Weinert's Review of "The Comprehensibility of the Universe"

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Friedel Weinert has written a long and detailed review of my The Comprehensibility of the Universe (Oxford University Press, 1998): see this Journal, vol. 75, 2000, pp. 296-309. I am pleased that Weinert should, in his review, enthusiastically affirm the desirability of bringing philosophy of science and science together, to recreate natural philosophy.

However, during the course of his review-article, Weinert makes a number of misleading remarks about the contents of my book - or so it seems to me. In this note I do what I can to put the record straight. What follows will seem excessively critical; but this is because I concentrate on where, in my view, Weinert goes wrong, and ignore what he gets right.

In my book I argue for a conception of science that I call aim-oriented empiricism (AOE). This construes science as adopting, as a part of scientific knowledge, a hierarchy of cosmological assumptions about the comprehensibility and knowability of the universe, these assumptions asserting less and less about the universe as one ascends the hierarchy, thus being more and more likely to be true. Corresponding to these cosmological assumptions there are methodological rules which govern acceptance of assumptions lower down in the hierarchy, and which, together with empirical considerations, govern acceptance and rejection of scientific theories. The top two assumptions, at levels 10 and 9, are such that accepting these assumptions as a part of scientific knowledge can only aid, and can never damage science (or the task of acquiring knowledge more generally) whatever the universe may be like. These are justifiably permanent items of scientific knowledge. As we descend, from level 8 to level 3, the corresponding theses make increasingly substantial assertions about the nature of the universe: it becomes increasingly likely that these theses are false. At each level, from 8 to 3, we adopt that assumption which (a) is compatible with the assumption above it in the hierarchy (in so far as this is possible), and (b) holds out the greatest hope for the growth of empirical knowledge, and seems best to support the growth of such knowledge, at level 1 (evidence) and level 2 (testable theory).

Here, then, are some of Weinert's comments which do not, in my view, do justice to AOE.

1. One of Weinert's more serious criticisms is that I deny that there is ever discontinuity, at the level of theory, in the evolution of physics, there being no such thing as Kuhnian revolutions. Weinert refutes this by giving examples of discontinuous theoretical revolutions (see p. 304 of his review). In fact nowhere do I reject discontinuity at the level of physical theory. I do, it is true, at one point assert "all new ideas in physics and mathematics are never utterly new (or they would be meaningless). They invariably arise as modifications of pre-existing ideas (in the kind of way indicated in Sections 3 and 4 of Chapter 3); or they arise as a result of making explicit what has been implicit in what has gone before" (p. 121 of my

book). But I am here arguing against incommensurability, not discontinuity. The kind of modifications of pre-existing ideas, that I discuss in chapter 3, enable one to introduce radically new theories, that mark extreme discontinuity with earlier theories. But much more important than any of this, I make clear elsewhere in my book (for example, pp. 180-1), that AOE enables us to appreciate how there can be continuity through a revolution at level 4 or 5 or above, and at the same time extreme discontinuity at levels 2, 3 and even 4. In the case of the revolution brought about by Galileo, we must go right up to level 5 (in the hierarchy of assumptions) before we find continuity. One of the great strengths of AOE is that it can do justice to the idea that, through a scientific revolution, there is continuity (at level 4 or above) and at the same time discontinuity (at level 4 or below). Weinert is quite wrong in attributing to me the view that there is continuity at the level of theory (level 2).

2. Weinert is critical of my account of simplicity (unity or explanatory power). My solution, he says (on p. 300) "is achieved at an expense of complexity, which would not pass the test of Occam's razor. It contends that the representation of scientific knowledge involves a hierarchy of ten metaphysical assumptions concerning the comprehensibility of the universe." And elsewhere (p. 305) he asserts "In reality, Maxwell's assumptions do not get 'simpler' and this is just what is required if they are to act as constraints in the construction of new theories." This ignores that there are two distinct problems of simplicity: (1) the problem of what the simplicity (unity, explanatory power) of a theory is, and (2) the problem of how persistent preference for simple theories in science is to be justified. My solution to (1) involves no assumption higher than level 4. The solution is, in a sentence, that the more the content of the totality of fundamental physical theory, T, accords with, or exemplifies the level 4 assumption of physicalism, so the greater is the simplicity of T. Assumptions further up the hierarchy are not involved. As far as my solution to (2) is concerned, the whole hierarchy of assumptions is involved; but here, it is essential that assumptions above level 5 allow for the possibility that the universe is not 'simple', being made up, perhaps, of many different kinds of physical entity interacting by means of different forces. Only in this case is it possible for science (proceeding in accordance with AOE) to discover that the universe is not simple, which, for all we know for certain, may be the case. Weinert's criticisms are not valid because they fail to note the distinction between (1) and (2). (I might add, in response to Weinert's Occam's razor remark, that the assumption at level 4 implies assumptions above; the hierarchy is not as Baroque as it may seem at first sight.)

