V. V. Nalimov

THE RECEPTIVITY OF HYPOTHESES

> Not the aspiration for discovering indubitable truths should be made the principle but the ability to find the dubitable in what traditionally was considered indubitable.
>
> (A. Lubishev: *Lessons of the History of Cognition* [in Russian]).

> To every thing there is a season... a time to plant and a time to pluck up that which is planted.
> (Ecclesiastes, 3, 1-2).

The attention of scientists is now being drawn to a new branch of knowledge known as the "philosophy of science." It is true, however, that philosophers of this country are not very happy about this word combination and often identify it with logical positivism. Indeed, it would seem better to speak not of the philosophy, but of the logic of scientific development. Science has become an object of study, and there has emerged metascience, i.e., a science studying the logic of scientific structures. This field of knowledge cannot so far boast of generally accepted results. But

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it has done something else: new acute questions have been formulated and discussed fruitfully and originally. With this paper we are making an attempt to enter the discussion. The philosophy of science has not certainly taught scientists to discover the truth but it has indubitably increased their criticism towards their own activities, and this is really a very important accomplishment.

PROBLEMS IN SCIENCE

If we were not afraid of certain schematization we could point to three basic and obvious structural constituents in science —*problems* or questions to be solved; *hypotheses*, whereby these questions are solved, and, last, *means* whereby these hypotheses are accepted or rejected.

We seem not always to understand quite clearly the outstandingly great role played by well-formulated, accepted and permitted questions in our intellectual activity.

Susanne Langer,[1] developing the idea previously stated by Cohen, even believes that the development of every culture may be characterized by a set of questions, some of which are permitted and formulated and others forbidden. This already envisages the limited possibility of rational responses, or, as Langer puts it, a question is an equivocal sentence whose determinant is its response.

Difference between cultures is first of all difference between questions permitted. This statement may best be illustrated by the version of the dialogue between Christ and Pilate given in the Gospel according to St. John. Christ says at the interrogation at Pilate's, "I came into the world for this, that I should bear witness to the truth" . . . Then Pilate asks his next question, "What is the truth?" The question remains unanswered.

Pilate, being a man of Hellenic culture, has to begin with discussing the question of what the truth is. Christ is a representative of another culture where the question is forbidden. Christianity, at least in its early period, used to deal with other questions, say, that of treating good and evil, or the question

[1] Susanne K. Langer, *Philosophy in a New Key, A Study in the Symbolism of Reason, Rite, and Art*, Cambridge, Mass., Harvard University Press, 1951.

of retribution, but these questions could have been posed only if that of the nature of truth was forbidden.

Science at every stage of its development is determined by a set of allowed questions. It is easy to give examples of absolutely forbidden questions of the kind: "Whence came Ohm's law?" "What does it exist for?" or even stronger ones "How, why and with what purpose have the laws of nature been formed?" "What is the goal of world existence?"

Sometimes science seems to outstrip itself and answer yet unformulated questions—in this case the answers prove untimely. Mendel answered a question yet unformulated at his epoch, and for this reason was not acknowledged for a long time. At the same time many clearly formulated questions for a long time remain unacknowledged. This may be illustrated by the case with Malthus: outstripping his time, he formulated a question which was considered unlawful for a long period.

Retrospectively it seems that science can be regarded as a sequence of answers to a series of profound questions. In research nowadays great attention is paid to *experimental design.* But in our book [2] we remarked that an experiment may be designed only when a mathematical model of the phenomenon under study is given. But to give mathematical models means to ask nature a question expressed by symbols. Formulating a question, we state something proceeding from prior knowledge and then ask. Recording a mathematical model we give beforehand the analytical form and independent variables entering it and ask, e.g., what will be the parameter estimates computed on the basis of experimental results.

Profound questions, either explicit or implicit, are a feature characterizing any system which purposefully enriches itself with information. My father, an ethnographer, told me that Northern peasant hunters knew everything that could be observed in nature. But they used to observe without asking questions, and though they did obtain knowledge it was not scientific in this sense. They did not ask questions merely because they had no hypotheses

[2] V. V. Nalimov and T. I. Golikova, *Logicheskie osnovaniya planirovaniya experimenta (Logical Foundations of Experimental Design)*, Moscow, "Metallurgiya," 1976.

without which formulating profound questions is impossible. The state of things seems to be highly similar in some branches of knowledge, especially in the old ones, as biology or psychology. Many times I used to ask people defending a thesis: "What question have you answered?" But the question proved to be absent from the beginning, it was just activities.

