World War II without necessarily considering this to be regrettable. This is where mainstream economists and philosophers of economics most definitively disagree. Many of those who are critical about the present state of economics consider the rise of the Arrow-Debreu model and its methodology as the main cause of a serious aberration from the course of legitimate and worthwhile science. A representative proponent of this view is Mark Blaug, a historiographer of economics (Blaug 1996) who also wrote the first textbook on the philosophy of economics, mainly based on Popper's philosophy of science and its idea of falsifiability as the touchstone of scientific legitimacy (Blaug 1992). Equipped with Popper's philosophical conception, Blaug has repeatedly criticized general equilibrium theory and its axiomatic reformulation as being devoid of empirical content and thus of any scientific merit. To give you a taster of this debate, let me quote rather extensively:

“If we can date the onset of the illness at all, it is the publication in 1954 of a famous paper by Nobel laureates Kenneth Arrow and Gerard Debreu; it is this paper that marks the beginning of what has since become a cancerous growth in the very center of microeconomics. The Arrow-Debreu paper provided a rigorous proof of the existence of multimarket equilibrium in a decentralized economy, a notion which Léon Walras had entertained eighty years earlier but which he had failed to establish convincingly. This proof was rigorous by mathematical standards but it required some assumptions which clearly violated economic reality; for example, that there are forward markets for every commodity and for all conceivable contingencies in all future periods and yet that no one holds money as a store of value for more than one period. Even so, Arrow and Debreu did not manage to prove that such a general equilibrium is stable in the sense that it is actually attained from whatever position at which we start. In short, the Arrow-Debreu proof had more to do with mathematical logic than with economics.” (Blaug 2002, 36–37)

Now, even if one were ready to subscribe to this strikingly dismissive judgment, one might still wonder how such importance could be attached to only one single paper. But, of course, in Blaug's opinion, too, the problem is not the paper itself but the fact that “this paper soon became a model of what economists ought to aim for as modern scientists” (Blaug 2002, 37). This, he claims, is the result of serious misapprehension:

“In the process, few readers realized that Arrow and Debreu had in fact abandoned the vision that had originally motivated Walras. For Walras, general equilibrium theory was intended to be an abstract but nevertheless realistic description of the functioning of a capitalist economy and he was therefore more concerned to show that

markets will clear automatically via price adjustments in response to positive or negative excess demand—a property which he labelled ‘tâtonnement’—than to prove that a unique set of prices and quantities is capable of clearing all markets simultaneously. By the time we get to Arrow and Debreu, however, general equilibrium theory has ceased to make any descriptive claim about actual economic systems and has become a purely formal apparatus about a virtual economy; it has become a perfect example of what Ronald Coase [. . .] has called ‘blackboard economics,’ a model that can be written down on blackboards using terms like ‘prices,’ ‘quantities,’ ‘factors of production,’ etc. but which nevertheless is blatantly and even scandalously unrepresentative of any recognizable economic system.” (Blaug 2002, 37)

The last sentence obviously alludes to Debreu’s famous announcement in the preface of his *Theory of Value*: “Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations.” (Debreu 1959, x) By this remark, Debreu obviously tried to justify the temporary separation of logical analysis from empirical interpretation as a methodological virtue, which enables us to concentrate on the goal of achieving a maximum of conceptual clarity, whereas Blaug, on the contrary, obviously regards this methodological strategy as one of the worst scientific vices possible, the true purpose of which is to render the theory irrefutable by empirical evidence. Accordingly, the consequences of Debreu’s work for the overall development of post-war economics are characterized as having induced severe aberrations from the path of true science:

“Its leading characteristic has been the endless formalization of purely logical problems without the slightest regard for the production of falsifiable theorems about actual economic behavior, which, we insist, remains the fundamental task of economics. The widespread belief that every economic theory must be fitted into the GE mold if it is to qualify as rigorous science has perhaps been more responsible than any other intellectual force for the purely abstract and nonempirical character of so much of modern economic reasoning.” (Blaug 1992, 169)

He even goes so far as to claim that “[i]f there is such a thing as ‘original sin’ in economic methodology, it is the worship of the idol of the mathematical rigor more or less invented by Arrow and Debreu in 1954 and then canonized by Debreu in his *Theory of Value* five years later, probably the most arid and pointless book in the entire literature of economics” (Blaug 2002, 38–39). Thus, according to Blaug, it is the failure to adhere to the values and rules of a strictly empiricist methodology such as Popper gave to us that should lead to a complete rejection of Debreu’s research programme. In Blaug’s view, theoretical work in economics is only valuable if the theories developed can straightforwardly be put to empirical test. Anything else is idle play with aesthetically appealing, yet scientifically completely worthless symbolic constructions.

Blaug's negative assessment of general equilibrium theory in the vein of Arrow and Debreu is by no means an exception, and it should again be acknowledged that it is not restricted to philosophers of economics. Several economists, among them some very prominent ones, have uttered similar accusations. One of the first was Nicholas Kaldor, with an article boldly announcing *The Empirical Irrelevance of Equilibrium Economics* (Kaldor 1972). An in-depth reply by Frank H. Hahn (1974), one of the most respected but also most thoughtful general equilibrium theorists of his generation, in which he highlighted several of Kaldor's misunderstandings did not prevent others from reiterating his negative claims more or less uncritically. Actually, it now seems as if writing a most scathing critique of neoclassical economics in general, and of general equilibrium theory in particular, has become a kind of initiation ceremony for newcomers to the field of philosophy of economics—let me just mention a recent survey published under the title *Still Dead After All these Years* (cf. Ackerman 2002).

