A COMPOSITION ON THE TOPIC PROPOSED

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My colleagues and friends at the Laboratory of High Energies of the Joint Institute for Nuclear Research suggested me that I should write a composition on the topic proposed as to how the interest in science occurred to me, how my adherence to a definite field of physics was accounted for and why I had been working so long at the two institutes, namely the P.N.Lebedev Physics Institute of the USSR Academy of Sciences (FIAN) and at the Joint Institute for Nuclear Research in Dubna.

Having looked through my scientific papers, public talks, interviews and popular articles I found that my point of view on the significance of the value aspects in science has been changing little during almost half a century. My article published in the journal Voprosy Filosofii N.10, 1974 is just devoted to the discussion of the value aspects of natural science. This subject was also considered in my other articles, e.g. in Literaturnaja Gazeta this problem was discussed for many years.

Having been appointed director of the Laboratory of High Energies I much reflected on the strategy of a modern scientific center, on the «purpose function» of big scientific institutions. In the articles mentioned , as well as in my scientific papers, I emphasized that the goal and the value system of fundamental investigations have been formulated by the classics of natural sciences. They are the formation of the theoretical basis, the minimum set of notions and assertions which could be used to obtain in a logical way the notions and assertions in experimental sciences (the world picture). Hence there follows an hierarchy of the significance of the results obtained namely to which level of knowledge they are attributed : either to the general principles (symmetry, invariance), the laws of Nature (relationships between the measurable , invariant and dimensionless quantities), or to the accumulation of facts in the domain of the physical quantities in which the laws have not yet been formulated or there are disagreements in experimental data. Of course, such a purposeful orientation occurred to me not at once.

I took an active interest in physics late, at the age of 20, when I transferred from the third course of the building faculty of the Moscow Institute of Engineers of Railway Transport (MIIT) to the Moscow Engineering Physics Institute (MIFI), the Moscow Mechanics Institute at that time. That was the year 1946, when the best students from technical institutes were selected to become specialists in atomic science and engineering. The competition that took place among smart and self-confident young people much encouraged intensive engagements in science. In addition, there were many prominent and actively working physicists among MIFI's professors who deeply understood the methodology and aesthetic aspects of science. Later on I got to know that the geniuses of theoretical physics such as Poincre and Dirac were raising the aesthetic attitude to the equations and to the scientific results to the level of the methodology of science.

When I was going to the seventh form of the Moscow secondary school number 114, I had the wonderful teacher of mathematics, Anna Sergeevna Almazova. She succeeded in revealing to us, mischievous boys, the beauty of the Euclidean geometry, and made us to feel (naturally we were yet unable to understand it) that the harmony and a strict order underlie the world structure. She awoke our ambitions by provoking us to find the finest and laconic solutions and our aspiration to knowledge. To my mind, that was more important than the knowledge itself. Unfortunately the school courses of physics and chemistry stimulated no interest in them. These courses, as well as the university ones are of fragmentary character and they are presented as a collection of concrete facts and laws without the explanation of their hierarchy, significance, and methodology. For instance, Poisson's brackets are introduced at the end of the university lectures on mechanics as a crown of creation . Usually one does not explain the power of this method which makes it possible to formulate (as Dirac has done it) the theory of relativity and quantum mechanics. Likewise, electromagnetism is presented as separate laws and phenomena without demonstrating the fact that they are only the consequences of Maxwell’s equations , one of the greatest generalizations of the laws of Nature.

I.E.Tamm often liked to develop the idea « the student is not a vessel that should be filled, but he is a torch that should be inflamed ». Tamm’s seminars in FIAN were a wonderful school for physicists of my generation. To be precise, there were two weekly Tamm’s seminars . One of them was a formal one held on Tuesdays ,and the other named semiofficially «triop» (free talks) took place every Friday. A wide range of problems was discussed in the democratic atmosphere. The wonderful ability of Igor Evgenjevitch to grasp the essence of any problem was very important for the beginners, and not only for them, in choosing their own ways in science.

During the last years of studies at the Institute and later on I had the good luck of contacting outstanding scientists who gave me the idea about the methodology in physics, about the major values. After graduating from MIFI, I was especially lucky to be invited to work at FIAN. At this great research institution it was possible to meet, and not only to meet, but also to talk with such eminent scientists as V.A.Fock, M.A.Markov, N.N.Bogoliubov, M.A.Leontovich, I.M.Frank, P.A.Cherenkov, L.D.Landau, D.I.Blokhintsev, I.Ya.Pomeranchuk, E.L.Feinberg, S.N.Vernov, Yu.M.Shirokov and many others.