Estimating my own experience I would like to remark that I am usually impressed by the conferences, discussions, reports and publications where new questions are formulated. From this viewpoint I try to evaluate now my own works. But often I have to participate in various scientific conferences where in the reports I hear answers to unformulated questions. Sometimes these are just specifications or slight developments of something already done, sometimes an example to support something already known, or, finally, mere comments upon something already said.

At present, due to the attenuation of the previously existing exponential growth of allocations for science, the question of re-distributing funds between various fields of knowledge becomes very acute. Here one needs a *criterion*. And probably the possibility of formulating problems meaningfully and originally will become such a criterion.

HOW HYPOTHESES ARE FORMULATED

We know nothing of how hypotheses in science are formulated. New hypotheses cannot be deduced immediately from observation results.

This statement becomes especially convincing if we, after M. Kendall,[3] compare human abilities with those of a computer. One of the cardinal differences here consists in the fact that a man observing new phenomena can formulate new fruitful hypotheses; we have not so far managed to teach a computer to do this. Inductive logic proves unyielding to algorythmization. Models we are so accustomed to in science may be obtained only from premises and not immediately from observation results. After Karl Popper [4]

[3] M. G. Kendall, "Statistical Inference in the Light of the Theory of the Electronic Computer," *Review of the International Statistical Institute*, 34, n. 1, 1966, pp. 1-2.

[4] Karl R. Popper, *Conjectures and Refutations, The Growth of Scientific Knowledge*, New York and London, Basic Books Publisher, 1963.

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we must acknowledge that the first peculiarity or, if you like, the first paradox in science development is: the creative constituent of science, the process of formulating novel hypotheses, does not possess any traits specific only for science. In any case, we cannot distinguish it from myth-creating.

This is a very important statement which has as a consequence that at the moment of formulating a hypothesis one has not to worry about its foundation; it is more important, according to Russell,[5] to believe in it, supposing that it proceeds from certain intuitive, i.e., merely inexplicable motives. Serious reasons for its foundations can be obtained only during the subsequent theoretical or experimental development. It is only too bad that a scientist already at the first stage, at the moment of formulating a hypothesis, has to say something in order to account for what he cannot as yet account for.[6]

HOW HYPOTHESES ARE ACCEPTED

Scientific hypotheses cannot be verified while they are being tested experimentally. The only thing to be done is to show that it is not falsifiable, i.e., it is not refuted by experimental results. But the same observations may be consistent both with the hypothesis under consideration and with a number of yet unformulated ones. This is an amazing asymmetry: no experiment favorable to the given hypothesis can provide sufficient grounds for accepting it unconditionally, but a single negative experiment is enough to reject it. Hypotheses always remain open for further testing; here, according to Popper, lies the source of the progress of the natural sciences. Indeed, science permanently reconsiders the rightfulness of its hypotheses: the possibility of carrying out a crucial experiment depends both on the level of constantly developing theory and on experimental techniques.

Hypotheses cannot be verified, they can only be unfalsified—this is Popper's second paradox.[7] And though Popper began

[5] B. Russell, *Human Knowledge, Its Scope and Limits*, London, Allen and Unwin, 1956.

[6] The ideas exposed in this paragraph were prompted by Yu. A. Schreider in discussing my report.

[7] Popper, *Conjectures and Refutations*, and *The Logic of Scientific Discovery*, London, Hutchinson, 1965.

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as a member of the famous "Viennese circle" he considers himself not a positivist, but a critical realist.

Popper's second paradox, though in a slightly different form, may be found in any handbook of mathematical statistics. The important thing is that Popper gave a philosophical flavor to this statement well-known to every statistician. This gave rise to an interesting discussion—see, e.g., the collection of papers[8] which contains the papers of such famous philosophers of science as Kuhn, Popper, Lakatos, Feyerabend, Masterman, Watkins and Toulmin. Thus, Lacatos pointed to the fact that hypotheses *are* accepted; this happens when it becomes clear that they predict new interesting facts . . . As a matter of fact, here it would be more pertinent to speak not of hypotheses but of programs for action. And further Popper's mechanism of falsifiability is sure to start working.

