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LUDWICK FLECK'S EPISTEMOLOGICAL CONCEPTION AND CONTEMPORARY DISCUSSION ON THE NATURE AND DYNAMICS OF SCIENTIFIC KNOWLEDGE

The name Ludwik Fleck (1896—1961), the Polish microbiologist, had until recently been hardly mentioned in the philosophical methodological literature. His works on epistemological problems, written in the middle of the 1930s, had been thoroughly forgotten or known only to a narrow circle of specialists. He again became a talking point after T. Kuhn in his *Structure of Scientific Revolutions* described Fleck' s book [1] as being one of the works which had the greatest influence on the formation and development of his own concept of scientific revolution [2]. Due to the wide publicity of Kuhn's book, Fleck's philosophical works again attracted the attention of researchers. In recent years this interest has increased. References to L. Fleck are becoming common in methodological publications. Almost simultaneously, an English translation of L. Fleck's book, edited by T. Trenn and R. Merton and with Kuhn's preface, appeared in the United States and its second edition was published in Germany by the efforts of L. Schäfer, professor of Hamburg University, and T. Schnelle who headed the organization of an international colloquium 'Ludwik Fleck' held in 1981 in Hamburg (FRG).

It would be wrong, of course, to explain the revived interest in L. Fleck's ideas and his personality only as a response to present-day discussions on Kuhn's conception of science. At the same time, it is beyond doubt that an understanding of the essence of these discussions enables us to define the independent significance of L. Fleck's methodological and epistemological ideas and, consequently, to understand better

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[1] L. Fleck: *Enstehung und Entwicklung einer Wissenschaftliche Tatsache. Einfuhrung in die Lehre vom Denkstil und Denkkollektiv.* Basel 1935.

[2] T. Kuhn: *Structure of Scientific Revolutions.* Chicago 1962.

the evolution of views which in the 1960s found expression in the anti-positivistic counter-reform in the philosophy of science.

The very title of L. Fleck's main philosophical work looks like a challenge to neo-positivism, prevailing in the philosophy of science in the middle of the 1930s. The fact, the unshakable foundation of science, the last instance of the truth of scientific theories, as the leaders of the Vienna circle believed for instance, was in L. Fleck's view a historically conditioned cognitive process developing in time. According to this view, which in the 1970s became nearly a keynote of epistemological discussions in the post-positivistic literature, to consider time was a daring heresy or not quite harmless whim. It is significant that, to explain his conception of the fact, Fleck turns not to logical reconstruction of linguistic phenomena of science, but to the history of science, consciously trying to bring the latter into a dramatic conflict with the methodological principles of neo-positivism. In this way, more than a quarter of a century before Kuhn and S. Toulmin, he tried to apply in practice a departure from logic to the history of science, which was to become the most debatable and essential aspect in the views of the historical school in the philosophy of science in the 1960s and 1970s.

L. Fleck's monograph is conceived as an examination of the history of syphilis and discovery of the Wassermann reaction, accompanied by epistemological comments. The discovery of the serodiagnostic reaction to syphilis discovered in 1906 by the outstanding German bacteriologist August Wassermann and his colleagues Albert Neisser and Karl Bruck was a most interesting and instructive episode in world science. After Fritz Schaudinn and Erich Hoffman discovered *Spirochaeta pallida*, the syphilis agent, in 1905 and Jules Bordet and Octave Gengou developed the diagnostic method permitting to define the nature of antibodies contained in a serum by a known antigen and, conversely, to define an unknown antigen by a known antibody [3]. Wassermann proposed using this method for serological diagnosis of syphilis, being confident that the

[3] J. Bordet's and O. Gengou's method, called otherwise complement fixation is based on the hemolysis phenomenon, i.e. the ability of the serum of animal immunised by heterologous erythrocytes to dissolve these erythrocytes owing to the formation of special 'antibodies', hemolysins, in its blood. Bordet and Gengou discovered that two types of substances are present in such serum: heat-stable hemolytic amboceptor and a complement easily destructible on heating and acting as a link between the amboceptor and erythrocites in the absence of which hemolysis does not develop. According to Bordet's and Gengou's law, the antigen and the respective antibody fix the complement; the absence of hemolysis indicates the existence of such a link, i.e. the presence in a given bacteriological system of an antigen corresponding to the given antibody; the start of hemolysis testifies that the antigen and the antibody in a bacteriological system do not correspond to each other and do not therefore fix the complement. *Bolshaya Medicinskaya Enciklopedya*, 2nd ed., Vol. 4., Moskva 1958, p. 1016—1019.

