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Discussion Paper

by

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on

William W. Bartley's

THE DIVISION OF KNOWLEDGE

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1. In this session I have been asked to comment on William Bartley's paper on "The Division of Knowledge". First of all I must say that I agree almost completely with the theses that Bartley has expressed (mainly with those against the Wittgensteinians), and this is a very embarrassing situation for a discussant. I believe that the most proficuous job I can do is to focus on a specific point of the paper, so as to show then why I agree with the theses expressed by Bartley, and if it is possible to defend them from a type of approach relating to the problem of unity of sciences that I consider <u>objectively</u> incompatible with them.

2. We want to use a Kant's quotation, that has become very fashionable in intellectual quarrels, even though we might risk to appear a little banal: "May God protect us from our friends. From our enemies, we can try to protect ourselves".

   We believe that this should be the correct way as to how the critical rationalist should react towards theses that would pretend to be in agreement with his way of thinking. In a more crude way, we believe this is what a critical rationalist should say in relation to Walter Weimer's theses.

   Bartley asks himself the question (1) if any, and eventually what, disagreement between his positions and Weimer's exists. We will discuss further this problem in some specific points more properly concerning the role and nature of methodology: right now we will limit ourselves to argue that <u>if</u> the main Weimer's theses are true, then the entire position of Bartley that "there are no intrinsic logical reasons requiring the division of knowledge" and also that is "at least theoretically possible for there to be a unity of the sciences" it is strictly false.

3. Weimer's paper (2) it extraordinarily rich and stimulating, and just for this reason it should deserve all our plause. Moreover it admirably argues by means of examples drawn from the most advanced scientific

research -even that fundamental field that is Prigogine's thermodynamics. Nevertheless, we believe that it does not succeed either in demonstrating the greater part of the specific theses, nor in tracing a 'new' general theoretical frame within which the future philosophy of science and epistemology should move. We are going to start from some of the specific positions, then we are going to discuss the general theoretical frame proposed.

We believe not to be wrong when we state that in Weimer's work a crucial role is reserved to the concept of limits to the explanation that it would take place in the kingdom of 'high complexity': where "models are either more complex than the phenomenon under study, or equally complex". Weimer, taking after von Hayek, believes that "for high complexity we are limited to understanding the abstract regulative principles of the order rather than ever being able to model its particulars (either deterministically or deductively)" (3).

Weimar correctly defines the limit imposed to models in the kingdom of high complexity as the 'von Neumann's conjecture': and from this conjecture the apodictic statement that says "this puts the program of explanation by covering laws of particulars in complex domains in the realm of utopian fantasy" (4) is a perfect example of non sequitur. After all, it is not only their character of conjectures that is preventing from considering true Weimer's theses. In fact, is it the statement that the explanation by covering laws becomes impossible a logical consequence of the 'fact' that at a determined level of complexity the model is more (or equally) complex than the object to modelize? The problem of the explanation tout court has a content which is common to that of the economy of the explanation itself (in the classical meaning after someone like Mach, for example): but the two problems do not coincide at all.

Laws' characteristic purpose is to introduce regularities (and it is still unessential that they should reflect a pre-existing ontological order): even if regularities should be of a more complew order than the object (of the complex) to be explained, this would not take anything away from the value itself of the laws and from their explicative and predictive power. Whether then it is 'economically' convenient to use these laws or not is a completely different problem.

After all, the fact that the model of an object is more complex than the object itself does not correspond to state that the laws that govern the model itself have to be more complex than the object. The greater complexity can be obtained simply by an extraordinarily high number of elements of the object (of the complex), and also by interactions and feed-backs among a relatively limited number of elements which are governed by a relatively limited number of laws (or connecting rules).

We particularly care to emphasize how -on the contrary of what Weimer asserts- in our opinion these kinds of problems have not any necessary connections with the self-explanation's problem. The impossibility of the self-explanation is a merely syntactical impossibility that perhaps can be referred to the purely analytical expression: 'a (natural) number cannot be less or more than itself' (6). It has therefore not any implying relationship with the statement that says: for a degree of sufficiently high complexity a model of a phenomenon is more complex than the phenomenon itself. Leaving aside the character of a conjecture that this thesis shows, it is sufficient to note that in its logical content -as in its definition- it has the indication of a determined number (or numerical interval, perhaps) and therefore it cannot have the level of universality that is competing to the thesis of the self-explanation impossibility.