But the history of science has witnessed how hypotheses falsified by experimental results still were not rejected. J. Monod, an outstanding biologist, a Nobel prize winner and director of Pasteur Institute in Paris, mentions the trap which once caught Darwin's theory.[9] From Popper's stand, this a second-rate theory since there cannot be a crucial experiment to test it, i.e., an experiment which would jeopardize it. But, historically, such crucial observations once seemed to exist. Darwin's contemporary Thompson, one of the few physicists of the epoch who was able to calculate, showed that under the assumption that the Sun is a heap of coal, the fuel with the highest known caloricity, it could provide the Earth with the energy necessary for the development of life only for a short period of time clearly insufficient for the evolutionary process. This was a purely experimental refutation of Darwin's theory since the dimensions of the Sun and the quantity of necessary heat are experimentally determined values. Darwin was depressed by the calculations and badly spoiled the second edition of his book. But his theory was not rejected. Now,

[8] *Criticism and Growth of Knowledge*, ed. by. J Makatos and A. Musgrave. Proceedings of the International Colloquium in the Philosophy of Science, 1965, Vol. 4, Cambridge University Press, 1970.

[9] J. L. Monod, "On the Molecular Theory of Evolution," in *Problems of Scientific Revolution. Progress and Obstacles to Progress*, The Herbert Spencer Lectures, 1973, ed. by R. Harré, Oxford, Clarendon Press, 1975, pp. 11-24.

remarks Monod, we know that it implicitly[10] contains the concept of solar atomic energy, but who could have known this at that time?

At the same time we know of the immense effect of the negative Michelson-Morley experiment on progress in physics. We feel it is not the negative results which are important but the possibility of comprehending them theoretically.

So, in what way is a hypothesis accepted or refuted? We believe that in science there exist *protective mechanisms* other than falsifiability which allow one to make a *stabilizing* selection.

PARADIGM—A PROTECTIVE MECHANISM IN SCIENCE

Kuhn's greatest merit is that he introduced the concept of *paradigm* into science.[11] To our mind, paradigm is an intellectual field, a fuzzy area of axioms determining what is scientific in science. Paradigm protects science from the weeds.

The statement by Norbert Wiener is well-known, that mathematics is made by 5% of mathematicians, the remaining 95% only playing a protective role sheltering it from being polluted by insufficiently strict constructions. But in what way is this done? The notion of proof itself cannot be strictly formalized, as follows from Gödel's proof.[12]

I happen to be a member of the Section of Mathematical Research Methods of the editorial board of the journal "Industrial Laboratory." We reject about 50% of papers as not interesting or not being strict enough. But our discussions are often very heated since we lack a clear-cut criterion. Paradigm plays a double role: positive, since it allows scientists to concentrate their efforts in one clearly outlined direction, and negative, when it outlives itself and becomes a hindrance to new ideas. New "mad" (Bohr's expression) hypotheses at the moment of their emergence are hard to tell from the weeds.

[10] Monod also points out the fact that Darwin's theory implicitly contained a concept of a discrete hereditary code, while Lamarck suggested continuous changeability.

[11] T. S. Kuhn, *The Structure of Scientific Revolution*, Chicago, University of Chicago Press, 1970.

[12] For greater details, see S. Kleene, *Introduction to Metamathematics*, Amsterdam, North-Holland, 1952.

The paradigmatic pressure may become unbearable, and this situation calls forth ways to overcome it. Here is an illustration. Mathematical statistics, on the one hand, is a branch of mathematics, and its theoretical structure preserves the strict level of pure mathematics; on the other hand, its object is the description of external phenomena, and here mathematical Puritanism becomes ruinous. But this contradictory situation proved to be solvable: American statisticians boast that they have divorced themselves from pure mathematicians. This has given them the opportunity to organize their own departments in many universities, which are quite independent from mathematical ones. They have their own journals. They have their own notion of prestige, which makes the statisticians orient themselves not so much towards the strictness of constructions as towards the significance of the experimental data obtained as a result of applying these constructions. We have nothing of the kind in the U.S.S.R. Here statistics is either an appendage of economics of a branch of pure mathematics. In the latter case it must be as strict as pure mathematics is. As a result we have nothing to boast in applied statistical research.