For the uninitiated reader it would certainly be surprising to learn, after all these negative assessments, that Arrow was among the first to receive the above-mentioned 'Nobel Prize' for economics since its inception nearly 40 years ago—he shared it with John R. Hicks in 1972, for their contributions to the theory of general equilibrium and economic welfare—while Debreu was the recipient in 1983, again for contributions to general equilibrium economics and additionally for the introduction of new analytical techniques to economic science. All things considered, it seems fair to say that general equilibrium theory has been regarded not only as a respectable area of economics, but even as one of the most important, and that the contributions of Arrow and Debreu have received a particularly high degree of esteem from their peers. Should we therefore not conclude that Arrow and Debreu satisfied a legitimate scientific need that was urgently felt by many economists at that time and then ask ourselves what particular kind of need this may have been instead of simply assuming that the majority of economists chose stubbornly to march forward on an obviously wrong track?

2. The Pragmatism of Keynes

It is often proposed that, as an alternative to the allegedly futile general equilibrium approach, we should turn to the writings of Keynes and especially to his *General Theory of Employment, Interest, and Money* (cf. Keynes 1973). This book was written with the intention of guiding economic policy through the storms of the Great Depression of the 1930s and of explaining why several economic measures, for instance the financing of public works in order to remedy involuntary mass unemployment, should be supported despite the common wisdom of the economics of his time. In fact, in the face of the Great Depression, the recommendation of many prominent economists at this time, among them Joseph A. Schumpeter and Lionel Robbins, was to leave it all to the market. In contrast, the most fundamental message delivered by Keynes was that in the face of grave economic depression a policy of *laissez-faire* would be seriously wrong, and he tried to explain this by means of several concepts he invented for this

sake, of which the notions of ‘effective demand failure’ and ‘liquidity preference’ are just two of the most famous.

Despite the announcement of his book as a *General Theory*, Keynes’ methodological approach was actually rather pragmatic. He formulated a rather loosely constructed system of a few aggregate equations called the ‘consumption function,’ the ‘investment function’ and the ‘liquidity preference function,’ which were all built on ‘psychological laws’ postulated on a more or less *ad hoc* basis in order to capture the essential characteristics of human behaviour in the face of economic uncertainty. The methodological role model for this *modus operandi* was not physics but medicine (cf. Hoover 2006)—and in fact Keynes himself once likened economists to dentists rather than proper scientists (cf. Keynes 1972).

We could therefore characterize his methodological procedure as a kind of bottom-up approach, in which practical problem-solving comes before general theory formulation. In contrast, general equilibrium economics in the vein of Walras was a clear instance of a top-down approach—at least Walras was quite explicit when he demanded, by analogy with physics, that “given the *pure theory of economics*, it must precede *applied economics*” (Walras 1954, 71). He justified this by pointing out how pure theory must abstract from the complexities of reality and must first develop a conceptual framework “in terms of which it carries on its reasoning. The return to reality should not take place until the science is completed and then only with a view to practical application.” (Walras 1954, 71) Thus, Walras was clearly committed to a top-down methodology and many general equilibrium theorists have since followed his example.

Keynes, in contrast, was much more pragmatic, as his contemporaries clearly perceived (cf., e.g., Klein 1994, 63). This is not to say that Keynes was hostile to theory. On the contrary, his *General Theory* explicitly aimed at a theoretical explanation of a practice already established in many countries at this time (cf., e.g., Dasgupta 2002, 73). But its theoretical character notwithstanding, the *General Theory* was firmly grounded in a concern with practical problems: “Keynes was an economic adviser, first and foremost; he was a theoretical economist and the creator of a new intellectual system second.” (Kaldor 1982, 2) This origin in practical problems explains his much more pragmatic style of analysis compared with Walras’ rigorous approach.

Admittedly, Walras’s contribution was not very common among economists writing in English at this time. It was, however, familiar to an economist as prominent as Schumpeter, who regarded it as being the most important contribution to scientific economics ever, thus having at least some indirect influence. Nevertheless, a top-down approach in general was quite prevalent at this time and manifested itself by an adherence to certain dichotomies of which the distinction between ‘pure’ and ‘applied’ economics was perhaps the most important, manifesting itself, for instance, in a separation of value theory from the theory of money. At the time of the Great Depression, most theoretical economists were still concerned with the very foundations implying that important areas of applied economics, most notably the theory of the business cycle, were still seriously underdeveloped. No wonder that many economists held a rather idealized view of how market economies worked!

This was an unsatisfactory situation that Keynes set out to solve. His *General Theory* not only aimed at justifying an alternative economic policy but also intended to change the focus and the methodology of economic analysis more generally. In the years of the ‘Keynesian revolution’ which followed, the *General Theory* stimulated a vast amount of empirical work, partly due to the fact that it was accompanied by the construction and improvement of national accounting systems providing statistical data and by the development of econometric techniques, which used these data for testing the propositions of the Keynesian system and for estimating the constants of its equations in order to make them amenable to actual empirical application. In this way, Keynes’ followers established macroeconomics as an empirical discipline using innovative tools in order to provide scientifically backed policy advice on relevant economic problems.

For some time, the rise of modern macroeconomics in the aftermath of the *General Theory* was justifiably regarded as the greatest success story of the social sciences after World War II. This was mainly due to a successful reorientation in the order of research priorities. Instead of concentrating on working out the very foundations of economic science, of which Robbins’ classic *Essay on the Nature and Significance of Economic Science* (Robbins 1935) is only the most striking—and, in the long run, nevertheless most significant—example, many of the younger economists followed Keynes in his problem-driven approach.

3. Testability and Meaning

So far, all of this could perfectly be explained in Popper’s terms. Such an explanation would run something like this: the Great Depression had definitively falsified all economic theories founded on belief in the self-adjusting forces of the market. As a consequence, Keynes and his followers formulated testable empirical hypotheses about the actual causal workings of the economy and used those which withstood empirical testing for governmental intervention into the deficiencies of the unregulated market system.