In high energy physics, the leading position was occupied by D.V.Skobeltsyn’s school, who lead down, in the twenties, the experimental foundations of quantum electrodynamics , discovered the nature of cosmic rays by having detected for the first time particles of energies much higher than those of particles from radioactive sources, as well as discovered the multiple production of particles. It was D.V.Skobeltsyn who directed the attention of home physicists to research into processes occurring at the extreme energies. The synchrophasotron project contains his legible signature «Approved. D.Skobeltsyn» 5 January 1951. Being a brilliant representative of the pre-revolutionary Russian intelligentsia, a highly cultured man, Dmitrij Vasiljevich Skobeltsyn has played a crucial role in forming the traditions and working out directions of the research.

The true creator of FIAN in its present-day state was Sergei Ivanovich Vavilov, who had assembled together the outstanding physicists of our country and developed the research program of the Institute. In spite of the fact that in the hard postwar years he occupied the high position of the president of the Academy of Sciences of the USSR, his firm director's hand and constant support were felt in all Institute activities and especially in the construction of the FIAN acceleration center.

The Skobeltsyn's disciple, Vladimir Iosifovich Veksler was an indubitable leader of world's science in this area .As early as in 1944 , being a worker of Skobeltsyn's laboratory, Veksler made his epoch-making discovery of the phase stability principle. Already in 1947, the first accelerator, the 30 MeV electron synchrotron, was started up at FIAN under the guidance of Veksler. In 1949 one succeeded in putting into operation a 250 MeV electron synchrotron at which photoproduction of mesons was discovered which gave rise to the physics of electromagnetic interactions of hadrons.

Veksler's plans supported by Vavilov , Markov and other physicists from FIAN were the construction of electron accelerators for still higher energies. However this idea met with a sharp resistance of some other specialists in nuclear physics who said that electromagnetism had been studied and had been of no interest. The second viewpoint gained victory and the decision was made to build a proton accelerator.

Thedesigning of the synchrophasotron required much experimental work and the creation of an operating model accelerator. The latter was rebuilt into an electron synchrotron that is in operation up to the present time at the Lebedev Institute. Until 1954, this work was classified as a highly secret one which had resulted in losses of home physicists and engineers' priorities. The synchrophasotron was given a code name «Object KM « (Ring Magnet). In spite of a serious damage caused by the secrecy system , there was also a positive (as far as the subject in question is concerned) effect . The aim pursued by the majority of people who were working in secret areas of science was the results achieved rather than international recognition and the protection of their intellectual property .Those results were appropriated along with the removal of secrecy. This process is going on up to now when books, historical and popular articles are being written. It is very hard to establish priorities and the scientific significance of the investigations performed earlier basing on those late-coming publications. The exception is the developments of concrete designs for which appropriate official documents have been preserved. Among these are the project and the physical substantiation of the Dubna synchrophasotron («Object KM»).

My first supervisor of studies Matvej Samsonovich Rabinovich published a monograph «The foundations of the theory of the synchrophasotron». Thanks to his high honesty, my results were justly evaluated and cited in his monograph and in his other papers. That was a good lesson of scientific ethics, the ability of giving an unbiassed estimate of the significance of the results of his colleagues, especially of young people. My first contacts with Rabinovich date back to 1947 when he proposed to us, the two students of MIFI, V.V.Mikhailov and me, to examine the possibility of creating ring accelerators with race track magnets. He told us that in the distant future such accelerators might play the main role in high energy physics. We were actively engaged in calculations, frankly speaking, not always understanding what was the purpose of these complicated calculations. We began missing lectures and used to spend all free time at the old FIAN on the 3d Miuss street. Veksler's laboratory was situated there, in a small two-storey building. One late evening Veksler came to the room where Mikhailov and I were working .He talked with us a long time and after all he said to our supervisor: « All what these students have calculated should be verified carefully and presented in the form of reports» . He had succeeded in appointing us FIAN employees and we continued our studies. Later on, he came to MIFI to speak as a reviewer at the defence of our diploma theses. Of course, we did not expect such honor. Though there was a rather simple explanation for this. The government decision had been made to build the world's biggest proton accelerator. Veksler was appointed the leader. It was necessary to give the physical substantiation for this project. .