The second illustration concerns psychology. At the dawn of cybernetics many held the view that broadly formulated problems of control may be solved by the progress of computers and corresponding branches of mathematics. Lately, however, it has become clear that the principal problem of control is *man*. It is now evident that human sciences were hindered in their development by the general scientific paradigm formed due to the progress of exact sciences. The paradigm of exact science has no room for the study of man. All deep psychic human manifestations are principally irreproducible; in human studies the researcher is unable to oppose himself dichotomously to the object of research; theoretic comprehension of the so-called "altered states of consciousness" results in conceptions which seem unscientific within the framework of modern science. But the way out was discovered again: in the United States there are now published 91 journals of psychology, and if the journals of adjoining fields of knowledge are added, their number reaches 148. A variety of small journals is an attempt to create a variety of microparadigms. And, again, there is nothing of the kind in the U.S.S.R.

So, paradigm is a fine instrument. On the one hand, it protects science from litter, on the other hand, it may impede its development. We should be aware of the fact that in science *what was unscientific yesterday, today becomes scientific.* It is of great importance that the system of scientific publications correspond to the logic of scientific progress. But is this requirement fulfilled in our country, where the system of limitations on scientific papers is very rigid?

HOW SCIENCE GROWS

Here we must acknowledge Popper's *third* paradox:[13] the progress of knowledge can be presented as a process of revolutionary change and not mere accumulation. Human knowledge is not accumulated with the growth of science like books are accumulated in libraries and exhibits in museums.[14] In the process of science development the most essential in it, including its language, is destroyed, changed and re-built.

Popper's viewpoint may be interpreted as a concept of permanent revolution in science. It is often opposed by that of Kuhn[15] on the existence of two cycles of scientific development: lengthy periods of *normal science* and short outbreaks of scientific revolutions. During the first ones science progresses quietly, proceeding from certain fundamental knowledge and methodological notions generally accepted at the time. It is to denote the intellectual climate of this period that Kuhn introduced his notion of a much-dwelt-upon paradigm. According to him, a paradigm generates scientific collectives built as closed communities where critical analysis is forbidden.

We do not believe it rational to emphasize the profund oppo-

[13] Popper, *Conjectures and Refutations* and "Some Comments on Truth and Growth of Knowledge," in *Logic, Methodology and Philosophy of Science*, Proceedings of the 1960 International Congress, Stanford University Press, 1962, pp. 285-292.

[14] The concept of exponential or logistic growth of the number of scientists, papers or funds for science is a glance at the same problem, but from a different angle. (See V. V. Nalimov and Z. M. Mul'chenko, Naukometriya (*Scientometrics: The Study of Science Development as an Information Process*), Moscow, "Nauka," 1969.

[15] *Criticism and Growth of Knowledge*, and Monod, *op. cit.*

sition of these two outlooks. They describe the same phenomena as certain metaphors. On a large time-scale we perceive the development of science as a continuous evolution, on a small time-scale as a creation of separate closed collectives often turning off the highway and degenerating in their isolation. But in closed collectives, too, we often observe hidden struggle, and if it is absent then we shall speak of the danger of normal science, as it was put by Popper.[16]

IS THE PROBABILISTIC EVALUATION OF HYPOTHESES POSSIBLE?

Next step in Popper's conception is a fight against the broadly-accepted tendency in science, originating from Laplace, to speak not of the truthfulness of a hypothesis, but of its probability. If, says Popper, we regard the progress of science as the emergence of theories with increasing content, thence it should immediately follow that their probability decreases. To illustrate this he gives the following example. Let us denote by A the proposition "It will rain on Friday", and by B, the proposition "We shall have fair weather on Sunday," then AB will be the statement "On Friday it will rain, and on Sunday we shall have fair weather." It is obvious that the content of the conjunction AB of two propositions A and B will always be greater or at least equal to that of statements of separate components, and the probability of emergence of joint events will always be less or equal to that of the emergence of separate events. It may be symbolically expressed as follows:

$$CT(A) \leq CT(AB) \geq CT(C')$$
$$P(A) \geq P(AB) \leq P(B),$$

where CT (A) means "the content of statement A" and P (A) — the probability of event A. Hence it follows that the increase in the proposition content is accompanied by the decrease of its probability. Popper is apt to consider this simultaneously trite and fundamental result as a discovery.

