

Why the "FACH simple" views the unity of science as inexorable oblivion. Reply by F. VANDAMME

Ronald Commers, in his interesting review of my book, makes a few remarks, that give me the occasion to clarify my position in greater detail than I was able to do before. I thank him for this. I first want to point out that I understand very well his position, and that the best method of making our public see the relative justification of his position, consists in pointing out also its relative limitations. As I understand it, it is the point of view of the average consumer of science.

We do agree with Kuhn that also in science, the consumption of science is of common occurrence. Consumption of science means an uncritical application of a theory with the purpose of immediate practical use. This consumption impedes the arising of questions and criticism of the theory used, for these slow down the process of immediate application. From the point of view of the science consumption only the immediate results are important even if some ad hoc extra conditions have to be introduced. Also the consumer of science does not only want to get his data in the way he is accustomed to, but also he wants these data which he is accustomed to. All new data he refuses to see. Therefore, as Kuhn remarks, science consumption has an important conservative force.

But so it becomes immediately clear, why again and again the consumer tells that the new theories are irrelevant, that the results of the unity of science are useless, etc. For Old results are in the synthesis approach brought forward in a different frame and even eventually moved to the background. New elements on the contrary are brought in the center. So it is easy to understand why a consumer of science does not feel much for a new theory and even he may think the theory to be empirically senseless, because of the old concepts, he was accustomed and stuck at, have almost entirely disappeared.

A "Fach simple" viz. someone who is stuck at the belief that his field of study is isolated from the other fields (even if he is doing some lip-service for integration and synthesis) is to a certain extent comparable with the consumer of science. Once he is confronted with structurations of his field a) dependent on neighbouring or other disciplines and b) with new data brought to the fore, he also doesn't recognize anymore his own old conceptual frames and therefore believes the new approach has no empirical significance (cfr. the Einsteinian revolution). This explains we think, the claim of many naive scientists that synthesis of several disciplines means only a return to a somewhat more abstract level and to this extent a more "emptier" level. The more we unify the more we have to make abstraction, and therefore the more irrelevant and obviously the unification becomes.

R. Commers illustrates — we believe — this current limited misconception of the role of unification and abstraction in science. Abstraction in science is necessary but functionally dependent on the possibility of the later stage of concretisation. Abstraction is only acceptable to the extent that it directly or indirectly permits the explanation of more concrete data. The history of the unity of science proves that the unification has been very fruitful for the finding and formulation of new laws, with more concrete import. Biophysics is a beautiful example here, but today we can also point to important results in this respect, e.g. Prigogine (his fundamental laws on irreversibility of processes in chemistry, he and his collaborators have applied on thermodynamics, theoretical biology and even on the sociology of urbanism), Thom R. (his mathematical theory of catastrofes with application in mechanics, biology and linguistics) and Mandelbrot (his objects fractals).

In our suggestions for developing a frame for the synthesis between economy and communication, and ecology and communication we have had explicitly the same requirements in mind. For this sake we have indicated how "money" could be more

efficiently approached in such a frame, and how the organisation of the production, the market, the distribution, the consumption has to be looked at from the double interconnected point of view. "Market", "production" etc., (as well as real processes or as theoretical concepts) presupposes communication, as many authors have remarked. Some of the most important authors which have, to our knowledge, tried to relate communication and economy are : Jakobson, Rossi Landi, Parson T., Tarde, Targot ... Marcelo Dascal even in recent work argues that already Leibniz points out similarities between money-systems and signalling systems So an approach to these in a synthetic communication-economic frame is important and scientifically promising.

Indications of the fructuality of this synthesis are f.i. a) the possibility of interrelating the Marxist economic approach and the neoclassical approach upto a certain extent and b) the possibility of getting a frame for introducing the ecological considerations in economy by intermediary of communication and the remodeling of some basic notions on economy necessitated by it. An analogous point can be made about economy and ecology, as we have explained in our book.

Therefore the point viz that the so-called oblivation by synthesis is based on a wrong and onesided view on "synthesis" and "interdisciplinary work" in general, is the central remark that we want to make at the occasion of Commers review. There are however still some less central remarks we want to make. So about Commers critic on Morris 'notion of communication'. Commers argues that both "exchange" and "communication" must be seen as modi of interpersonal relations. However most work done in this field indicate how the human communication is related to and imbedded in wider forms of communication and has therefore to be based on a general approach to communication (Jakobson, Wiener, Shannon, Sebeok, etc...). Here again the "unity" is enriching instead of obliating.