specific anti-spirochaetal antibodies must be present in the blood and extracts from organs of a syphilis afflicted person. Having proved to be an exceptionally succesful diagnostic test, the Wassermann reaction gained universal recognition and to this day is irreplaceable in clinical practice. But further studies showed that the original interpretation of this reaction and its theoretical principles from which Wassermann proceeded were wrong. It became clear, for example, that water extracts from organs of persons infected with syphilis may be replaced by alcoholic extracts which do not contain microbe proteins but only lipoids. The same results may be obtained when use is made of extracts prepared not from afflicted but normal organs of men and animals. It was demonstrated that the serum of human blood, immunised by bodies of destroyed *Spirochaeta*, does not produce a positive Wassermann reaction and also that this reaction is not specific to syphilis. It is positive in some other *Spirochaeta* infections (recurrent fever, leprosy, malaria and sometimes scarlet fever).

Nonetheless the theoretical and practical importance of the reaction discovered by Wassermann was enormous. Apart from its exceptional practical value (a non-specific reaction is easily compensated by control diagnosis of infectious diseases different from syphilis), this reaction in fact marked the beginning of serology as a scientific discipline. Though modern science has made considerable headway in the attempts to give theoretical interpretation of the Wassermann reaction, to this day there is no generally accepted interpretation and final conclusions have not been drawn yet. However, the very problematic nature of these attempts pointed to the fundamental character of the basic concepts of bacteriology and immunology and emphasised the inadequacy of one-sided conceptions of the disease and the need for a comprehensive systematic approach to the questions of bacteriological medicine and biology of man.

Fleck uses this example as an illustration of the basic epistemological thesis of his book: facts are not discovered but are shaped by scientists according to a commonly accepted, 'vision of the world', to a style of thinking *(Denkstil)*. Analyzing Wassermann's discovery, Fleck takes the view that it became possible because the obtained result fully corresponded to the expectations of scientists, whose style of thought was moulded under the influence of classical ideas of immunity — as being a specific reaction of the organism to infection. Owing to this, the discovery was almost immediately recognised by the majority of specialists, despite the fact that there were rather sufficient grounds (as it turned out later on) for doubting not only Wassermann's initial theoretical premises, but also the authenticity of his experiments. The absence of reliable control experiments and the pure culture of *Treponema pallidum* (i.e., the presence of heterologous antigens in the serum), rather imperfect extraction techniques, almost complete lack of information as to

what degree of hemolysis should be seen as a positive (or negative) reaction, low reproducibility — all these and other factors, otherwise quite sufficient in order to reject the proposed diagnosis method or, at any rate, to treat it with a large measure of scepticism, were not taken into account.

This was undoubtedly the effect of such factors, seemingly 'extraneous' in academic science, as acute psychological rivalry between the German and French bacteriological schools, the policy of financing microbiological research by government institutions, considerations of national prestige. In Lakatos' terminology, the scientific community's recognition of the Wassermann reaction under the impact of the afore-mentioned (and similar) factors is an episode in the 'external' history of science. A fuller description of this episode should include also factors of a psychological nature: enthusiasm of the scientists aware of the broad possibilities openning before practical diagnostics, the desire to step over the formerly seemingly insurmountable barrier as quickly as possible when doctors for the first time in the history of medicine got not only a means of diagnosing syphilis at a curable stage but also a means of preventing syphilitic epidemics.

But, in Fleck's view, the main role in the recognition of the Wassermann reaction as an outstanding discovery of medical bacteriology was played precisely by the 'conformity' of the results of his observations to the style of thinking prevailing in microbiology. Wassermann saw and comprehended what he was to see and comprehend as a member of a definite scientific community sharing the basic mode of world vision specific to this community.

Fleck sees in this example a confirmation of his idea that a scientific fact is not a result quite independent of theoretical premises (a protocol statement in neo-positivistic terminology) but, on the contrary, the content of observation turns out to be largely dependent on a definite thought style which alone makes it possible to 'see', in a chaos of isolated perceptions, something acquiring the status of a scientific fact later on.

Here an analogy could be drawn to the concept of the 'theoretical load' of the data of observation which plays such a substantial role in the works of contemporary philosophers of science. It is important to note that Fleck has in mind something more than a mere statement of the obvious fact that a scientist always employs certain preliminary theoretical assumptions, hypotheses in selecting empirical data. This was never denied by neo-positivists [4]. But for them the facts always served

[4] Let us recall by way of example what A. Ayer said in connection with the history of Kepler's discovery of the laws of the movement of planets. As is known, from the mass of empirical data accumulated by Tycho Brahe Kepler consciously

as a foundation and ultimate basis of any theoretical discourse while Fleck asserts a reverse dependence: facts not only bear the 'imprint' of a theory, they are determined in their rise and subsequent development by theoretical premises constituting the framework of the 'thought style'.