If we stopped here, there would in fact appear to be no room for anything like the work of Debreu. Even the work of Walras, which Blaug obviously regarded as being at least an honest attempt to refer to real economies, should be regarded as much too cumbersome to render it useful for the derivation of solutions to pressing practical problems here and now. As we have seen, it was not least a commitment to a top-down methodology that seduced traditional neoclassical economists of his time into neglecting the absence of self-adjusting market forces during the Great Depression. Many of them seem to have been stuck in ideal worlds of perfectly competitive markets, which prevented them from even perceiving what was going on in the real world. Robbins later famously regretted his contribution to economic policy advice during the Great Depression as “the greatest mistake of my professional career” (Robbins 1971, 154). Thus, the ‘Keynesian revolution’ obviously made a very important point.

However, the story of the ‘Keynesian revolution’ was more complicated than could be grasped by telling simple stories about the falsification of unrealistic economic theories by way of comparison with how the economy *really* works—

anyway, as the evolutionary biologist Richard Dawkins put it in another context: “‘Really’ isn’t a word we should use with simple confidence.” (Dawkins 2007, 416) This should be emphasized since philosophers of economics sometimes give the impression that they are confronting mainstream economics with an economic reality as such, to which they apparently believe they have unfailing access (cf., e.g., Lawson 1997b). There is, of course, something like an economic reality, but what it actually consists in, and which of its aspects are the most salient, is by no means an uncontroversial matter. The *General Theory*, for that matter, was still a highly stylized theoretical system based on ‘stylized facts’. What differentiated it from other theoretical systems of the same period were its choice of particular hypotheses and its methodology of combining and analyzing them on a highly aggregate level. As has already been said, this resulted in a more pragmatic approach, but this pragmatism did not come without a price. To begin with, it is well known that the *General Theory* was far from readily accessible to its readers. On the contrary, even its most sympathetic readers found it rather hard to make sense of. Paul A. Samuelson, for instance, called it a “badly written book” and even ascribed part of its success to its “very obscurity and polemical character” (Samuelson 1946, 190).

It is also well-known that for this reason Keynesianism was brought to the professional public mainly via interpretations and reconstructions by Hicks, Samuelson, Franco Modigliani, Don Patinkin and many others, who interpreted the Keynesian theory within the framework of, alas, Walrasian general equilibrium theory that had just been introduced to the English-language discourse on economics by Hicks in his *Value and Capital* (cf. Hicks 1946). Thus, what was called ‘Keynesianism’ in those days soon became more aptly known as the ‘neoclassical synthesis’, and it was general equilibrium theory that served as the conceptual framework for attempts to explain what Keynesianism was all about. The main device within this neoclassical reconstruction was the famous—in later years: notorious—IS-LM model as introduced by Hicks in his early review of the *General Theory* (cf. Hicks 1937). This model seemed to succeed in representing Keynes’ most important insights by means of only two intersecting curves, representing the equilibrium states of investment and saving (IS) in one curve and of liquidity preference and money supply (LM) in the other, all in only one diagram, and it became the standard tool for macroeconomic analysis during the next decades.

This story is, of course, not in the least new, and a lot more space would be needed to describe and explain the much more intricate developments of macroeconomics since then. What interests me here is the importance attached to the problem of understanding what Keynes actually wanted to say at all. Accordingly, the task of the self-declared Keynesians of the first generation was to explain the meanings of the concepts Keynes had invented and to explain the actual content of the economic theorems he had wanted to put forward. All this was far from straightforward, and even Keynes struggled with the implications of his theory. This is revealed by an anecdote referring to a discussion between Keynes and his pupil Abba Lerner, in which Keynes strongly opposed one of Lerner’s conclusions and did not realize that it had been derived from his own

theory, a fact that prompted Evsey Domar to say that he had better read the *General Theory*—and meant Keynes by this (cf. Colander 1984, 1572–1573).

In short, it was far from clear to anybody what the *General Theory* really meant, and this gave rise to several attempts at interpretation and clarification by framing Keynes's propositions in the much more lucid language of neoclassical general equilibrium. By casting the Keynesian ideas into the well-understood framework of general equilibrium theory, several “semantic squabbles that dominated much of the debate” (Blanchard 2000, 1378) were resolved, among them conceptual confusions regarding distinctions like *ex ante* and *ex post*, the notion of an equilibrium with unemployment or the content of Say's Law, a notorious proposition allegedly claiming that every supply would create its own demand, so that no general demand failure could ever occur. Although the expressed purpose of the *General Theory* was to refute this claim, it was immediately shown that no proposition of this kind could be found in the original writings of Say and that, worse, no serious economist had ever believed such a proposition. Much effort had to be exerted to resolve these issues and to explain the rationale of the Keynesian claim—since, of course, there was one. It was especially the important contribution of Don Patinkin that succeeded in consolidating the ‘neoclassical synthesis’ by interpreting and clarifying the Keynesian theory within a general equilibrium framework (cf. Patinkin 1956).

