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[Although my own field...]

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Tekst jest udostępniony do wykorzystania w ramach dozwolonego użytku.



I. Szumilewicz

Although my own field is not history of science but philosophy of science, I am, of course, extremely interested in the history of science, because I do believe that a historical approach to the philosophy of science is a very good one. My impression from our meeting is that the historians of science need to cooperate and so do the philosophers of science. And I believe that such a cooperation may be very useful for both sides. There is of course nothing new in the idea that the problem of classification and periodization of science is a very difficult and complex one. There are many causes of it. Let me list some of them.

1) We should choose a set of methodological postulates, but such postulates are based on the acceptance of some theories and philosophical conceptions, or are calling to the common sense meanings. Our choice of the set of postulates has therefore some arbitrary elements. We may choose however a metapostulate so that the set of postulates should give us the picture of the objective trend and development of science. But such a basic metapostulate contains, of course, some arbitrary elements, too.

2) We should have a theory of definition, and a definition of what is science, what is its subject, and what methods it is using. All that is, we know very well, a very complex problem, connected of course with the whole body of social activity of men.

3) We should choose a set of criteria of development, and the possibility of making a choice is here of great diversity. This may be, for example, a criterion in science itself, or in its external relations to other branches of human activity. To quote an example from the first one: we may say that the sign of a new period of science is the differentiation and change of some fundamental notions, for example, the bifurcation of the notion of mass into inertial mass and gravitational mass, and the evolution of time and space concepts; this is the criterion of the great turn from Newton's to Einstein's physics. But within the problem arises the question of how many of the terms should be changed to decide that a new period has begun, and, of course, a host of other questions. A good example of a criterion based on external relations is

the influence of one science upon other sciences or branches of culture and social activity, for instance, the influence of the second law of thermodynamics and of the notion of entropy upon chemistry, biology and philosophy.

4) It has to be shown how the group of problems works. Of course, we should stress that when making a choice of one set of methodological postulates, definitions of science, criteria of development, we do choose a particular frame, and there exists a host of various frames. And I doubt whether the laws and regularities of (for example) development of science are going to be invariant to observers in various frames. We should remember this when introducing a particular observation frame.

Let us now examine Professor Kedrov's papers on periodization and classification from this general point of view. It is possible to make some remarks.

1) We do not know from the paper which definition of science does Professor Kedrov accept.

2) The development is defined as the way from simple to more complicated subjects and laws. But then a problem arises — what the term "simple" means. The principle of simplicity, to use a paradoxical phrase, is a very complex one. What precisely the term "simple" means, what is the definition of simplicity? There are various concepts of simplicity. For example, Professor Popper claims that what is intuitively the simplest of various alternative hypotheses is also always the most falsifiable one. Another point of view, commonly accepted in the intellectual community, is that "simplicity" means not to multiply unnecessarily the entities admitted into one's theories. There exist many aspects of "simple"; in which of them this term is used by Professor Kedrov? In a logical, epistemological, ontological, pragmatological one, etc.? Besides Professor Kedrov claims that the development of science is going from "simple" to "complicated". But there exist some very serious arguments that the trend of the development of science is going in an opposite direction, from complicated to simple. And some of the philosophers of science claim that it is a sign of the underlying simple structure of nature itself.

3) Professor Kedrov proposes to divide the development of science into five parts: A, B, C, and two periods in between A and B, and B and C. The part A, if I understand correctly, is an observational part. But it should be clarified what observation means. Pure observation does not exist. Solitary pure facts are a myth that has been rejected by the philosophy of science. The period B is analytical and C — synthetical. Analysis, according to the author, means to divide the whole body of science into particular branches, and C is a period of integrating them. But the processes of analysis and synthesis are still going on. The old

branches of science are undergoing a process of bifurcation, but we do observe a process of integration too, some branches of science being joined in one, as for example in the case of cybernetics.

Professor Kedrov claims besides that the contemporary science is undergoing a process of integrating, because we observe that in between two sciences as for example physics and chemistry there appears a new branch — physical chemistry. But the process of integrating is, I think, a very specific one, as no individual is able to have an outlook on the whole body of science. Such an outlook is like a platonic idea — it exists for the mankind as a whole, but an individual is today further from catching it, than ever in the history of mankind. The development of science is very, very rapid; a physicist, for example, cannot grasp the whole of physics. (Professor Oppenheimer quoted as an example that he had taken part in a symposium of physics when the fundamental paper was understood only by very few participants of the meeting. An integrating outlook on the whole body of science is therefore for an individual of our time a platonic idea.

I suppose that these remarks are an example of the usefulness of collaboration between historians and philosophers of science. Prof. Kedrov's paper seems to me essentially right, but it would be necessary to clarify such concepts as simplicity, analysis and synthesis and the understanding of the logic of choosing the explanatory postulates.

E. Rosen

According to Professor Olszewski, periods in political history are sharply defined, whereas periods in the history of science and technology are not sharply defined. But in political history, some periods are not sharply defined, for instance, the fall of the Roman Empire. And in the history of observational astronomy, we know the year and the day, and very nearly the minute, when the period of telescopic observations began. The same may be said also for the beginning of the period of radio astronomy.

A. A. Zvorykine

Professor Olszewski's lecture attracts the attention of all the historians of science and technology as both from the theoretical and practical point of view every scientist — when preparing works concerning the history of science and of technology — ought to resolve in