Reply

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The critique[1] by Dr. Wallau of my article[2] raises a number of interesting points. In the interest of brevity, I have chosen to respond here only to those of major contention. These are:

1) "As a philosophy Popper's logic of science is not to be understood as a concrete guideline, but serves as all sciences as a description of the "essence of the world".
2) The falsification criterion is contrary to the legend, by which Berson was taken in, not the fundamental characteristic (criterion of meaning) of science in Popper's mind, but serves merely to its demarcation from pseudoscience.
3) A *fundamental* prohibition of the ad-hoc rescue of falsified theories, as it is stated by Berson, belongs also to the sphere of the legend and not to the structure of Popper's thoughts."

These opinions of Dr. Wallau cannot be reconciled with numerous statements in Popper's writings. That Popper intended his logical deductions to be the basis of a scientific methodology, a set of guidelines to action, is widely accepted among philosophers of science and is discussed in many writings on the methodology of science.[3–10] Moreover, it also is clear in passages including among others the following from Popper's *Conjectures and Refutations*:[11]

> p. 27: "…if you are interested in the problem which I tried to solve by my tentative assertion, you may help me by criticizing it as severely as you can; if you can design some experimental test which you think might refute my assertion, I shall gladly, and to the best of my powers, help you to refute it."

> p. 31: "…the work of the scientist consists in putting forward and testing theories."

> p. 42: "According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival."

> p. 50: "…I shall try to establish the rules, or if you will the norms, by which the scientist is guided when he is engaged in research or in discovery."

> p. 53: "Methodological rules are here regarded as *conventions*. They might be described as the rules of the game of empirical science…"

> p. 54, in a section entitled *A Theory of Method*: "Just as chess might be defined by the rules proper to it, so empirical science may be defined by means of its methodological rules. In establishing these rules we may proceed systematically. First a supreme rule is laid down which serves as a kind of norm for deciding upon the remaining rules, and which is thus a rule of a higher type. It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification. Methodological rules are thus closely connected both with other methodological rules and with our criterion of demarcation. But the connection is not a strictly deductive or logical one. It results, rather, from the fact that the rules are constructed with the aim of ensuring the applicability of our criterion of demarcation; thus their formulation and acceptance proceeds according to a practical rule of a higher type."

In other words, the falsification criterion of demarcation is not identical to a complete methodological set of rules, but it acts as the "supreme rule" from which the other procedural rules are derived as corollaries. The rule prohibiting ad-hoc rescues without enhanced vulnerability is a corollary which Popper himself derived from his supreme rule of falsification.

Another indication that Popper's logic was intended to lead the working scientist becomes clear when we remember that he, like most other philosophers of science, uses historical case studies in support of his procedural recommendations. The argument for case studies is set out, for example, by Hesse,[12] who proposes that not only should philosophers use historical cases in the development and refinement of their methodology, but also that scientists should give attention to methodological considerations in their work:

> "Firstly, a logic of science … should supplement mere description with *normative considerations* [emphasis supplied]. This is because it presupposes that there are norms or criteria of 'good science' in terms of which scientists judge scientific theories, and that these have some elements, perhaps tacit, of internal logical coherence and rationality … [ultimately] the relation of logic and [historical] cases will be one of mutual comparison and correction."

Do philosophers intend their case studies to be historical analyses leading to philosophical conclusions, or normative recommendations specifying *actions* for scientists to carry out? From the very nature of the enterprise of philosophy of science, I think the answer must be "both". If the growth and

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success of science in the past can be analyzed according to some philosophical system, this analysis amounts to a recommendation to scientists that they adhere to the same historically proven kinds of procedure in the future. We find a similar conclusion in a passage from Kuhn.[5] In response to Feyerabend's question[13] whether we should read Kuhn's remarks about scientific development as *de*scriptions or *pre*scriptions, Kuhn writes:

> "The answer, of course, is that they should be read both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish."