But the 'thought style' as Fleck sees it is not just a certain conceptual framework or an independent system of concepts, perceptions, views, estimates, methods of problem solutions and the like, closed in its logical integrity. It is first of all a social and not logical concept, though both aspects are interconnected. The thought style is, on the one hand, an essential condition and premise for the existence of special social groups, communities participating in one and the same intellectual process ('thought collective', *Denkkollektiv*) and, on the other hand, precisely the collective, group, social character of intellectual processes finds its expression in a given thought style. 'Every epistemological theory is trivial that does not take this sociological dependence of all cognition into account in a fundamental and detailed manner', L. Fleck said. 'But those who consider social dependence a necessary evil and an unfortunate human inadequacy which ought to be overcome fail to realize that without social conditioning no cognition is even possible. Indeed, the very word "cognition" acquires meaning only in connection with a thought collective' [5].

Attaching exceptional importance to these interconnected and mutually determinible conceptions, L. Fleck believes that the traditional 'subject-object' epistemological relationship should be replaced by the tripartite link 'subject-thought collective-object' in which the main role is played by the second component, for it is the thought collective that determines the character of the thinking activity of the individual and, in consequence, also determines the character of the object under study. 'Cognition is therefore not an individual process of any theoretical "particular consciousness". Rather it is the result of a social activity, since

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selected those which acorded with the Copernican system. Since this system diverged more from the facts (observations of Tycho Brahe) than the Ptolomaic system, Kepler boldly changed the mathematical expression of Copernicus theory without changing its content and postulated the elliptical form to the Mars orbit. This example, Ayer says, shows that the induction model of scientific theories as mere generalisations of observed facts could be wrong even in relation to empirical theories. Observations must be selected. In order to know what is being observed and under what conditions, some preliminary hypotheses are needed. A. Ayer: *The Origins of Pragmatism. Studies in the Philosophy of Charles Sanders Peirce and William James.* London 1968, p. 89.

[5] L. Fleck: *Genesis and Development of a Scientific Fact.* Ed. by T. Trenn and R. Merton. Chicago—London 1979, p. 43.

the existing stock of knowledge exceeds the range available to any one individual' [6].

What is the role played in this tripartite relationship by the objective reality the study of which is the object of individual efforts of the subject and the efforts of the 'thought collective' determining the direction, content and meaning of individual cognitive actions? This is the most difficult question for L. Fleck's epistemological conception. He realised the danger of subjectivisation of thought processes in scientific cognition and tried to avoid it by differentiating 'active' and 'passive' elements. 'Active' elements are a multitude of premises, concepts, ideas the choice of which may be described and explained on the basis of analysis of historical and socio-psychological factors of thought activity. In other words, active elements of knowledge and thinking are special characteristics of the thought style appearing as necessary conditions of the cognitive process itself. 'Passive' elements constitute what is found as a 'residue' of knowledge, as the content of it which does not fit into a multitude of assumptions and their consequences and which is therefore taken as an objective and 'real' guarantor of the coherence of the entire given system of knowledge.

However, analysis of 'passive' elements shows their heterogeneity, the presence in any empirical statement of non-obvious premises dis-

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[6] *ibidem*, p. 38. L. Fleck apparently took the idea of social determination of the types and styles of thought from E. Durkheim and L. Lévy-Bruhl. Let us quote for the sake of comparison some statements of the latter: 'Ideas called collective [...] may be distinguished by the following criteria specific to all members of a given social group: they are handed down in it from generation to generation; they are imposed in it on individual persons [...]. In their being they do not depend on an individual person. This is so not because these ideas presuppose some collective subject different from individuals comprising the social group but because they display features which cannot be comprehended and grasped only through the examination of the individual as such [...]. In order to understand the mechanism of social institutions, especially in lower societies, we should before that get rid of the prejudice contained in the belief that collective ideas in general and ideas in lower societies in particular abide by the laws of psychology based on analysis of an individual subject' (L. Lévy-Bruhl: *La Mentalité primitive*. Paris 1922. German edition 1927. Quotation after a Russian edition: *Primitivnoe myshlenye*. Moskva 1930, p. 5). Fleck tried to apply this idea of French sociologists to the analysis of developing scientific knowledge. No doubt, this attempt was paradoxical: in fact it lead to the identification of cognitive processes of science and psychological phenomena characteristic of preliterate peoples. This conversion of epistemology into a department of social psychology, most probably, sharpened and laid bare contradictions intrinsic in this doctrine and, first and foremost, by hypertrophy of the *socio-psychological* aspect of thinking (including scientific thinking) to the detriment of *general epistemological and logical aspects*. Let us not forget, however, that L. Fleck's arguments were sharply polemical and were directed against the hypertrophy of the *logical* aspect of thinking among neo-positivists!