In retrospect, the macroeconomist Olivier Blanchard appraised these contributions by recognizing how this “systematic, general equilibrium, approach to the characterization of macroeconomic equilibrium became the standard, and, reading the literature, one is struck by how much clearer discussions became once this framework had been put in place” (Blanchard 2000, 1379). Some later critics found much delight in denigrating these reconstructions as being nothing more than a ‘Bastard Keynesianism’ scrambled from some of Keynes' less revolutionary ideas and that kind of neoclassical thinking Keynes had set out to free us from. However, one should not insinuate that they were intended to domesticate any subtle insights of the *General Theory*—as we have seen, they were simply trying to make sense of it (cf. Dasgupta 2002, 74). It is true that a later generation of self-declared Keynesians became increasingly discontented with the standard reconstructions and recommended that we return to Keynes' original writings—among them Axel Leijonhufvud, who in an influential study (cf. Leijonhufvud 1968) tried to point out important differences between Keynesianism à la Hicks and his contemporaries, on the one hand, and the ideas of Keynes, on the other. However, it was far from clear that these studies were really able to give a more faithful account of what Keynes had ‘really’ meant—which was an awkward question anyway. But even if it was true that Hicks and his contemporaries had streamlined some of Keynes' original ideas, it was their representation that made them accessible in the first place. This is nicely illustrated by the following little anecdote:

“I remember when Leijonhufvud's book came out and I asked my colleague Gary Becker if he thought Hicks got the *General Theory* right with his IS-LM diagram, Gary said, ‘Well, I don't know, but I

hope he did, because if it wasn't for Hicks I never would have made *any* sense out of that damn book.' That's kind of the way I feel, too, so I'm hoping Hicks got it right." (Lucas 2004, 13)

To sum up, although the *General Theory* had undeniably made innovative contributions to the transformation of economics into a more practical science with particular relevance to the pressing problems of its time, much conceptual work still remained to be done, and it was not least the success of this conceptual work that paved the way for actually testing and applying Keynesian ideas by means of econometric methods. It was this kind of formal reconstruction that helped to pinpoint the meaning of certain tenets of Keynesian theory in order to avoid pseudo-discussions about, for instance, the 'true' meaning of Say's Law or of the notion of involuntary unemployment, both of which were of first-order importance in Keynes' own thinking, but later turned out to be highly ambiguous, if not downright misleading (cf. Kates 199; deVroey 2004).

4. Progress and Its Problems

It is here that the philosophical accounts of Popper and his successors reveal their most serious shortcoming. Since Popper always tried to downplay the importance of language for the sciences, his philosophy provides no systematic place for the idea of explicating scientific terms and concepts. In contrast, the notion of 'explication' was rather central to Carnap's philosophical work (cf. Carnap 1952). This can be traced back to a more general difference between them in their assessment of the 'linguistic turn' in philosophy at the beginning of the 20th century. While Popper regarded the 'linguistic turn' as being an aberration, Carnap viewed 'linguistic' analysis as the most important remedy for what he called pseudo-problems in philosophy. In consequence, Carnap spent much effort on constructing formal language systems for the most precise expression of meaningful propositions. After having tried to work out, unfortunately not to much avail, a general criterion of significance in order to distinguish between meaningful and meaningless sentences once and for all and thus to get rid of metaphysics and traditional philosophy altogether, he turned to the logical analysis of science itself. His general motive, however, was still the elimination of pseudo-problems, which he saw as an important prerequisite for successfully investigating the real problems arising in the sciences.

The analysis of the meaning of scientific propositions and terms appeared to him to be of the utmost importance, for quite obvious reasons: "Obviously we must understand a sentence, i.e. we must know its meaning, before we can try to find out whether it is true or not." (Carnap 1936, 420) Hence, he tried to explain how even the most abstract theoretical terms and propositions attain empirical meaning via their connection to terms and propositions which are more directly linked to empirical observation.

This is not the place to explore the details of Carnap's philosophy, let alone to defend them against criticism. Many of the criticisms raised against it were certainly justified; actually, some authors recognized that Carnap was usually

among the first to criticize the shortcomings of his own attempts and to try better in further attempts (cf., e.g., Putnam 1988, xi-xii). However, some criticisms were positively flawed. This particularly applies to fundamental objections raised by Popper, Quine and Kuhn and their followers. In contrast to the ways in which these critics depicted his philosophy and the positivist approach in general, Carnap's idea was never to force everything that philosophers or scientists might ever want to say into the straitjacket of one codified formal language, it was only to increase the possibilities of expressing their propositions more clearly by this means. Moreover, he was by no means committed to any kind of foundationalist view built on dichotomous distinctions between theory and observation or between analytic and synthetic sentences, but, on the contrary, had explicitly relativized these distinctions to the respectively chosen language-system. The choice of a language-system was in turn to be justified solely by its contribution to the successful pursuit of one's scientific aims. Thus, there was no *a priori* choice to be made for or against a certain language-system, nor was there any justification for restricting scientists to the use of only one these language-systems. Carnap's famous 'principle of tolerance', which could already be found in his *The Logical Syntax of Language* (Carnap 1937), expressed this view most clearly. In a later formulation, this principle amounted to the following claim:

"Let us grant those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. Let us be cautious in making assertions and critical in examining them, but tolerant in permitting linguistic forms." (Carnap 1950, 221)

It seems therefore only adequate when Alan Richardson recently emphasized the genuinely *pragmatic* approach of Carnap's philosophy (cf. Richardson 2007a).

Overall, recent investigations have shown that the differences between Carnap on the one side and Popper, Quine and Kuhn on the other had usually been vastly exaggerated in order to emphasize the originality of the latter's contributions (cf., e.g., Friedman/Creath 2007; Richardson/Uebel 2007). The general hostility towards anything which appeared to be committed to any kind of positivist philosophy strangely resembled the hostility towards anything which appeared to be committed to neoclassical methods or propositions in economics. In both cases, a general label attached to positions one wanted to overcome replaced a more thorough analysis of the contributions of one's predecessors and the merit of one's own additions to it. There were, of course, differences, but many were only of a gradual kind. As to the difference between Carnap and Popper, it was repeatedly and aptly emphasized by the former that the latter tended to exaggerate the differences between their views (cf. Carnap 1963, 877).