This fourth paradox by Popper can be substantially criticized.

[16] *Criticism and Growth of Knowledge.*

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We believe that here a very serious idea is expressed without sufficient accuracy, whence comes the possibility of its erroneous comprehension. The point is that it makes sense to speak of probability of an event only when the space of elementary events is given with sufficient unambiguity.[17] If we deal with the probability of a serious scientific hypothesis we have to mention the space of statements on which the probability can be estimated, otherwise everything will lose its sense. New revolutionary theory emerges on the intellectual field formed by a significantly different, previously existing theory. If the probability of a new theory is estimated in the space of statements given by the previous theory, its probability will obviously prove very small—the less, the more revolutionary it looks. If we trace the process of the development of science we shall see that the most promising and fruitful scientific hypotheses at the moment of their creation arouse frantic opposition in scientific circles. Which means that they were regarded as having a small probability from the stand of their intellectual background. (We give a probabilistic description of the mechanism of resistance to new ideas in our paper).[18] Now let us assume that the new hypothesis has predicted new effects not resulting from the old one, and that they have successfully been discovered. The prestige of the theory will immediately rise and it will determine further lines of research. Around the theory a definite intellectual field will be formed, and its probability will increase in the space created by these new

[17] If this is ignored, immediately false paradoxes arise. As an example we shall give the paradox of Mises (as described by V. V. Tutubalin in *Teoriya veroyatnostei* [*Probability Theory*], Moscow, Moscow University Press, 1972): In the classical probability theory there is the definition: two events are called incompatible if they cannot occur simultaneously, and the theorem: the probability of the sum of two incompatible events equals the sum of their probabilities. R. Mises invented the following paradox: a tennis player can go to a contest either in Moscow or in London, the contests taking place simultaneously. The probability of his winning the first prize in Moscow is 0.9. (of course, if he goes there), in London is 0.6. What is the probability of his winning the first prize here or there? Solution: according to the classical theory, the two events are incompatible, and for this reason the probability in question is $0.9 + 0.6 = 1.5$.

This paradox is, as a matter of fact, a result of misunderstanding since the probabilities 0.9 and 0.6 relate to different spaces of elementary events.

[18] V. V. Nalimov, "Novatorstvo kak proyavlenie intellektual'nogo bunta (Innovation as a Manifestation of Intellectual Rebellion)," *Izobretatel i ratsionalizator*, 7, 1976, pp. 38-40.

statements; later the probability may start decreasing again. So when a new and unusual hypothesis emerges its probability estimated on the space of all previous statements will be small. This is synonymous to stating that the hypothesis is unpredicted and revolutionary... By the way, the necessary consequence is the impossibility to forecast scientific and technological progress, since how can we choose out of a set of low-probability hypotheses the one whose probability will sharply increase in the future?

All the above is, if you like, nothing more than a semantic reformulation of Popper's fourth paradox.

WHAT IS SCIENCE?

Proceeding from the concept of falsifiability Popper easily draws a demarcation line between science and non-scientific-metaphysical statements. Only those statements whose validity can be tested may be said to prove scientific hypotheses.

But this demarcation criterion can by no means be regarded as unconditional.

Numerous counterexamples can be cited to show that the principally unfalsifiable phenomena are often considered scientific while phenomena formally unfalsified by an experiment are sometimes viewed as unscientific. Here belong the evolution theory, the hypothesis of the genesis of a biological code, the ideology of psychologism, and finally, Popper's conception as well. They all cannot be experimentally jeopardized but we are prone to classify them as scientific even if some of them give rise to objection. On the other hand, the ideology of Yoga, or, to be more correct, practical recommendations following from it may be experimentally jeopardized, but, nevertheless, modern science, proceeding from its paradigm, is unable to accept it as scientific. The key to separating scientific and non-scientific conceptions should be rather in their propensity to self-development, i.e., to self-destruction. If you like, this is a dialectical definition of science. However, it is only a necessary condition, not a sufficient one: we can point to religious systems which, in the process of evolution, are changed beyond recognition. How can necessary and sufficient conditions be formulated? There is no answer to this question. It seems impossible to divide scientific

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activities from other human activities since everything people do bears the stamp of the versatility of human consciousness.