3. Elsewhere (p. 303), Weinert points out correctly that my solution to the problem of simplicity (problem (1)) depends on distinguishing sharply between the content and the form of a theory. But he then goes on to say "The detailed discussions of theoretical unification in physics ... depends essentially on a close scrutiny of the equations ... It is the form of the equations, which reveals the process of unification" (pp. 303-4). But here Weinert fails to appreciate the distinction between using the physically interpreted equations of a theory to point to a possible dynamic structure, the content of the equations being what is at issue, and merely exhibiting the form of the equations. It is the former that I do in my book. I use physically interpreted equations to discuss possible dynamic structures. Weinert is simply wrong when he says "form and content are not separated in this discussion" (p. 304). Weinert is also wrong when he says that, in my discussion of unification in the history of physics I fall "back on the 'unification-as derivability-idea' which ... [Maxwell] had earlier criticized" (p. 303).

4. Weinert declares that I call the assumptions, in the hierarchy of AOE, untestable; but this, he goes on, "conflicts with some of ... [Maxwell's] own examples and scientific practice. The

mechanistic worldview of the seventeenth century arose from the scientific discoveries of the founding fathers of modern science. In the twentieth century both quantum mechanics and relativity theory led to fundamental conceptual revisions of the basic epistemological tools, which had lain at the root of the mechanistic worldview" (p. 301). There are two things wrong with this. First, the fact that the mechanistic worldview clashes with quantum <u>theory</u> does not mean that the mechanistic worldview is empirically <u>testable</u>. Clashing with <u>theory</u> is not the same thing as clashing with <u>evidence</u>. (And even if the mechanistic worldview did arise from scientific discoveries, this does not make it testable either.) Second, if Weinert had read my book more carefully, he would have noted that I do <u>not</u> hold that the metaphysical assumptions, at levels 3 and above, are necessarily untestable. On p. 271 I say that my "notion of 'metaphysical' does not draw a sharp line between the metaphysical and the testably scientific. The 'metaphysical' doctrine that there are only repulsive forces in the world [a key component of the mechanistic worldview] is refuted by the observation that attractive and cohesive forces do exist: the 'metaphysical', as understood here, cannot be equated with the 'unfalsifiable' in a Popperian fashion".

5. At one point Weinert asserts: "Throughout the book Maxwell vacillates considerably about how far the empirical evidence reaches up into the realm of the cosmological assumptions" (p. 300). I am baffled by this assertion. I see no such vacillation in my book. What I stress is that the higher up revisions take place in the hierarchy of assumptions, so the more drastic and convulsive would be the revolution in science, or in knowledge. In chapter 5 of the book, in particular, there is a careful and detailed discussion of the circumstances in which assumptions, at various levels, up to level 9, might be revised in the light of the empirical success and failure of competing research programmes.

6. Weinert attributes to me the view that "The assumptions [of AOE} must become part and parcel of scientific theories" (p. 298). But as I make clear from the outset (see, for example, p. 12) accepted physical theories <u>clash</u> with the level 4 thesis of physicalism. Only when a unified 'theory of everything' has been discovered will the level 4 thesis of physicalism be 'part and parcel' of a physical theory.

7. In my book I criticize a doctrine that I call <u>standard empiricism</u> (SE). This asserts that scientific theories are chosen on the basis of evidence alone, no permanent assumption being made about the universe independent of evidence (so that if preference is given to simple theories, this does not mean that science assumes permanently that the universe itself is simple). Weinert attributes to me the view that this is "one of the influential philosophical schools of this century" (p. 297), and goes on to say "Maxwell recognizes of course that SE has undergone considerable changes from Popper's naive falsificationism, in which a single conjecture faces the court of evidence, to Lakatos's sophisticated falsificationism, in which rival research programmes compete in progressive problem solving. But once this distinction has been acknowledged in the early part of the book, it is quickly forgotten. The later criticisms are launched against the SE <u>tout court</u>, sometimes leaving the reader in some doubt as to whether a straw man is under attack" (p. 298). But this distorts my argument. SE is not a "school of philosophy" at all; it has not "undergone considerable changes" in the way Weinert indicates. It is rather a quite specific <u>thesis</u> about science, formulated briefly above, which happens to be a common component of a range of well known and influential views about science, which include: logical positivism, inductivism, logical empiricism, hypothetico-deductivism, falsificationism, conventionalism, constructive empiricism, pragmatism, realism, induction-to-the-best-explanationism, the views of Kuhn and Lakatos. What the thesis of SE is remains constant throughout the book: it does not change, or revert

to something cruder, as the book progresses. Nor is it true to say that Lakatos's contribution is forgotten. I make clear that Lakatos's notion of "research programme" is an essential ingredient of AOE (see for example ch. 5, pp. 176-183; and p. 273).