Nevertheless, there was in fact one important difference between the conceptions of Carnap and Popper, to which I have already referred above: it consisted in Carnap's explicit recognition of the importance of language and meaning analysis for philosophical and scientific problems that was rejected by Popper. While

Carnap emphasized the importance of an explication of scientific concepts and the benefits of constructing formal language-systems in order to express one's ideas more clearly, Popper sweepingly dismissed a concern with these aspects of science as being derived from an 'essentialist' view, most clearly expressed in his discussion of the role of 'nominal' and 'real' definitions in science (cf., e.g., Popper 2003, 22).

This is not to say that Popper was opposed to the use of formal analysis in philosophy or science, or that he did not try to express his thoughts most clearly. Quite the contrary, his literary style was often of admirable lucidity, although he sometimes obscured the substance of philosophical topics by coining his own idiosyncratic terminology—let me, as just one example, point to his nitpicking distinction between 'confirmation' and 'corroboration' of hypotheses. But Popper was clearly opposed to the general spirit of the 'linguistic turn' in philosophy and his followers adopted this denial of the importance of language as a topic for philosophical analysis to the detriment of their own conceptions.

A clear instance of this can be found in the work of Imre Lakatos, who developed the most popular approach to the philosophy of science among philosophers of economics for quite a time. Conceptual and 'linguistic' problems are virtually absent from this approach. Although there is an explicit distinction between theoretical and empirical progress in Lakatos' 'methodology of scientific research programmes', closer scrutiny shows that on this account both types of progress are exclusively defined by empirical criteria. In Lakatos' view, theoretical progress consists solely in the derivation of novel predictions while empirical progress is defined as the actual confirmation of these predictions (cf. Lakatos 1970; Shearmur 1991). This is precisely how Blaug wanted to assess the merits of the 'Keynesian revolution' (cf. Blaug 1996, ch.16).

However, as Larry Laudan pointed out in his *Progress and its Problems*, much important work in science consists in something completely different: it consists in the clarification of concepts that are central to the paradigmatic theory or research programme dominating the respective scientific discipline (cf. Laudan 1977). Hence, overall progress in a particular scientific discipline can only be assessed by distinguishing between empirical progress and conceptual progress, of which the latter consists in the solution of problems that arise within a particular theory due to the way in which it is articulated and logically organized. Accordingly, empirical and conceptual problems are not located on the same level of analysis; conceptual problems are more clearly theory-dependent, which means that they only occur within a particular theoretical structure:

"If empirical problems are first order questions about the substantive entities in some domain, conceptual problems are higher order questions about the well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first order questions." (Laudan 1977, 48)

More precisely, Laudan distinguishes between internal and external conceptual problems, the first being concerned with problems of inconsistency within a theory, but also, and more importantly, with problems of conceptual ambiguity (Laudan 1977, 49–54), while the second concern problems of the compatibility of one theory with others which are already accepted. Both kinds of conceptual problems are of paramount importance for the development of a scientific discipline but have been neglected in most accounts of scientific progress until today. Those who, like Kuhn, have considered them at all, have depicted them as a source of irremediable incommensurability and thus of irrationality. A more balanced view on how science proceeds would instead accept that conceptual problems, i.e. problems of the meaning and meaning change of central terms and propositions of a scientific theory, are part and parcel of the work of scientists and have to be solved by explicit linguistic analysis, sometimes by using deliberately constructed language-systems in the sense of Carnap and sometimes by using more informal methods.

Let me close this section by saying that in placing Laudan alongside Carnap, I do not intend to give the impression that Laudan himself saw himself in such a line. On the contrary, Laudan characterized Carnap and the other logical positivists as purely empiricist philosophers and thus put them into the same camp as all the others who had neglected the conceptual side of scientific progress (cf. Laudan 1977, 47). To me this seems clearly false and can be explained by an uncritical acceptance of the caricature of logical positivism already established at this time. But even if Laudan were right, this would not matter much since it is the significance of conceptual problems that is important for my present purpose. Compared to this, my historical thesis about certain continuities between the approaches of Popper and Lakatos on the one hand, and between the approaches of Carnap and Laudan on the other, is only of secondary importance.

5. Debreu as the Carnap of Economics

Now, what bearing does the difference between the views of Carnap and Popper concerning the analysis of meaning and the construction of language-systems have on the assessment of economics theories and general equilibrium theory in particular? The answer should be quite obvious. I have claimed that economics, especially after the ‘Keynesian revolution’, was haunted by severe problems of, so to speak, self-understanding that were due to severe conceptual ambiguities. To a certain degree, they were certainly caused by unfortunate terminological choices and conceptual vagaries of the *General Theory*, but to some degree they only revealed a more general conceptual confusion prevalent at this time. At any rate, they demanded a more thorough formulation of the conceptual foundations of the discipline. Lionel Robbins’s important essay quoted above was only one of the most important attempts in this direction. As we have seen, compared to this fundamental level of analysis, Keynes’ contribution was more directly concerned with the solution of actual policy problems, but as we have likewise seen he had to introduce some innovative concepts, the precise meanings of which had to be understood—and, in cases of ambiguity, regimented—in order to avoid pseudo-

discussions about, for instance, the validity of Say's Law or the existence of an equilibrium with involuntary unemployment.