Here it seems pertinent to ask another question: is science a rational system or an irrational one? The answer will again be equivocal. Science is certainly rational, because to expose and prove scientific ideas one has to resort to formal Aristotelian logic. But at the same time it is irrational, for new hypotheses come as insights; to believe a new low-probability hypothesis and start working in the direction outlined by it one has to base oneself on something other than pure logic; and, last but not least, the choice of a crucial experiment and thinking over its results seems not to be a fully logical procedure. Bohr's principle of complementarity seems to justify metaphoric constructions,[19] in other words, it seems to reject the law of the excluded middle.

HOW CAN THE EPISTEMOLOGICAL ROLE OF DISCOVERIES BE ESTIMATED?

Scientific hypotheses, or, at least, some of them may predict essentially new and previously unknown effects. It is in the possibility of such predictions stimulating precisely directed activity on the part of the researcher that the principal power of scientific theories, and sometimes the criterion of their truthfulness, is found by many. Indeed, we know that a lot of effects in science, especially in physics, were discovered not by chance, but as a result of theortically directed research. However, the following question still remains open: what epistemological value may be ascribed to discoveries in natural sciences? If hypotheses are only guesses sequentially replacing one another and not true cognition of nature in some indubitable and strict sense, then discoveries made by means of these guesses are probably to be interpreted also not as links in the progress of cognition but as consistent and more and more profound mastery of nature. The history of culture supplies us with a lot of examples of the serious

[19] V. V. Nalimov, "O nekotoroi paralleli mezhdu printsipom dopolnitel nosti Bora i metaforicheskoi strukturoi obydennogo yazyka (On a Parallel between Bohr's Principle of Complementarity and Metaphoric Structure of Everyday Language)," in *Printsip dopolnitel nosti i materialisticheskaya dialektika,* Moscow, "Nauka," 1976.

mastery of nature achieved on the basis of fairly odd, from the modern viewpoint, theoretical constructions. Above we have spoken of Yoga. The brilliant achievements of Yogis may be said to be *supported by practice*. And yet modern science cannot acknowledge this conception as a step towards the truth. Another example is the culture of ancient Egypt: its amazing technical achievements were stimulated by altogether odd ideological structures.

Therefore, is it not wiser to be cautious and ascribe to what we call scientific discoveries no more than a status of mastering nature? Theories can stimulate this process in a greater or lesser degree, which may be a measure of the heuristic power of the theory. But why should it simultaneously be a measure of its epistemological power? By the way, it is not at all simple to define or at least explain what true cognition is.[20] Having given up religious concepts are we not ascribing to human beings what has previously been naturally ascribed to the Demiurge, the creator of Worlds? If we are holding to the view of the evolutionary development of the intellect it seems natural to believe that this process originated from the desire to master nature rather than to cognize it. But, probably, we must call the mastery of nature cognition (since we are ignorant of the true meaning to be put into the notion of true cognition), and then all arguments will cease. But scholars are confident that science has epistemological power. This confidence is just a constituent of our paradigm. The answer, "Why are they confident?" is forbidden in the frame of this paradigm. The philosophy of science attempted to break the etiquette acutely formulating this question, but it could not give a convincing answer.

[20] We often hear that space research has allowed us to cognize the Moon. But remember that we say a man cognizes a woman when he has his first intimate contact with her. True, he learns a lot—but what he learns turns out to be a mystery more profound than what he faced before. To make love with a woman, a man must be mature. Humanity has by now matured to the point where it can directly contact the Moon, biological cells, genes, and elementary particles. But isn't it better. in this case as in the previous one. to speak of possession rather than cognition?

SOME HISTORICAL PARALLELS AND THE PRINCIPAL CONSEQUENCE OF POPPER'S CONCEPTION OF GROWTH OF KNOWLEDGE

If we want to give special contrast to a philosophical study, its analysis must be carried out on the background of similar ideas but generated in a quite different intellectual field. Popper's conception is a concluding link in the long chain of European rationalism beginning with the Hellenic world. And therefore it seems natural to compare it not with European, but with Oriental traditions of rationalistic criticism.