Weimer, taking after von Hayek, considers as an application of this thesis that the brain is not able to explain itself. We cannot discuss at depth here this thesis, and we will only mention it in as much as our reasoning is concerned (7). If the brain is understood as a determined system (composed of a determined neuronal connection), then von Hayek's thesis derives from the impossibility of having a self-explanation; but if von Hayek's thesis intends to have a value, by way of setting a limit to the _empirical_ research in the domain of the study of the brain, then it is very doubtful. In our opinion, there are at least two arguments in favour of this judgement. The first is very banal, but not for this reason less powerful: not every brain has equal complexity and therefore it is possible that a brain of _n_ complexity succeeds in explaining the way of working of a brain of _n-1_ complexity. (We are not interested in this instance in establishing which criterion of more or less complexity might be: one only needs to presume that one exists: it might be only a topological and not necessarily a metrical one). To say it in a slightly ironical way, this means that the understanding of the brain has no theoretical limitation except when the scientist's brain is inferior to that one of the studied subject... Secondly, even if we limit ourselves to the case of a (given) brain that is trying to explain itself, one has to bear in mind that a brain can develop itself exosomatically: for instance, it can use the memory of a computer, and interact with it in order to understand its own way of working. By creating algorithms the brain can therefore determinate the working of some systems that can be composed by elements (and their relationships) whose maximum number cannot be determined _a priori_, and therefore _can_ become a _complete_ representation of the brain itself (8).

4. The arguments that we have so far outlined are completely independent from the assumption that -as Weimer holds- there is an essential difference between the _object_ of the 'moral sciences', and that of the other sciences; in other terms they are valid also if we presuppose that this difference really exists. But can we admit that such a difference as far as the _complexity_ and the concept of _order_ are concerned really exists? Weimer uses this example about the brain. Assuming the well-known Polanyi's graphical representation of a polycentric control, Weimer says that "a more adequate representation of a cosmos such as the brain would require a three dimensional sphere with control connections going through the

interior. By the time one considers the possible interconnections of 5×10^{11} neurons the sphere is in effect solid: anything can 'control' anything else" ([9]). Probably an adequate graphic representation should not be tridimensional but _n_-dimensional (where _n_ is equal to the number of the elements). However, we have not to ask ourselves this question, but another one: is it really true that the kind of situation outlined by Weimer is such as to set up an essential separation between the 'moral sciences' and the other sciences? If we are referring to the (high) number of neurons contained in the human neocortex, the answer is immediately negative: in the 'simple' 'Keplerian' problem of the two bodies (e.g. the sun and the earth) the number of the 'elements' which interact is extraordinarily higher than the figure hereabove stated. But it is not even possible to say that the _kind_ of the interaction is in this case different: here too "anything is connected with everything, and anything 'control' anything else". The interaction between two planets, to be precise, is not an interaction between two bodies but among _n_ bodies: a 'body' interacts _simultaneously_ with all the others, and the interaction between two bodies influences and it is _influenced by_ all the other bodies and by all the others interactions.

If then we look at things as they are in one of the most 'elementary' problems of the classical physics, it does not seem possible at all to detect a qualitative distinction with respect to things as they are in the study of the brain and the society. It is easy to object to our example that physics is able to deal with the problem of the two bodies exactly because it has not to take into consideration all these interactions (e.g. it can assume the masses of the two bodies as if they were concentrated in their gravitational center): but _this is precisely the best confirmation of our thesis_. Where would then be the _logical, theoretical_ impossibility, for which in the 'moral sciences' it should not be possible to discover some methods that allow us to unify the multiplicity of the reality, in the same way as it happens in the physical sciences? ([10]). The fact that so far none has arrived to such a point -given and not admitted that this is true: we intend to refer to the results obtained in the field of artificial intelligence- has no theoretical or logical value.

We believe that only one way is open for those who hold the impossibility of a science of the society: to hold that the individuals have free willingness and that this makes impossible to search for laws (in the strong sense) under which their actions can be reconduced. Obviously it is beyond our limits to face such a problem. We only want to underline that the existence of the free willingness does not make immediately impossible the existence of sociological laws (including the economics laws) and that if the existence of the free willingness makes impossible to search for the 'covering laws' it also makes impossible the existence of any kind of explanation, including the 'explanations of the principles' (11).