This kind of tidying-up of the *General Theory* was done by the first generation of Keynesians, among them Hicks, Samuelson, Modigliani and Patinkin already mentioned above. The work of Arrow and Debreu, the origins of which, like the origins of Keynesianism, are to be found in the 1930s, may still look quite esoteric compared to the work of the Keynesians, even those of the neoclassical synthesis, but the example of Hicks who had worked in both areas indicates that there may be some connection. In fact, those applying the framework of general equilibrium to the reconstruction of Keynes could in turn profit from the more fundamentally conceptual work of Debreu and others. Thus, if we assess the work of Debreu in the light of Carnap's idea of constructing language-systems for science, we might be in a better position to grasp its real purpose and positive significance for the development of the discipline. Some authors at least pointed in this direction, although without making an explicit connection to logical positivism. For instance, in their introductory appraisal of the efforts to prove rigorously the hitherto only informally postulated existence of a general equilibrium, John Eatwell, Murray Milgate and Peter Newman clearly pointed out that

“[...] this *could* have been a mere matter of mathematical aesthetics, but in the process something remarkable happened. For in setting out their models of general equilibrium with enough clarity to permit exact proofs of consistency, these pioneering theorists were forced to say *exactly* what they meant. So it came to pass that one of the virtues of the prevalence of general equilibrium theory is that to know what one is talking about is today rather less uncommon than it used to be.” (Eatwell/Milgate/Newman 1987b, xii)

This may still sound as if the increase in conceptual clarity was only an unintended by-product of these contributions, but actually it seems to be more adequate to say that it was precisely the purpose of these attempts. The conceptual confusions accompanying the 'Keynesian revolution' were only one indicator of the ambiguities connected with such central concepts like the concept of 'equilibrium' in single markets as well as in the economy as a whole. Only recently was it fully realized that “there can be no serious and constructive debate among different trends in economic theory until the different meanings of the concept of equilibrium are defined clearly and precisely and the reciprocal relations are understood in sufficient depth” (Vercelli 1991, 2). This is exactly what was done in the aftermath of the 'Keynesian revolution', not least by using important distinctions like those between the existence, the stability and the uniqueness of equilibrium and the analytical methods to prove them for certain well-defined models of an economy. Thus, sweeping claims about 'the' equilibrating tendencies of 'the' market economy, which were typical of many discussions in pre-war economics and which are still widespread in many political discussions, were replaced by more differentiated and cautious claims. Debreu's own work certainly did not contribute directly to the clarification of the ideas of Keynes, but

it did establish new standards of conceptual clarity, upon which many of the 'Neo-Keynesians' of the 1970s in particular could fruitfully draw.

By saying this, I do of course not wish to claim that Debreu's work should be considered sacrosanct against all criticism. As in the case of Carnap's formal analysis of the languages of science and philosophy, it may have generated exaggerated hopes that analysis from now on could exclusively proceed in the precise ways of formalization and axiomatization. But these exaggerations are not part and parcel of the general equilibrium approach as such. If we look at Arrow and Hahn's *General Competitive Analysis* as one of the most important successors of the *Theory of Value*, we see that already a more pragmatic and sober attitude towards the potentials and limitations of this particular kind of economic language-system had won through (Arrow/Hahn 1971). Especially the chapter on Keynesianism hints at the restrictions of an interpretation of Keynes' system in the language of Arrow and Debreu (cf. Arrow/Hahn 1971, ch. 14), but this does not mean that nothing can be gained by interpreting certain aspects of it in this way. Quite the opposite, much has been gained by doing so to date.

Accordingly, I do not intend to say that everything theoretical economists might ever want to say must necessarily be cast in terms of general equilibrium theory, nor do I intend to say that Keynes' theoretical contributions could only demand scientific respect insofar as they are capable of being cast in these terms, and I certainly do not intend to deny the scientific merits of Keynes altogether. On the latter, I completely agree with Mark Blaug that it is one thing "to kill the myth of Keynes as a veritable knight in shining armour riding out against the wage cutters, the advocates of Say's Law and the proponents of the Treasury View, and quite another to deny the genuine novelties of Keynesian economics, as if the General Theory were only the special theory of rigid wages and the liquidity traps. There really was a Keynesian revolution!" (Blaug 1996, 675)

I wish to deny, however, that general equilibrium theory in the vein of Debreu must necessarily be interpreted as an attempt to force one particular language-system onto the whole discipline of economics. Nothing in the use of formal language-systems as such suggests such an exclusivist view and, as we have seen, such an intolerant stance would be clearly alien to the spirit of Carnap's philosophy. Blaug's impression to the contrary may be an adequate description of the factual behaviour of many theoretical economists in the aftermath of Debreu, but this is not something that is inherent to the general equilibrium approach as such, and it does also not appear to be the correct description of Debreu's own attitude. It is true that E. Roy Weintraub and others recently stressed the connection between Debreu and the Bourbaki programme in meta-mathematics and have thereby suggested a foundationalist attitude of Debreu (cf., e.g., Weintraub/Mirowski 1994). But even if there is such a biographical connection and, more importantly, a demonstrable impact of the Bourbaki programme on Debreu's work, we are not committed to interpreting it as being foundationalist in the sense of being fundamentalist and exclusionary. A more charitable account of what was performed and achieved by this strand of research could easily refer to Carnap and his increasingly liberal account of formal language-systems and their benefits for science. Seen in this light, the language of general equilibrium

theory could be seen as one but only one of several admissible language-systems in economics, albeit a very powerful one.

It should therefore be deplored that the obvious virtues of this particular language-system have misled some macroeconomists, especially Lucas and other ‘rational expectations’ theorists, into claiming that only an economic theory that is cast in terms of the general equilibrium approach should be regarded as scientifically respectful. If this claim on the part of the rational expectations theorists was to be interpreted as an empirical hypothesis about how macroeconomics might become a more successful discipline, they were of course perfectly legitimate—but apparently wrong if judged by the empirical evidence that is available now. Nevertheless, the importance of their attempts at theoretical integration and unification cannot be denied. Even economists sympathizing with the Keynesian approach have been able to acknowledge that this approach, its empirical inadequacy notwithstanding, contributed something significant since it “produced valuable new conceptual tools for analyzing the dynamics of a market economy in contexts involving risky prospects and incomplete information” (Grandmont 1989, 267).