One of such early schools of logical criticism in ancient India is the philosophy of Jaina, whose emergence dates back to the sixth century B. C. (for greater details on Jainism see, e.g., the paper by Mahalanobis [21]). Mahalanobis tries to demonstrate the closeness of these ideas to modern probabilistic notions. The authors, describing this system, give definitions of the philosophy of non-absolutism, pluralism, relativism... In this philosophical system, on the basis of logical analysis of nature it is stated that the truthfulness of any proposition is only conventional: an opposite and contradictory proposition can always be recognized as true in this or that sense and proceeding from various grounds; reality can be regarded from various angles. We would have expressed this idea in modern language, following Hutten,[22] as follows: theories in science are just metaphors, they generate models which behave as if, *but not in the same way as* the phenomena described by them.

Logical nihilism was expressed even more violently by Nagarjuana (in the beginning of our century), the representative of the philosophy of Madhiamika (middle way) which has roots in the teaching of Buddha (one can get an idea of Nagarjuana's logic of judgements, by reading for example the English version of his tractatus [23]). By a sequential chain of precise logical judgments Nagarjuana comes to the following conclusions: that

[21] P. C. Mahalanobis, "The Foundations of Statistics," in *Dialectica*, 8, n. 2, 1954.

[22] E. H. Hutten, *The Language of Modern Physics, An Introduction to the Philosophy of Science*, London, Allen and Unwin; New York, Macmillan, 1956.

[23] Kamaleswar Bhaitacharya, "The Dialectical Method of Nagarjuana," *Journal of Indian Philosophy*, n. 3, 1, 1973.

thought cannot cognize either itself or something else; truth is inexpressible; knowledge is impossible; there is no difference between truth and illusion; the world of experience is illusory. By means of logical analysis Nagarjuana tries to prove the impossibility of building empirical knowledge. His results are summed up by Radhakrishnan in his well-known monograph, *Indian Philosophy*:[42]

Nagarjuana exhibits the conditions which render experience possible, shows their unintelligibility, and infers the non-ultimate character of experience . . . The world of experience is bound by the relations of subject and object, substance and attribute, actor and action, existence and non-existence, origination, duration and destruction, unity and plurality, whole and part, bondage and release, relations of time, relations of space; and Nagarjuana examines every one of these relations and exposes their contradictions. If non-contradiction is the test of reality, then the world of experience is not real.

However, Nagarjuana is still on the middle way. He does not reject the truths of the mind even if they are not ultimate. Knowledge of practical truth proves with him the way to transcendental knowledge.

Another thing remains to be mentioned: the ever-repeating refrain of ancient Indian philosophy is that our knowledge is only destruction of ignorance. Liberation from ignorance is the way to Nirvana.

The cautious relation to knowledge in ancient India may be well illustrated by the following quotation from Isha Upanishad,[25]

9. Those who worship ignorance, enter into gloomy darkness, into still greater darkness those who are devoted to knowledge.

True, we should remember that the concept of knowledge in India at that epoch was peculiar, different from the modern one, since it was related to the ethico-applied trend of thought.

Now let us see how Popper's critical realism will look against the background of the rational nihilism of ancient India.

[24] S. Radhakrishnan, *Indian Philosophy*, Vol. 1, London, Allen and Unwin, 1948, pp. 656, 697-698.
[25] *The Upanishad of the Va'yassane'ya Sanhita*, Calcutta, Upanishads, 1853.

Both trends become aware of logical difficulties relating to posterior synthetic judgments. But these difficulties are solved differently. Nagarjuana shares complete nihilism—the confidence in the impossibility of constructing empirical knowledge—while Popper's view is not nihilism but criticism, the understanding of scientific conceptions being only conjectures but in no way structures deduced from experience; the latter has another role, that of falsifying hypotheses. In this case all troubles coming from the logic of constructing knowledge from experience are removed. True, the most important question—the creative process—remains unexplained by Popper: it is just referred to the class of conjectures, i.e., alogical procedures. As a matter of fact, Popper's entire concept is only a factual description of what is going on in European science. Here the following idea, though a bit paradoxical, might be formulated: Indian thinkers proved to be too consistent and could not allow the inconsistent way of scientific development taken by European thought.