5. If our remarks on the claimed impossibility of laws and explanations within the society have any value, it follows that the theses that Weimer draws from the definition of the science as a 'complex order' (e.g. about the (im)possibility of a prescriptive epistemology), must be reconsidered. We believe that the crucial point to evaluate whether Weimer's theses might agree with Bartley's positions lays here. However we prefer to face it from another point of view: if Weimer's theses were true, then the science would not have any criterion of progress -with all the relating consequences.

"The situation is similar in both society and science [...]. The rationalist, represented in society by progressivist and socialists and in science by explicit and prescriptive methodologists, wants to plan progress by putting as much as possible (ideally everything) under explicit, consciously rational control [...]. In science methodologists often refer to a single ('the') scientific method which, if only followed rigorously (i.e., unconsciously and uncritically!), will yield valid knowledge [...]. In both science and society everything is to be explicitely rational and consciously controlled. The cosmos is to be made rational by turning it into a taxis [...]. The scientific community is an abstract cosmos, bound only by general rules of conduct that are never capable of fully explicit expression" (12).

If, as Weimer thinks, these statements are conceived to refute the methodology à la Popper, we can already say that they are inadequate. It is certainly true that the methodology of the Logik may appear to offer an image of itself as consisting in a rigid set of prescriptions -a kind of a 'Baconian' method as this was conceived by most of the philosophy of the past century. But it only needs a deep analysis to show how the image of a routine to be followed is false. As Gerard Radnitzky -who more than anybody else has developed this aspect of Popper's thought- wrote: "la méthodologie [...] peut, indirectement, aider à accroître l'efficacité du chercheur. Par exemple, en offrant des moyens pour conceptualiser des situations de recherche et diverses possibilités de développement, en analysant et en appréciant critiquement les routes qu'ont suivies des entreprises de recherches réussies, en rendand explicites des présupposés tacites et des dépendances plus ou moins remarquées à l'égard de certains styles de pensée, etc. Tout cela dans le but d'accroître sa liberté de décision - et non pas pour lui prescrire comment il doit procéder" (13). However this is not the most important point. We must rather ask ourselves which are the consequences of Weimer's theses, according to which the science as the society is a 'cosmos'. Obviously they are numerous, even taking into consideration the most relevant. But there is one which is such as to make ourselves doubtful about the validity of the definition itself of science as a 'cosmos': as we have claimed before, in such a case one would not have any criterion in order to affirm the existence of a scientific progress. If the science is a spontaneous order, whose output (mainly theories) cannot be judged on the basis of criteria which are external to the scientific proceeding itself (as it would happen if one

would accept a prescriptive methodology), then one should conclude that <u>either</u> it is not possible to talk about progress, <u>or any successive</u> output would <u>anyway</u> represent a progress. But this last position encounters some difficulties which in our opinion are not surmountable. Above all it presupposes a <u>teleologism,</u> a <u>finalism</u> according to which the evolutionary process of science must necessarily lead us to a better situation.
<u>By definition</u> any further output will be better than the previous one. Even if the evolution of science should lead us to abandon Einstein's relativity in favour of Eudoxus' homocentric spheres or any <u>new</u> equivalent theory (should it only happen that the knowledge of the relativity were lost due to some accident of history) we should conclude that a scientific progress has taken place. It is obviously possible to affirm that no progress has taken place (on the contrary, that there has been a regress) because -for example- the new theory solves less problems than the previous one: but this is exactly what is forbidden by the assimilation of science to a 'cosmos', because in this way an <u>hetheronomous</u> judgement would be posed. Furthermore if the progress criteria should depend from the single steps achieved by science, we would be in a perfect <u>historical relativism</u>: the criteria would become manifold and they <u>could</u> be contradictory among themselves. Progress criteria (and hence also the criteria according to which the theories should be chosen) would only be <u>post festum</u>, and therefore science would remain without <u>purpose</u> (because what is so defined <u>a posteriori</u> is not really a purpose).

On the contrary therefore of what Weimer holds, the situation in society and in science is <u>very</u> different. In society there is not any reason why we should talk about progress independently of some given <u>moral values</u>: but if the same would be valid in science nothing and nobody would make science anything more than a mere <u>instrument</u> or <u>convention</u>; and we could never seriously affirm the existence of a scientific progress, <u>neither of progress from, neither of progress towards</u>. What is at stake then is therefore much more than the 'simple' Popperian 'constructivistic' view of science.