playing their affinity with the given thought style. From this Fleck drew the conclusion, that the objective reality is a changing passive component of knowledge which cannot in general be separated as something stable and immutable. 'Is we attempt a critical separation of the so-called subjective from the so-called objective in concrete terms, we will find again and again the active and passive links within knowledge that were mentioned earlier. Not a single statement can be formulated from passive links alone. Active links, usually inappropriately called "subjective", are always involved. A passive linkage cen considered active from a different point of view, and vice versa [...]' [7].

Confusion with reference to the concepts of 'subjective' and 'objective', which is not removed but rather compounded by the differentiation between 'active' and 'passive' elements of knowledge in the given context is obvious (and T. Kuhn makes this point in the preface to the quoted edition of L. Fleck's book). Indeed, Fleck comes up here face to face with a formidable problem of activity of cognition, a problem which played such an important role in the dissociation of the dialectical and metaphysical traditions in the history of philosophy. He is indubitably right when he rejects the naive-realistic treatment of knowledge as a totality of immediate 'copies' of the objective reality taking shape in the consciousness of an empirical individual. He is also right saying that the clue to the mystery of human cognition should be sought in the analysis of sociohistorical characteristics of practical human activity. But making a correct guess about the general direction of the road, Fleck makes a mistake in choosing the correct road and therefore fairly rapidly finds himself in the dead-end street of relativism. Activity of consciousness is for him not a fundamental dialectical characteristic but a consequence of a socially and psychologically conditioned thought style of thought collectives.

Hence, the inevitable relativisation of the concept of truth which Fleck is inclined to see as being fully determined by a given thought style; the latter 'sanctions' the recognition of an observation as true. From this point of view he criticises 'dogmatists' in science for whom truth is identified with the available stock of knowledge at each given stage of scientific development. L. Fleck justly remarks that this position is totally unfit for interpreting the growth of scientific knowledge and, especially, the 'focal', revolutionary stages of this growth. But the trouble is that the position taken by Fleck himself is no less vulnerable. The assertion that truth is conditioned by a collective decision leads to the negation of a progressive change in science as a transition from some relatively true knowledge to other knowledge. 'One can never say that the same thought is true for A and false for B. If A and B belong to

[7] L. Fleck: *Genesis* [...], p. 49—50.

the same thought collective, the thought will be either true or false for both. But if they belong to different thought collectives, it will just *not* be *the same* thought!... Truth is not a convention, but rather (1) in historical perspective, an event in the history of thought, (2) in its contemporary context, stylized thought constraint' [8].

We see how in this aspect L. Fleck's (just as later T. Kuhn's) criticism of naive cumulativism becomes relativism and a dubious conception of incommensurability of thought styles. True, in the works of Fleck himself we will not find the concept of 'incommensurability' in the meaning in which it figures in the works of T. Kuhn, P. Feyerabend and T. Toulmin. Nor do we find references to the works of K. Ajdukiewicz, the author of the famous conception of 'radical conventionalism', one of whose main components was the thesis about a principled mutual non-translatability of semantically closed theoretical systems ('Ajdukiewicz's languages'). These works appeared in print almost simultaneously with L. Fleck's book, but we do not know whether L. Fleck was fimaliar with Ajdukiewicz's ideas when he worked on his monograph. Naturally, the conception of 'radical conventionalism' advanced by K. Ajdukiewicz [9] and L. Fleck's historically oriented anti-positivism were separated in the middle of the 1930s by a distance hard to cover. Nonetheless, as the further development of both trends showed, this distance was in the long run cut to a minimum in the 1960s by the 'historical' school in the philosophy of science. As demonstrated by J. Giedymin's studies, in Kuhn's and Feyerabend's conceptions we find transformed variants of Ajdukiewicz's ideas to no smaller extent that L. Fleck's ideas subjected likewise to an appreciable reworking [10].