But the most interesting is juxtaposition with the notion of knowledge as destruction of ignorance. Popper's concept can be expressed in similar terms. If the growth of science is not a mere cumulation of knowledge but a permanent creation of new hypotheses rejecting the previous ones, then it is no other than a consistent process of destroying previously existing ignorance. It is here that the most significant difference from ancient Indian concepts lies: Nagarjuana limited the fight against ignorance to the development of criticism—he gave up constructing any positive concept since he saw only too well that grounding it he would have to face the same weak points he himself criticised so effectively. In Popper's conception the fight against ignorance is a chain of constructing stronger and stronger ignorance. The justification of this roundabout progress is the process of mastering nature accompanying it. But the process of cognition turns into a series which does not obligatorily converge to true knowledge if the process of cognition could be thought of as going into infinity. Worse than that, we believe that the process of cognition cannot go on long enough without breaking. At every step old ignorance is destroyed by constructing new stronger ignorance which it becomes harder and harder to destroy, as time goes by. Is not it just the state which many physicists have now achieved, especially in the theory of elementary particles? Old concepts in physics prove insufficient both for a profound comprehension of intensively cumulating new experimental data and for prediction of novel effects. At the same time these conceptions are powerful enough to oppose revolutionary change. Here is how the well-known physicist L. A. Artsimovich picturesquely describes the situation in a popular-scientific article.

> They have so far been saved from the most dangerous disease called "Crisis of Genre" (it consists in the disappearance of fruitful scientific problems. In research institutes caught by the disease scientific workers are bursting with energy while the directors pass sleepless nights pondering where to direct the unused energies of a large collective) only by technological applications perpetually increasing in variety and practical value.

At any rate, the prolonged crisis in theoretical physics is the acknowledged fact. Certainly, the construction of such all-embracing and, therefore, unavoidably cumbersome theories as in physics, does not take place in all fields of knowledge.

But let us return to Popper's conception of scientific growth. It ought to result in a certain finely arranged agnosticism though Popper himself does not come to this conclusion. Popper even does not think himself a relativist. He says:

> I am not a relativist: I do believe in absolute or objective truth, in Tarski's sense (although I am, of course, not an absolutist in the sense of thinking that I, or anybody else, has the truth in his pocket) . . .
>
> I do admit that at any moment we are prisoners caught in the framework of our theories; our expectations; our past experience; our language. But we are prisoners in a Pickwickian sense; if we try we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again.[26]

We fail to comprehend Popper's optimism, though, logically, it follows from his premises. We feel like asking the question:

[26] *Criticism and Growth of Knowledge.*

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did not some cultures (say, Egyptian) perish and certain outstanding trends of thinking (say, ancient Indian) decay due to their achieving the level of ignorance (in the latter case expressed, e. g., in the logical extreme of nihilism) which used not to allow the possibility of destroying it? Who knows to what extent the power of ignorance in European knowledge will be conservative?

METAOBSERVER'S GLANCE AT SCIENCE

Imagine that the Earth is visited by a metaobserver from another world, free from prejudices of our conceptions of science. We think his report will probably look like this:

Science is a kind of Game. The Game has special rules which are known and clear to everybody although they have never been classified and codified. The rules have been basically constant for about 300 years. In the process of the Game ingenious and ever more complicated theoretical structures are created, but the players do not seem to believe them in an absolute way. At any rate, they perceive ultimate knowledge as a delusion since only the scholar who manages to destroy what has previously been created is considered truly gifted. What is the prize in the Game? This is not quite obvious: for some this is a possibility to build a most ingenious theory, for others and for those not directly participating in the game the possibility to master previously unknown powers of nature which they unaccountably succeed in doing proceeding from their ephemeridic theories, for still others the possibility of getting hold of something purely material. The latter play the same Game but according to quite different rules, and interfere with other people. One of the principal rules seems to stipulate that the Game must not be dull. The moment it loses its acuteness, more ingenious conjectures start arising, the rules are modified and, what is the most surprising, again everything is all right though it becomes more and more difficult to play.

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