6. After the considerations so far put forwards, how do we have to analyze the thesis that "the only tenable unity of science is in terms of unity of methods and aims" (14)? If Weimer is right, then we believe that the thesis is almost empty, and that he is trying to find an agreement with Bartley's position while there is none.

The first consideration one has to make is that Weimer cannot legitimately talk about a single method of sciences, except as a product of an historical enquiry on the sciences themselves: but by now it should be clear that it is certainly not through the history of sciences that a method (or even a limited plurality of methods) can be individuated and defined. But the reasons of our disagreement are much more specific: having found an essential difference in the object between the 'moral sciences' and the other sciences, Weimer -volens nolens- comes to an essential difference in their method. It will suffice here to indicate two points.

The first concerns the concept of test in the 'moral sciences'. As Weimer writes, "in complex domains [...] research is more demonstration than experimentation, qualitative rather than quantitative. Instead of the experimental isolation of relevant variables empirical research in complex social phenomena consists in the construction of (or stumbling upon) situations in which we demonstrate to ourselves that we can produce patterns of 'facts' of which we are already well aware. Our demonstrations test our theoretical model only in the sense already noted -neither by justificationist confirmation nor refutation, but rather only by comparing them for consistency with our analogical knowledge of social phenomena" (15). We have to confess that we are unable to understand exactly the literal meaning of these statements. In their most likely meaning, it appears to us that they refer to a concept of 'pre-scientific experience' from which the physical sciences have emancipated themselves since the beginning of the scientific revolution; and at any rate, this concept is unable to found the validity of anything at all. Probably these very concepts could be applied to art criticism, if not even to cookery. In any case, it is evident that the way in which the term 'test' is used has nothing to share with Popper's (or Bartley's) concept of test. Moreover, a test in Popper's sense (as a comparison with reality and not only as a mere 'logical' consistency) it is made in principle impossible by Weimer himself, when -after von Hayek- he states that "explanatory theories of complex phenomena, such as evolutionary theory, transformational grammar, marginal utility and subjective value, coalitional models of neuronal organization, etc., can never predict the occurrence of particulars" (16). At most it would be possible the same kind of testing (very weak) that can be applied to the probabilistic laws; but we think that Weimer would consider also this possibility as an unjustified assimilation, as far as the explanatory theories of complex phenomena are concerned.

7. Weimer's (and von Hayek) theses are so interesting that their discussion would be really endless. But we believe that from the point of view of the problem of the unity of the sciences, one could certainly affirm that for them the very same thing that Bartley said on the distinction between Naturwissenschaften and Geisteswissenschaften holds: "the alleged distinction [...] is not part of the answer; it is part of the problem" (17).

Is it possible to unify sciences through their method? As far as I am concerned, I am unable to give an answer to this question. But even if one could demonstrate that there is a method which is shared by the different sciences, the unification could result scarcely interesting, if the same sciences were separated by some other less theoretical and more pragmatical factors. Sciences can be unified by their method: but it is de facto true that they are separated by those which in Italian are called 'metodiche', that is to say observation techniques, data processing, the use of statistical means, etc. Their importance in setting up the empirical possibility of the single sciences is such that even if one could demonstrate that there is a method by, say, 'conjectures and refutations' which is common to astronomy and botany, the distinction between the two would not be very much reduced, nor at an 'operational' nor at the theoretical level.

In this sense an enquiry could still be of a methodological type; however, 'methodology' here is something very different from the concept usually hold by the philosophers of science. The situation seems to be symmetrical: because if it is true that unity via their method does not appear to be sufficient in order to establish a satisfactory unity between sciences, it appears to be necessary: it will not be possible to consider as unified two sciences which share some given 'metodiche' without putting under the same kind of relationships the elements (observations, mathematical calculations, classifications, etc.) that constitute the 'metodiche' themselves (for example, because one science is following an inductive-cumulative method, and the other hypothetical-deductive one).