Indeed, though L. Fleck and K. Ajdukiewicz approached the problem of comparison of different theoretical systems from completely different and even opposite philosophical positions, there was much in common between their conclusions. According to L. Fleck, the facts of observation are determined by the initial conceptual foundation of the observer; according to K. Ajdukiewicz, the meanings of all terms and expressions of a given closed theoretical system are fully determined by the sum-total of initial (conceptual) terms and axioms containing them. In fact, for building a 'bridge' between these conceptions just a few important steps had to be taken: to recognize the fundamental theory swaying the minds of 'thought collectives' as a semantically closed 'Ajdukiewicz

[8] *ibidem*, p. 100.

[9] K. Ajdukiewicz: *Das Weltbild und die Begriffsapparatur*. 'Erkenntnis' Vol. 54:1934, 4, p. 259—287.

[10] J. Giedymin: *Science and Convention. Essays on Henri Poincaré's Philosophy of Science and the Conventionalist Tradition*. Oxford 1962; J. Giedymin: *The Paradox of Meaning Variance*. 'The British Journal for the Philosophy of Science' Vol. 21:1970, p. 257—268.

language' and postulate the social and psychological determination of the choice of this theory. For both these steps (subsequently taken by T. Kuhn and P. Feyerabend) we easily find the grounds in L. Fleck's work.

The style of thought is sometimes defined by L. Fleck as 'the readiness for directed perception, with corresponding mental and objective assimilation of what has been so perceived' [11]. 'Directed perceptions' or *Gestaltsehen* arising within the framework of a given style of thought become the basis for further intellectual and perceptive actions and for instruction. Subjects not sharing this basis cannot belong to a given thought collective; to be a scientist means to share the mode of world vision of a given scientific community.

Going away from the extremes of positivistic induction and cummulation, L. Fleck falls into another, opposite extreme, rejecting the concepts of objective truth and actually depriving the concept of scientific progress of a definite sense. He undoubtedly deeply felt the dynamism of scientific knowledge, its evolutionary, historical nature. It is impossible, he said, to understand a single modern scientific idea without analyzing the history of its formation. But, as this has happened more than once in the works of bourgeois methodologists and philosophers, historicism and dynamism in the description of scientific processes lacking dialectical--materialistic perception of reflection, become in Fleck's work relativism and concessions to irrationalism.

L. Fleck realised full well that the most original and unexpected thought style does not arise in a void: 'Probably only very few completely new concepts are formed without any relation whatsoever to earlier thought styles' [12]. The logical-conceptual framework, the main concepts of the thought style pass through a complex historical process of formation, beginning with certain preliminary states ('pre-ideas', proto-theories, etc.). It can be said, for example, that the idea of an infectious disease is the modern result of complex transformations of the ancient 'pre--idea' of the evil spirit moving into the human body. Generally known are the attempts to trace the history of some fundamental physical or cosmological ideas to the ancient atomism or *Mahabharata*. It is important, however, that such 'retrospections' become possible only from the position of an already formed thought style. In other words, the style determines not only the content and methods of solving present problems, but also the understanding of their genesis (history is viewed as pre--history) and also the directions of their further evolution (future development of knowledge appears as a series of replies to the questions

[11] L. Fleck: *Genesis* [...], p. 99.
[12] *ibidem*, p. 100.

raised today but not yet solved). In other words, the past and the future of science are viewed as projections of the present.

But the real history of science includes not only the evolution of pre-ideas and proto-theories to their present forms, but also a radical replacement to thought styles. Formulating this exceptionally important idea, L. Fleck directly comes up to the concept of a 'scientific revolution'. Here, as in other aspects of his conception, L. Fleck becomes captive of a hypertrophy of the social and psychological components of epistemological analysis. Just as T. Kuhn later on, he tries to explain revolutions in science exclusively in social and psychological categories, representing a change in a thought style as a derivative of changes in the psychology of members of a thought collective.

Of interest is L. Fleck's idea that significant changes in the social psychology of scientists marking a transition from one style to another are closely connected with radical transformations of the psychology of society as a whole caused by important historical events (wars, revolutions, etc.). 'For the sociology of science it is important to state that great transformations in thought style, that is, important discoveries, often occur during periods of general social confusion. Such "periods of unrest reveal the rivalry between opinions, differences between points of view, contradictions, lack of clarity, and the inability directly to preceive a form or meaning. A new thought style arises from such a situation. Compare the significance of the Early Renaissance or of the period following the Great World War' [13]. We should not, of course, interpret this idea in a vulgar sociological manner, i.e., directly associate any great scientific achievements with concrete political or economic upheavals. On the other hand, we cannot disregard also the impact of scientific changes on social consciousness, the state of the economy or from of policy. But L. Fleck is, of course, right in the view that the movement of science cannot be understood apart from the general socio-historical dynamics. Science is not an abstract filiation of ideas but a complex element of a general cultural process developing in contradictory interaction with the entire system of social relations and links. It is another thing that L. Fleck, apparently, took a simplistic view of this interconnection, treating social upheavals only as a background, a general condition facilitating the shattering of habitual foundations, contributing to a more rapid destruction of the old and the rise of a new thought style in science. But he saw this process itself as a consequence of some logically inexplicable changes in the psychology of individual members of scientific communities.