8. Weinert attributes to me the view that SE exercised an "iron grip" over natural philosophy, even in the 17th and 18th centuries. He then goes on to point out that Newton, Leibniz, Boyle and others, in violation of SE, explicitly discussed metaphysical assumptions and used them "as constraints .. in the conceptions of new theories" (p. 303). But I make clear that SE only became the official view of the scientific community much later. For example, I remark "Newton ... may be interpreted as having defended an early version of aim-oriented empiricism. This is apparent in Newton's First Rule of Reasoning, which asserts: 'We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve: for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.'" Elsewhere I point out that the distinction "between AOE and SE corresponds, very roughly, to the distinction between traditional rationalism and empiricism" (p. 20). Thus SE only became established as the official doctrine about the nature of science after the death of rationalism, some time after Kant (or perhaps only after Einstein): see pp. 19-21 and 41-2.

9. Weinert also remarks that even in the 20th century physicists such as Heisenberg and Bohr engaged in "intense philosophical discussion" about such things as "the notions of determinism and causality as a direct consequence of new empirical discoveries" associated with quantum theory. Weinert goes on "If SE had held such sway over scientists as Maxwell claims, these discussion would hardly have been possible" (p. 300). Fair enough. One implication of SE, namely that untestable ideas should be excluded from science, was not observed during the turmoil of the quantum revolution in the first three decades or so of the last century. But it is worth bearing in mind that Bohr, Heisenberg, Born and others were arguing, in effect, for an extreme version of SE, namely for a kind of instrumentalism or positivism that insists that theoretical physics should abandon the "attempt conceptually to grasp reality as it is thought independently of its being observed" (Einstein). And the temporary victory of Bohr's camp over Einstein's had the consequence that critical discussion of quantum theory, and critical discussion of metaphysical and philosophical ideas related to physics, was largely excluded from physics journals during the 1950's and 1960's (although, if one was a Nobel prize winning physicist one might be able to overcome this exclusion).

Weinert goes on to remark that one may doubt whether Einstein "was an early proponent of AOE" (p. 300). I entirely agree. In connection with quantum theory, Einstein was fundamentally arguing for scientific realism; his arguments are not for AOE; nor do they presuppose AOE. (For my own extended discussion of Einstein's highly ambivalent attitude towards elements of AOE, see my paper in the Brit. J. Phil. Sci. 44, 1993, pp. 275-305, referred to several times in my book.)

10. In connection with my advocacy of "conjectural essentialism" Weinert remarks "The Necessitarianism of the Australian school is an approach to the question of laws of nature, which has been criticized for its reliance on modalities. All these approaches lack what John Earman once described as 'the texture and feel of real-life laws'" (p. 305). But conjectural essentialism owes nothing to the "Necessitarianism of the Australian school" - by which, I take it, Weinert means Armstrong's views, published in 1978 and 1983. As I make clear in my book, conjectural essentialism is a restatement of a different view, published by me ten years earlier (Brit. J. Phil. Sci. 19, 1968, pp. 1-25). Criticisms that may well be valid when

directed against Armstrong's views, are not valid when directed against my earlier position. In particular, Earman's comment does not apply. Conjectural essentialism simply <u>reinterprets</u> existing physical laws and theories so that they are interpreted as attributing dynamic properties to entities, instead of merely specifying regularities that entities observe. Theories are just as empirical and factual as when given more orthodox interpretations; for essentialistically interpreted theories assert that such and such entities <u>exist</u>, with such and such <u>properties</u> (factual and empirical assertions).

11. Finally, Weinert gets a number of things wrong in criticizing my "propensiton" version of quantum theory, outlined in the last chapter of <u>The Comprehensibility of the Universe</u>, and elsewhere. He asserts, for example, that it cannot do justice to the particle aspect of quantum phenomena, and does not provide the means for localization of quantum systems. But neither of these charges is correct, although I cannot pursue these rather technical matters concerning quantum theory here.