If the claims of the rational expectations theorists were intended as methodological prescriptions that were to be accepted on *a priori* grounds, they cannot claim support from the logical positivists’ philosophy of science. There is no need for and no benefit to be had from committing economists to one and only one kind of language-system, as Carnap’s principle of tolerance has shown. But there is also no need for and no benefit to be had from forbidding the use of a particular language-system, especially one with the undeniable virtues of the general equilibrium approach. One could therefore give a more balanced account of the development of 20th century economics allowing not only for the shortcomings but also for the benefits of the temporary narrowing in the way in which economics was performed after World War II. Viewed in this way, the following description seems to be more adequate than the critical accounts of Blaug and his followers:

“Within economics, there are many different topical concerns, such as international trade, development, economic history, labor markets, public finance (and, more broadly, the interplay of economic and political institutions). Before the mathematicization of economics, these fields maintained significant intellectual autonomy. [...] There were, if you will, a number of regional dialects of ‘Economesse,’ dialects that were close to being distinct languages. [...] The newly dominant dialect of mathematical modeling lacked some topically important vocabulary; rather than speak in an unfashionable dialect, some things were just not discussed. This contraction of topical concerns had its positive points. The unification of dialects, for example, means that insights obtained in one context can be easily transferred to another. [...] I assert that the dominant dialect of mathematical deduction possess some powerful grammatical advantages.” (Kreps 1997, 65–66)

Kreps thoroughly recognizes that “the loss of certain topical concerns represented a real loss” (Kreps 1997, 66). Nevertheless, his overall balance of the development of economics after World War II is positive: “As time passes, we see first a narrowing of topical concerns as the language is unified and then a widening of concerns of as the language develops.” (Kreps 1997, 66) The period of narrowing consisted in a more general acceptance of the general equilibrium approach, which was only part of the more comprehensive ‘formalist revolution’, including the rise of Keynesian macroeconometrics. However, since the heterodox approaches resisting this rise of a new economic mainstream and its formal methods never completely disappeared, their continuous criticism could be and would be taken up in later stages, being integrated by piece-meal development wherever possible. For instance, the ‘institutionalist’ and the ‘informational’ revolution referred to above grew out of attempts to take up again some important problems of real economies—in these cases the existence of transaction costs and of informational problems—without losing what had been achieved with respect to conceptual clarity and analytical rigour.

The attempt to reconcile Keynesian macroeconomics with neoclassical microeconomics in order to provide for a ‘microfoundation’ of the latter can be comprehended in the same way by using Laudan’s notions of external conceptual problems—evaluating the compatibility between these two branches of economic research was a legitimate and important research programme that contributed to a better understanding not only of the intertheoretical relationships of economic science but also of the workings of real economies, because it improved the consistency of the theoretical models by which we can only understand these workings in the first place. Saying this is not tantamount to postulating that only a macroeconomic theory founded on a (particular kind of) microeconomic theory contributes to our knowledge. It is neither necessary nor desirable to ignore the systematic shortcomings and temporary imbalances of the particular ways in which economic science has developed during the last decades in order to be capable of recognizing also its major achievements and thus the merits of its mainstream.

6. What’s Wrong with the Philosophy of Economics?

So this is the solution to my puzzle. I have argued that the development of 20th century economics could be viewed as having followed a thoroughly rational course, by and large, if one takes into account not only the importance of empirical problems but also the importance of conceptual problems of scientific research. Post-war economics has experienced not only an increase in empirical work concerning the testing and application of its theories but also an increase in conceptual work leading to more scientific clarity. This is not to say that the whole development was rational in every detail, in that there have been no aberrations and exaggerations whatsoever—there certainly have, and in fact it would be irrational to expect otherwise. However, in order to identify them we first have to specify from which point of view they are to be assessed, and this is by no means an easy task, hence the rationality of economics as a working hypo-

thesis. Although some developments may strike us, at first glance, as downright irrational, the most important task of the philosopher of economics is certainly not to tell the economist from an external or even higher point of view what to do or how to proceed.

Instead, philosophers of economics should be most anxious to avoid bold normative statements and should devote more effort to understanding the rationale of the various ways in which economists are approaching the diverse problems they confront. It was characteristic of what later came to be called the ‘received view’ in the philosophy of science that it tried to formulate bold normative criteria in order to demarcate meaningful from meaningless or scientific from non-scientific statements. However, this aspect of the logical positivist endeavour long since disappeared, and rightly so. There is no external or higher point of view from which one can judge which statements, theories, or methods are sound and which are not. This does not mean that the philosophy of science must not be normative at all; but it means that its normative claims need to be aligned with the research realities which economists are actually confronted with. When the claims of philosophers of economics and the practice of economists collide, it will often be the former that have to give way. In this sense, the practice of economists is not just an object of the assessment by philosophers of economics, but also serves as a test case for the soundness of the philosophy of economics itself.

What we are trying to achieve, then, is a kind of ‘reflective equilibrium’ in the sense of Nelson Goodman (1983) and John Rawls (1971), i.e. an epistemological state in which all kinds of relevant input are integrated into the most coherent picture possible. However, since it is very difficult, if not impossible, to find out whether such a ‘reflective equilibrium’ has actually been achieved—there may also be more than just one such equilibrium—one should always be cautious in one’s judgements, especially if these judgements concern work performed in another branch of the scientific enterprise. The contribution of philosophers of economics may be of considerable help for economists, in that they can draw on the rich insights of philosophy of science in general, which in turn systematizes and rationalizes the various experiences of various scientific disciplines in order to understand how the acquisition of knowledge in general works, and to which extent it may be relied upon. Philosophers of economics draw on these more general investigations, but they also contribute to them by providing intimate knowledge of the specific conditions of investigating the economy. But they can, of course, only do so in a fruitful way if they actually have this knowledge, and a prerequisite for this is a great deal of curiosity and, as much as possible, an absence of preconceptions and ideologically motivated distortions.