Let us point out that the problem of determination of radical changes in the world vision and social psychology of scientific communities re-

[13] *ibidem*, p. 177—178.

mains debatable to this day. It is an undoubtable merit of post-positivistic philosophy of science and historiography that drew attention to the indissoluble connection between epistemology and sociology, as well as social psychology, demonstrating the inadequacy of an image of science built in artificial isolation from the real history of science. We owe it to the critical discussions of the 1960s and 1970s that our present-day ideas about scientific progress, the possibilities of a rational reconstruction of the history of science and the character of changes in the social structure of science are less abstract than they used to be and are nearer to historical realities. At the same time, is should be stressed that precisely the study of historical reality calls for more profound epistemology than the one revealed in the foundations of many non-Marxist conceptions of our time.

Of late the view has been expressed more and more often that changes in the social structure of science and far-reaching shifts in the relations between science and society which have come to pass in this century should be seen as a revolution, possibly, more significant and large-scale than revolutions in separate, including fundamental, fields of scientific knowledge. This idea is stressed, for example, in J. Ziman's works [14]. This idea viewed in the context of modern discussions about the nature of scientific revolutions, goes to the very root of the matter. It is not only that the sociology of science can adopt the category of a scientific revolution no less successfully and fruitfully than the methodology and philosophy of science. The question is posed more radically: neither the formulation nor the solution of epistemological problems can be adequate unless they reach an organic synthesis with the problems of the social and socio-psychological nature. In other words, the future development of out ideas about science, its structure and dynamic characteristics is connected with the reunification of the currently dissociated scientific disciplines, including sociology and social psychology of science alongside with logic, methodology and philosophy of science, into an integral, synthetic 'science of science'.

From this point of view it can be said, for instance, that the formation (just as destruction for that matter) of 'thought style' (in Fleck's terms) or 'paradigm' (in T. Kuhn's terminology) — making reservations, of course, concerning all substantiall differences between these categories — is not only and, perhaps, not so much an intellectual reply of the scientific community to a crisis situation in science as, rather, fulfilment of a definite 'social order', bringing into a state of relative harmony the interests of the scientific community (as a thought collective) and the interests of society as a whole. In short, the rise and

[14] J. Ziman: *Public Knowledge. An Essay Concerning the Social Dimensions of Science.* Cambridge 1968.

disintegration of scientific communities as thought collectives is determined by the system of interacting factors among which cognitive and social factors can be separated only analytically.

It follows from this directly that the traditional formulation of the question about scientific progress must be altered and embrace not only the problem of continuity in the development of knowledge, certyfying its relative truth in the general world, but also the problem of humanistic values in their organic unity with scientific and technical achievements.

Take, for instance, the problem of 'academic freedom' widely discussed at present. There can be hardly any doubt that this problem is directly related to the problem of 'scientific progress'. But in our days we more and more often see how this problem is being shifted from the cognitive to the economic and even moral field. And changes in one of these fields influence all other aspects of the scientific establishment.

Marxism has long ago come to the conclusion that the freedom of science cannot be regarded or, still less, practised in isolation from the freedom of society as a whole. This is undoubtedly true of scientific progress as well. Let us stress that in the complex mechanism of social movements science should not be a passive element. The scientific community can and must become a medium cultivating and developing humanistic principles and values. Apparently, there can be no question of mere being a progress of science outside this very movement towards the realisation of one of the most beautiful ideals of mankind.