As Vincenzo Cappelletti has very well said: "il processo di unificazione della scienza è sollecitato dalla ragione e frenato dalla storia, cioè dalla ricerca, e dalla storiografia" [(18)]. Both the scientific research and historiography show the individual and specific differences under and beyond the unifying needs of reason, that in the 'theoretical' prescriptive methodology find an important expression. In agreement with Weimer, we believe that the differences shown by the single sciences in their way of proceeding have to be brought back to their own very object; but we believe that there are not any real theoretical cogent (or even only highly probable) reasons because for the sciences of those that Weimer calls "complex phenomena", it should be raised that ignorabimus that was raised by Du Bois-Raymond facing problems that appeared too complex. In our opinion this is the only position that agrees with what Bartley holds in his paper in which he has so well discussed against the Wittgensteinians' theses. From this point of view we disagree with Bartley himself when, while he refutes Hirst's thesis that rationality is essentially limited, he affirms: "this is not the Hayekian doctrine of the limits of rationality -limits with regards, that is, to prediction and explanation in the treatment of complex phenomena" [(19)]. Bartley's claim is fundamentally true: but in our opinion it is not able to grasp the fact that if von Hayek's positions (and we think that Weimer's paper is a quite faithful development of them) are true, then rationality is really essentially limited. If anything it is essentially limited because the differences between the objects to which rationality is applied limit its universality -as it does not happen instead in Popper's epistemology and methodology. Of course, in saying this, we have not solved any of the many problems that Popper's epistemology and methodology present.

FOOTNOTES

(1) William W. Bartley, III, The Division of Knowledge, 12th I.C.U.S., 1983, p. 8.

(2) Walter B. Weimer, Spontaneously Ordered Complex Phenomena and the Unity of the Moral Sciences, 12th I.C.U.S., 1983.

(3) Ibidem, p. 5.

(4) Ibidem, p. 6.

(5) "It is beyond the capacity of systems to explain or model phenomena that are more complex than the systems themselves". Ibidem.

(6) Obviously because of the definition of 'explanation'.

(7) I wrote these passages together with Dr. Riccardo Viale.

(8) As in this case we are facing the problem of explanation and not that one of prediction, our position is not at all in conflict with the classical theses of Popper (as expressed in Indeterminism in Quantum Physics and in Classical Physics, "The British Journal for the Philosophy of Science", I (1950), pp. 117-133; 173-195).

(9) W. Weimer, op. cit., p. 16.

(10) Obviously, including approximation methods. Moreover the physical and biological sciences show how it is possible to create models of very complex situations and how these modelizations (that are partial) allow us not only descriptions but also predictions. We do not yet see the theoretical reason why they cannot be applied (as in fact are applied) to the studying of the brain or society.

(11) The first statement has at least one possible justification, although we are not able to say if there are other plausible ones: it is possible to give a definition of 'free willingness' such as that is not incompatible with the predictability of the individual actions (or if it is preferred, one can give a definition backed up by such a determinism that is not incompatible with the existence of the free willingness). This is a classical thesis, which dates back to the Medieval theology. The second statement can be justified as follow: in order that an 'explanation of the principle' becomes possible (or in order that a human action could be brought back to a 'rule' that is not a 'law') it is necessary that the absolutely free action should be limited within the two extremes of the possible options

(that are in fact stated by the 'rule' we are discussing). But that the free action should be limited within certain extremes -or within a spectrum of options- it is an assumption that is contradicting the definition itself of free action. (In this case, of course, we are presupposing that our first thesis has been shown to be false). In any case it seems to us that the legitimacy of the 'explanations of the principle' would imply the legitimacy of 'covering laws' of a probabilistic type. Furthermore, if we move from the 'law' as a description of the regularity of an individual's behaviour, to the law as a description of the regularity within the social aggregates, we must underline that in assuming that the actions of the individual by which an aggregate is made up are developed within two extremes or (a set) determined of options it does not necessarily follow that the social action that will result from the interaction of the individual action will develop within the same extreme (or extremes). (The "social action" in this case is not in contrast with the methodological individualism; we could substitute in place of this word another one like 'family' or 'bank' or 'government', and our thesis will still hold for every legitimate meaning of these terms).

(12) W. Weimer, op. cit., pp. 71 and 73.

(13) G. Radnitzky, Méthodologie poppérienne et recherche scientifique, "Archives de philosophie", XLII (1979), pp. 3-39; 295-325; p. 6.

(14) W. Weimer, op. cit., p. 1.

(15) Ibidem, pp. 9-10. The Italics are ours.

(16) Ibidem, p. 63.

(17) W. Bartley, op. cit., p. 4.

(18) V. Cappelletti, Unità e storia della scienza, 12th I.C.U.S., 1983, p. 22.

(19) W. Bartley, op. cit., p. 16.