Dr. Wallau also objects to my previous discussion[2] of Popper's criterion in *The Logic of Scientific Discovery* (p. 80 ff.),[14] which requires that an ad-hoc rescuing hypothesis must predict something new, an event not predicted by the original jeopardized theory. I showed that Kekulé's rescuing collision hypothesis did not meet this criterion. Dr. Wallau now states that: "the application of Kekulé's theory in the 19th century … shows that the scrutiny of its prediction was possible with the methods of the 19th century and that it therefore indeed was undertaken on a broad range of falsification experiments".

This formulation precisely misses the point. The experiments to which Dr. Wallau refers were of a type that were designed or could be interpreted to test the idea that all C–C bonds of benzene were equivalent. But the *original* Kekulé theory of 1865 *already embodied* this idea. The *rescuing collision hypothesis* of 1872, proposed only to explain C–C bond uniformity in benzene, did not predict anything further than did the original theory. So all the tests of the 19th century and for decades after were tests of the *original* 1865 theory and could not have falsified the collision hypothesis. Actually, in practical terms, one may question whether the collision hypothesis has been falsified even today, except in the broadest sense. It is true that it has been superseded by quantum mechanics, which by general agreement offers a much more satisfactory account of chemical bonding and many other phenomena. Thus, the emergence of quantum mechanics can be said to provide a good reason for rejecting the collision hypothesis, but I know of no single experimental test that has disproved the latter.

Dr. Wallau[1] believes that Popper did not intend his falsification scheme and its corollaries to provide guidelines or rules for action by scientists. I conclude that this does not agree with many statements to the contrary by Popper himself, nor is it compatible with philosophical practice in the development of scientific methodology. The assertion[1] that Kekulé's collision hypothesis did not violate Popper's prohibition regarding ad-hoc rescuing hypotheses conflicts with historical fact.

[1] W. M. Wallau, *Angew. Chem.* **2001**, *113*, 1241; *Angew. Chem. Int. Ed.* **2001**, *40*, 1201.

[2] J. A. Berson, *Angew. Chem.* **2000**, *112*, 3173; *Angew. Chem. Int. Ed.* **2000**, *39*, 3045.

[3] S. Toulmin, *The Philosophy of Science: An Introduction*, Harper and Row, New York, **1953**, p. 54, 201. Originally published by Hutchinson, London.

[4] J. J. C. Smart, *Between Science and Philosophy: An Introduction to the Philosophy of Science*, Random House, New York, **1968**, p. 198.

[5] T. S. Kuhn in *Criticism and the Growth of Knowledge* (Eds.: I. Lakatos, A. Musgrave), Cambridge University Press, Cambridge, UK, **1999**, p. 231.

[6] "Falsification and the Methodology of Scientific Research Programmes": I. Lakatos in *Criticism and the Growth of Knowledge* (Eds.: I. Lakatos, A. Musgrave), Cambridge University Press, Cambridge, England, **1999**, p. 91.

[7] C. G. Hempel, *Philosophy of Natural Science*, Prentice-Hall, Englewood Cliffs, NJ, **1966**, pp. 44–45.

[8] B. Gower, *Scientific Method: An Historical and Philosophical Introduction*, Routledge, London, **1997**, p. 50.

[9] P. Feyerabend, in *Against Method: An Outline of an Anarchistic Theory of Knowledge*, New Left Books, New York, **1975**. Reprinted as *Against Method*, Verso, **1997**, from the third edition of 1993.

[10] M. Fehér, *Changing Tools: Case Studies in the History of Scientific Methodology*, Akadémia Kiadó, Budapest, **1995**, p. 56.

[11] K. R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, Routledge and Kegan Paul, London, **1963**. Reprinted from the revised fifth edition of 1989 by Routledge, 1996.

[12] M. Hesse, *The Structure of Scientific Inference*, University of California Press, Berkeley, CA, **1974**, p. 6.

[13] P. Feyerabend in *Criticism and the Growth of Science* (Eds.: I. Lakatos, A. Musgrave), Cambridge University Press, Cambridge, UK, **1999**, p. 198.

[14] K. R. Popper, *The Logic of Scientific Discovery*, Routledge, London, **1959**, translated by K. R. Popper, J. Freed, L. Freed. From *Logik der Forschung*, the original German ediition of 1934 (reprinted 1992).

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