Now back to L. Fleck's arguments. Seeking the clue to psychological changes underlying the transition from one set of thought styles to another, he turns to the analysis of the structure of thought collectives. These consist of two basic 'circles': the esoteric circle of 'experts' (the scientific community proper) and the exoteric circle of people sharing the thought style of 'experts' but who do not directly produce scientific results. Each of these circles is in turn divided into sub-groups: a sub-group of 'narrow experts' (specialists in individual fields of science, people producing scientific ideas; 'journal science'); a sub-group of 'general experts' (specialists of a wide range, authors of generalising monographs, text-books, and reference books); a sub-group of those initiated into science (students, rank-and-file scientific associates — they use the literature produced by 'experts'); and, lastly, a sub-group of amateurs: they are not engaged in professional scientific activity and draw information, views and concepts from text-books and popular literature. A continuous contact is taking place between these sub-groups. Some individuals may under certain circumstances pass from one sub-group to another (e.g., yesterday's student may tomorrow become an 'expert'). Ideas begot by experts pervade all layers of thought collectives and then by the principle of feedback return to the esoteric circle, having ex-

perienced the impact of collective thought. The more democratic a collective, the more intensive and fruitful is the exchange of views between its members; at the same time, the more probable is the appearance of 'heretics' who wittingly or unwittingly come into a conflict with the generally accepted mode of conceptual interpretation of observations and begin to see what others cannot or do not want to see.

L. Fleck describes an experimental situation in which A. Neisser and his colleagues in 1906 discovered mutation changes in a bacterial culture — a fact which would not have been discovered if the experimentators strictly adhered to the principles of classical bacteriology with its conception of immutability of bacterial types. In order to 'see' mutations, they had to have the courage of observing a bacterial culture under a microscope not after the lapse of 24 hours as prescribed by the classical tradition, but in the course of several following days. More often than not, such a 'heresy' is committed by people with a penchant for autonomous, sovereign thinking, who are in themselves a 'thought collective' consisting of one man conducting a dialogue with himself, people who can afford the luxury of having their own opinion and not trusting authorities blindly.

However much endowed these people may be, their road to the discovery of the new is exceptionally hard. Resistance to thought style is a long and hard process, sometimes impossible altogether without any additional conditions (for example, L. Fleck remarks, the discovery of mutations in a bacterial culture happened in America and not in R. Koch's homeland where the influence of the classical tradition was too strong). At first a researcher refusing to look at the world through the prism of a given through style has the feeling of extreme uncertainly, vague images scarcely manage to break through a chaos of perceptions, a scholar is learning to see things anew, as it were. Then gradually a conceptualising intellect joins in the work, building a new framework ('prism'), possibly from the debris of the old or testing an already existing alternative framework: a competing theory, a proto-theoretical scheme, etc.[15] L. Fleck does not even try to find rational descriptions of this process, referring it wholly to the field of creative psychology. However it may be, the chaos of perceptions ultimately gives way to regular outlines — a new thought style arises and gains ground and within its framework are formed scientific facts, i.e., conceptualised constructions from the perceptual material grasped and formalised within a new thought style. 'This is how a *fact* arises. *At first there is a signal of resistance in the chaotic initial thinking, then a definite*

[15] Subsequently, P. Feyerabend would lay more emphasis on these ideas of L. Fleck, proclaiming the principle of proliferation of theories and the existence of a stock of alternatives as an essential condition of scientific progress and the only dependable 'antidote' to ossified dogmatism.

thought constraint, and finally a form to be directly perceived. A fact always occurs in the context of the history of thought and is always the result of a definite thought style' [16].

And so, science develops in two cardinally different ways: the first is the following of the well beaten road of the thought style established by authorities and the other way is the smashing of traditions and styles, discovery of new roads, creation of a new thought style. It is not hard to see an analogy between these ideas of L. Fleck's and the later division into 'normal' and 'revolutionary' science in T. Kuhn. In L. Fleck's case, however, 'narmol science' is free from the monotonous algorithmic structure ascribed to it by T. Kuhn. The example of the discovery of the Wassermann reaction shows that, even within a given thought style, scientists reach totally new and unexpected results, even if their expectations fully conform to the initial theoretical premises. The principle of creativity is organically intrinsic in science and, even when a scientist follows well-established prescriptions, he may discover something totally unknown, like Columbus who sought India but discovered America.

And still, most decisive breakthroughs are made by science owing to 'heretics'. The lot of heretics themselves shapes up differently: either they remain heretics and then their ideas, repudiated and ridiculed, are doomed to 'waiting for their time' or to oblivion; or heretics gradually create their own thought collective and already in the capacity of 'dogmatists' excommunicate other 'heretics' from it.

The dramas of individuals end with their lives. The drama of ideas continues infinitely. Each time a new style of scientific (and not only scientific) thinking lays claim to possessing the objective truth, to the only correct reflection of reality, the next round of cognition refutes this conviction, thereby revealing and accentuating its social and psychological status.