Commers also asks some questions about the general structure of the book. I want to take this opportunity to give the reader a general survey. In the introduction we referred to the main topics of discussion and the motivation of selecting them. The motivation was based on an analysis of the several aspects of the philosophy of science. These are : (a) the study of the role of sciences in society, (b) the worldview present in the sciences, and (c) the foundation of science. We have chosen for the last approach. Only incidentally we have taken up the other aspects.

We have tried to situate this approach to economic theory in a general methodological frame. So e.g. in chapter 2 we discussed the subject of economic theory. We have tried to relate this discussion to the general problem, with its relative importance, of the determination of the subject of a science. Neatly related to the discussion of the subject of economic theory, was the problem of economic rationality. Here again the link of economic rationality with "rationality" in general was discussed. Poppers work seemed rather usefull in this respect.

The second main topic in the book is the discussion of evaluation of theories and more in peculiar the evaluation of economic theories (chapter 4 and 5). We approached this topic in the traditional way by discussing the several theories of control (verification, confirmation, falsification). However as we believe that this is only a peculiar — it is true an important one — aspect of the general evaluation of theories, we thought then usefull to go on and introduce a general frame for evaluation and to discuss the interrelations of the several "dimensions" of evaluation.

The third and last topic of the book then is the problem of unity of science and the relevance of this for economic theory (chapter 6, 7, 8). Some approaches in this unity of science movement are treated (reductionism versus synthesis). Next some indications and partial attempts for synthesis between economic theory and communication theory, linguistics, ecology and logic are introduced and some hopefull results are mentioned. Let us

also remark that this book is intended as an introduction in the sense that a set of relevant topics in the field of the foundation of economic theory are treated. On each such topic only some lines of approach and some results are sketched.

About the important topics which we believe to be missing we would like to mention : (a) the topic of theory change, and (b) dialectics and economy. We have touched on both these topics incidentally. However, although they are very important today, we didn't treat them at length. In other publications we have already gone into these problems. So e.g. we have treated the problem of theory change in general and more specifically the transition from classical towards neoclassical theory in some articles. We have confronted the Kuhnian approach and its developments with the economic theory change.

But let us return to the review. There is a set of rather external and stylistical remarks made by the reviewer. I will not go much into detail here. Some of them are important (e.g. the wish to stress the difference between "economy" as the object of the economic theory, and as the economic theory itself). To take them into account in a new edition could eventually make it easier for the general reader to get the idea. Others are in my view rather a question of taste (e.g. treating a subject in one chapter or splitting it up in several chapters, etc.). There are however also critical remarks here which I believe to be based on a wrong or partial interpretation of the text. For instance the remark on the tautologies in the book. So the reviewer writes : "We found some paragraphs in this part of the book, that seem to be tautologies in our view". First of all let us remark that there is in principle nothing wrong about the use of so called tautologies. This is clearly demonstrated in logic, mathematics and all science' What is important is the kind of use made of it. So-called tautologies or analytic sentences as well as synthetic sentences can be used in a fructual or an useless way. This is what matters.

To illustrate his accusation the reviewer quotes : "Wat de invloed betreft van mikro-reduktie op de wetenschappelijke ruimte is het duidelijk dat ze zal zorgen voor een sterke toename van de systematisatie. Immers alle delen van de wetenschappelijke ruimte worden door de mikro-reduktie met elkaar in relatie gebracht..." The reviewer then goes on : "Is there another point in stating this, then in saying simply if micro-reduction brings together all levels of scientific activity and investigation, then micro-reduction brings together all levels of scientific activity and investigation". This is for sure a rather free paraphrasing of the idea expressed. The more if we read what follows immediately the quoted text : "Dit is een vruchtbaar iets, want het geeft aanleiding tot een herstruktureren van sommige delen van het wetenschappelijk universum"... So what we expressed was the following : "In reduction, the components of different theories are related to each other. Empirically however this proves also to be important (biophysics is here introduced as an example) for it proves to be a cogent stimulus for new research and new fructual restructurations". So again a plea for unity of science. We think it to be important for reason of its heuristical, epistemological and antropological value. With this plea we like to finish this note.