I have claimed that there actually have been, and still are, such distortions. If I am right, they may have different sources. One of them might be historical. I have argued that philosophers of economics have relied for too long on a one-sided picture of science, which can be attributed to the writings of Popper and, to a lesser extent, Lakatos. Since these authors placed too much emphasis on the empirical aspects of scientific research and not enough on its conceptual parts, reliance on their views quite naturally invited a broad dismissal of neoclassical

economics in general and general equilibrium theory in particular, because these have their strengths in precision of language and conceptual unification and their weaknesses in empirical applicability and confirmation. The writings of Blaug quoted above are only one of the most explicit manifestations of this tendency and its philosophical sources. A reliance on the more balanced approach of Carnap could have prevented such biased appraisals, but when philosophy of economics emerged as a separate discipline with its own journals, textbooks, and conferences in the 1970s, the dismissal of Carnap's writings and the views of the logical positivists in general had already won through (cf. Richardson 2007b). It is only recently that historians of philosophy have started to evaluate this period more thoroughly and to give a more adequate account of Carnap's and the logical positivists' views and their relationship to those of their successors.

This may at least partly explain why so many writings by philosophers of economics were characterized by an undifferentiated hostility towards everything that bore the stain of being neoclassical in descent. Particularly after the economic mainstream had started to express its ideas in an increasingly mathematical and even axiomatic language, it became only too obvious that much of the work performed in the course of mathematical and axiomatic reconstruction of economic ideas was at best only indirectly connected to the task of testing these ideas and applying them to practical problems of economic policy making. Since the Popperian strand of the philosophy of science could not explain, let alone justify, this kind of work, those who were opposed to this development of the discipline anyway could use the philosophy of science as they perceived it as an apparently objective and authoritative weapon against the neoclassical mainstream and its most abstract manifestation, general equilibrium theory. Some at least appreciated it as 'applied mathematics' but denied that it made any contribution to economics as an empirical discipline (cf. Rosenberg 1992). Others tried to reconstruct and justify it along the lines of Lakatos' less dogmatic approach as the hard core of economics (cf. Weintraub 1985), but then they felt obliged to show that it resulted in progress in the sense of Lakatos (cf. Weintraub 1988), which, as we have seen, was still defined solely in empirical terms. But this could not convincingly be shown.

Of course I do not claim that philosophers of economics still think exclusively along the lines of Popper and Lakatos. Today, Popper as well as Lakatos have lost much of their reputation among philosophers of economics. After an initial euphoria, starting in 1974 with maybe the first official conference on the philosophy of economics ever, with contributions by philosophers of economics as well as prominent economists (cf. Latsis 1976), a certain disappointment in Lakatos' approach started to gain ground—even a certain degree of hostility (cf. Blaug 1991, 500). According to Blaug, the reasons for this change in attitude were twofold: one was precisely the flexibility of the approach, which was now perceived as a weakness since its consequence was that one could never be sure that one had reconstructed the particular economic theory in the correct way—and how could one hope to make unambiguous normative claims about an economic research programme if it could be reconstructed in very different ways?

The second reason was that an increasing number of authors realized that one simply could not understand what many economists were doing if one relied solely on empirical criteria. The dismissal of so many respected economists as being simply off the mark increasingly appeared to be unsatisfactory—although Blaug himself still holds fast to it. Thus, it is fair to say that the philosophy of economics today is much more diversified than it was in the 1970s when it began to form as a self-conscious field of research. Nowadays, there are all kinds of philosophical approaches from which economics is interpreted and assessed. Nevertheless, hostility towards neoclassical economics in general and general equilibrium theory in particular is still very common, although it has become much harder to justify by reference to a philosophical position that everyone would accept. However, the lack of a convincing and generally accepted argument may actually have amplified this hostile attitude, for it now seems to have changed its nature; from having once been the result of a philosophical assessment it has now turned into an axiom for most philosophers of economics—the claim that something is wrong with economics does not appear to be a point that actually has to be argued in all seriousness; instead it is the indisputed premise from which the discussion usually starts.

The upshot of this is quite ironic: it seems that, due to the demise of the Popperian and Lakatosian approach, the philosophy of economics has lost the normative ground from which it once used to attack the neoclassical mainstream; but in consequence, the majority has not changed its attitude towards the mainstream, but has immunized itself against this unwelcome metatheoretical result—and thus has committed one of the deadliest sins possible in science, according to Popper. To a certain extent, this is only too understandable: since virtually no economist takes notice of what philosophers of economics do, the latter always live in fear of the possible irrelevance of their work; and the loss of normative ground has only aggravated this fear (cf. Davis 2003). Today, opposition against the neoclassical mainstream and against general equilibrium theory has become a kind of defining characteristic of the discipline of philosophy of economics as such, and the fact that today what once could be meaningfully called ‘neoclassical’ has changed so much that the term itself has virtually lost its meaning seems to have made not much impact so far (cf., however, Colander 2000). However, if today a moderate pluralism with respect to theories and methods seems to gain ground in the philosophy of science in general as well as in the philosophy of economics, there is no reason why it should exclude, of all things, those theories and methods which are accepted by a majority within the very discipline one is allegedly trying to make sense of.

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