We have already mentioned L. Fleck's relativistic motives. It is important to determine how far relativism goes in his ideas about science and its history. The relativism of a professional methodologist, studying phenomena and processes characteristic of science as if from the sidelines, is one thing. Decisive here may be the philosophical position of the researcher which dictates to him the method of evaluation. And it is yet another thing when we deal relativistic tendencies in a practical scientist like L. Fleck. Epistemological relativism which he borrowed from his philosophical sources was bound inevitably to enter into conflict with the intuition of a researcher for whom the continuity of scientific knowledge is quite natural and vitally important. L. Fleck did realize the relativity of the truth of the methods of explaining objects and processes proposed by science; historicism is a substantial feature of

[16] L. Fleck: *Genesis* [...], p. 95.

his epistemological views. But while polemically sharpening his argumentation against anti-historicism and metaphysical fundamentalism of logical positivists, he unjustifiably absolutised the relativity aspect of scientific knowledge which makes his position rather vulnerable to criticism.

In L. Fleck the history of science appears as an endless process of fall and rise of scientific ideas. But what it is that sets this process in motion, are there internal mechanisms lending themselves to rational description and without which movement would be impossible? In L. Fleck we can find only nebulous hints at the scientists realisation of the practical social significance of scientific achievements and also at the ethical characteristics of men of science worshipping the ideal of truth which hallows their self-abnegation, the necessary depersonalisation in a thought collective and, at the same time, something that pushes them on to a heresy, to a revolt when the time comes for a new radical step in science[17]. All this is interesting and true but, naturally, not sufficient for explaining the driving forces of science.

Now, after four and a half decades of its original publication L. Fleck's book can be read differently. One may see in it additional material for the study of the anti-positivistic tradition in the philosophy of science, an interesting page in the history of Polish culture and science in the first half of the 20th century. It can be read also as a pioneering work largely anticipating the later methodological ideas of T. Kuhn, P. Feyerabend and S. Toulmin and also as one of the early works on the sociology of science. A still closer look at it will reveal a germ of deep philosophical and methodological ideas which only now begin to come into the focus of attention for specialists and the relevance of which is emphasized by the latest requirements of scientific and technological development. Such is the lot of brilliant and original works: the further they are removed from us in the historical perspective, the more tangible become their merits, and even their defects acquire a special instructive meaning.

L. Fleck's works orientating philosophy and methodology of science on history have themselves become part of history. History is said to give lessons to those who want and can learn from it. Let us point to at least two important lessons drawn from a critical acquiantance with the epistemological reflections of the Polish scientist.

The present-day discussions aimed at the tracing of an adequate image of science show two distinct tendencies. The first of them is the search for a firm philosophical foundation for methodological and scientific models. The shakiness of philosophical basis is the main cause of failures and imperfection of models of scientific development proposed

[17] *ibidem*, p. 142—144.

by contemporary Western writers. A question whether these models are distinct or not deepens today an interest in philosophical problems, until recently unknown to neo-positivists. In this respect L. Fleck's conception is an instructive example of how brilliant erudition, deep penetration into the substance and history of scientific research, getting into a conflict with the absolutization of relative aspects of scientific knowledge and hypertrophy of social and psychological methods, lead eventually to a simplified and paradoxical image of science.

The other tendency concists in constructing a multi-dimensional, comprehensive, systematic image of science in contrast to the flat projections of this image absolutized by virtue of the afore-mentioned philosophical defects. Formulating this task, M. Yaroshevsky writes: 'Science as a form of socio-historical activity has three aspects and can therefore be reflected adequately only in a system of coordinates: subject-logical, socio-scientific and personal-psychological' [18]. Characteristic examples of this tendency in methodological literature abroad are J. Holton's works [19] and the works of S. Toulmin [20]. But the choice of dimensions (coordinates) for building an adequate image of science is objectionable in these works too, just as the methods of the combination of these dimensions. Detailed discussion of this problem would lead us away from the framework of this article. Let us note only the obvious fact that the task in question is still a long way from a satisfactory solution. In this respect L. Fleck's conception gives us an important lesson: its obvious weak points and contradictions largely stem from the one-dimentionality, from an overemphasis of separate social and psychological characteristics and the understimation or distortion of other dimensions of science. At the same time, strong and valuable aspects of L. Fleck's analysis reside precisely in his search for new facets in the image of science.

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[18] M. Yaroshevsky: *Struktura nauchnoy deyatelnosti (The Structure of Scientific Activity).* 'Voprosy filosofii" 1974, 11, p. 98.

[19] J. Holton: *Tematicheski analiz nauki (Thematic Analysis of Science).* Moskva 1981.

[20] S. Toulmin: *Human Understanding,* Vol. 1. Princeton 1972.