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DISCUSSION REVIEW

THE INTERFACE OF PHILOSOPHY AND PHYSICS[•]

JOSEPH AGASSI Boston University

These two volumes,[♣] edited by Mario Bunge – one of the few avant-gardists in both physics and philosophy – contain both stimulating and frustrating material. Some of the writers knowingly exert pressure on their readers (Clifford Truesdell); others flatter them by assuming that they know so much. The volumes are packed with material, and it takes a long time just to read them, let alone understand them. Had I known of anyone competent to understand these two volumes in all their aspects, I would have asked the editor to relieve me of my undertaking to review them. I could not read, for example, Jean Pierre Vigier, but merely gaze with astonishment at his few pages in which he geometrizes Lee algebras to get particles as warps in an Einsteinian universe of sorts, aiming to unify elementary particle theory, nuclear theory, etc., into one system – unfortunately with four spatial dimensions. This is not philosophy in any way, but straight high-powered speculative physics - which might, who knows, change everything, including philosophy.

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[♣] Mario Bunge, editor, Delaware Seminar *In the Foundations of Physics*, and *Quantum Theory and Reality*, Vols. I and II of *Studies in the Foundations of Methodology and Philosophy of Science*, Springer-Verlag, New York, 1967, pp. 193 and 117, respectively.

Readers obsessed with keeping up-to-date are advised, for the sake of their own sanity, to keep away from these two volumes. The volumes are not fully up-to-date, yet they are advanced enough to depress one no end, especially the contributions by writers who think it their duty to perform an abominable snow-job, to use Abner Shimony's phrase. Also the pressure to keep up-to-date spoils the fun and the feel for significance. Unless there is genuine interest in the problems of the field – the philosophy of physics or any other – the use for it is questionable. And a few highly sophisticated arguments, including some in this volume, could be simplified with benefit, both technical and intellectual, were the problems better put. For example, this volume contains various readings of the uncertainty relations formula, and even of the uncertainty principle which is the interpretation of this formula. In a review it is impossible to contrast these because the job of translating them to a common system is already too taxing. Another example is Bunge's axiomatization of quantum theory, which, I hope, will be closely examined by the more expert.

Let me present one problem, which is presented in almost each essay in the second volume under review and in at least two of the first. Is contemporary physics – quantum theory and/or relativity – committed to philosophical subjectivism? It seems to me that we are not yet prepared to discuss this question. To make the discussion effective we may first have to discuss the import or the implication of this question. Let us concede, first, that quantum theory and/or relativity is intrinsically subjectivist. Is thereby subjectivism objectively proven? Is there not at least the choice between objectivism and subjectivism? Suppose that giving up subjectivism amounts to the loss of quantum theory, perhaps of science itself. What exactly is the loss incurred.

Did Einstein, for instance, lose quantum theory? Did Bohm? Anyway, let us also concede that there is a loss. We have lost Newtonianism (in the sense in which there is a loss), and perhaps we should lose more, hoping to make room thus for more.

The question is, what is the question? What question quantum theory answers, or what quest does it satisfy? Those who want predictions will predict and will not read any of Bunge's volumes; by the time these volumes will lead to prediction, if ever, they will be obsolete and out of sight; those concerned with prediction care neither for philosophy nor for history. Those who want to find out about the nature of things do not worry whether quantum theory shakes objectivism, but naturally try to read that theory objectively. They wish to succeed; but whatever they lose if they fail, it is not objectivism.

Those who still take seriously the uncertainty principle, the principle of complementarity, and von Neumann’s proof, may benefit from studying the history of these ideas. Unfortunately, the present volumes are more up-to-date than historical. There are hardly more than hints to the effect that the classic works of Heisenberg, Bohr, and von Neumann are out-of-date. Yet the volumes are highly critical of these writers. The Heisenberg microscope is ruthlessly destroyed by Henry Margenau and Leon Cohen; the natural width of a spectral line is taken as an obvious out-of-date idea by Edwin T. Jaynes; von Neumann’s alleged proof is declared to be nothing but a redundant postulate by Margenau and James L. Park and by Bunge. Henry Mehlberg is jovial in his hilarity - were von Neumann’s place in history not so secure, I doubt that Mehlberg would feel so free to be so critical. As to Bohr, he gets a very rough handling from his great admirer Sir Karl Popper;

I dare say this will disturb quite a number of physicists - and perhaps in a beneficial manner. For my part I find Sir Karl's argument quite satisfactory yet far from solving the central problem. It is not a solution, as Sir Karl may think, but a call for the restatement of the problems at hand (except for the problem of the reduction of the wave packet: I cannot see how anyone can deny that Popper has fully solved it).

The peculiarity of quantum probabilities lies in the fact that ? interferes but is no probability measure, whereas ??|^2 is a probability measure which does not interfere. This led to various formulations of the problem, the best of which is that of Reichenbach: it follows from the previous sentence that the shutting or opening of one slit in the two-slit experiment affects at a distance the transition probability of interaction between the other slit and a passing particle. Sir Karl employs his by now well-known propensity interpretation of probability to show that the same holds for a standard pin-board experiment. But the transition probabilities in all small neighborhoods on a pin-board suffice to describe the distribution of balls at the bottom of the board, and do not do so in quantum experiments. Popper is right in claiming that propensities (transition-probabilities) are dependent on setups as wholes; the question still is of action-at-a-distance in the sense that we may wish to break up the ? field into smaller "rays" – this is how I read a very interesting suggestion by Ralph Schiller who combines the Jeffreys-Wenzel-Kramers-Brillouin approximation method with Bohm's sub-fields.

I must mention at least that the first volume includes some excellent discussions of thermodynamics, especially by Jaynes and by Harold Grad, and of relativity, by E. J. Post and by Peter Havas. Space allows me to make only passing comments. When a question arises concerning testing - what theory exactly is testable, and how - philosophers toss the problem to physicists and *vice versa*. Surely, here an interesting and useful part of the interface may be studied. This is only an example of a more general situation: the more precise a study is in a relevant manner, the more philosophical it seems - perhaps because the broad relevance of a few hand-picked small logical points creates its own peculiar problems. Also such precision seldom settles problems: enlightening as it should be, it often raises more problems than it settles. Finally, educationally, stressing problems is highly useful. It is much of a consolation and an encouragement to the beginner and the uninitiated to note that not all of their difficulties are due to their own deficiencies alone. It is time physics textbook writers got sensitized to such things as discouragement and so on, at least they may greatly increase their efficiency from reading these two volumes.