We found ourselves in the epicenter of Veksler's tireless activities. Being inspired by his enthusiasm we fulfilled all the tasks set forth before us. Those might be the qualitative analysis of differential equations, modeling measurements or the business trips. These activities made it possible to determine the primary goal and to throw away the problems and tasks of minor importance. In the severe forties, specialists engaged in the physics of accelerators had high responsibility for their quantitative results. At the same time, the creative atmosphere at FIAN and my teachers constantly made me to understand that the images and pictures of the processes created on the basis of the quantitative results and formulas were the final products rather than these results and formulas themselves. At that time one used to say that those formulas needed to be still « rolled» (to be turned over and over) . Soon Mikhailov and I had come to understand that the accelerators were tools for scientific work and could in no way be the highest goal of fundamental investigations . At that time, we had the great fortune to meet M.A.Markov who was not only an outstanding physicist, but also a true philosopher.

In 1947, his famous article entitled « On the nature of physical knowledge» appeared in the journal «Voprosy Filosofii». It was just for this article that he had been declared a « leader of physical idealism in our country» and was criticized cruelly. Strictly speaking, this paper devoted to the interpretation of the physical and philosophical matter of quantum field theory had just marked the beginning of political persecution of scientists. However we were little interested in these discussions and our attitude to philosophers was sceptical. M.A.Markov was, for us, the first theorist who clearly realized that progress in quantum field theory would be stipulated by experiments on accelerators. By that time, he passed from FIAN Theoretical Department to Veksler's Laboratory and began to develop the program of the first experiments. It was just that we were always discussing. I use to say «we» implying Vadim Mikhailov and me. I met Vadim in 1943 when I passed my school-leaving examinations . Since that time we became inseparable friends. We entered MIIT, moved to MIFI and were appointed to FIAN where we carried out all the scientific work together. We wrote the two diplomas and cast lots which of them would be defended by Vadim and which by me. The same was done with our candidate theses. Unfortunately Vadim had no time to defend his thesis. He died tragically in the Pamir mountains at the age of 25.

For the first time, we went to the mountains in 1945 more or less by chance. An unbelievable beauty of mountains we saw after difficult and scanty boys' life during the war impressed us very much and greatly changed our attitude to the world around us. We decided at once that the mountains were our destiny. I can say with certainty that the most vivid impressions of our youth are connected with mountaineering. Besides, work as a climber trainer was an essential help in student's` poor life. For the twenty years, there has been stored a lot of recollections and impressions .These are especially the first ascents and the peaks that have never been subjugated before. I recall our winter climbings, when everything was seen in a different way, several meetings of strong and courageous people , bright colors of new and new mountain regions. However there are many sad and painful recollections about the true friends who are no more among the living. The loss of Vadim Mikhailov with whom we were inseparable in science and mountains about ten years was the first tragedy in my life. I participated in the rescue operation. But we had not succeeded in saving Mikhailov' s group. Because of very hard weather conditions two other members of our party were lost. About a year I was depressed and could not work at all. Somewhat later , I nevertheless returned to mountains, but the former elation no longer occurred to me. M.A.Markov considered Mikhailov as one of his best disciples and felt his death keenly.

The physics of the interactions of photons with nucleons and nuclei, the physics of electromagnetic interactions of hadrons constantly attracted my attention .Being one of the organizers of the Council on the physics of electromagnetic interactions in the USSR Academy of Sciences, I am president of this Council up to now. I had an opportunity to contact with prominent specialists in this area, to take part in large-scale international conferences. In this connection, telling about the goals and ideals which occurred to me in the process of my work, it should be noted that I was most impressed by Markov's ideas about extensivity and the inner structure of elementary particles, about the fact that the particle form factors could not be rigid, while a deformed form factor implied locality, point-likeness of interactions. The point-like property of the electromagnetic interaction and the possibility of using the smallness of the electromagnetic coupling constant enabled us to apply quantum field theory to predict the behavior of the cross sections for photoproduction of positive, negative and neutral mesons on hydrogen and deuterium.

The researches carried out by Mikhailov and me which were initiated by Markov were the first to determine the lines of development of experiments in the domain of meson photoproduction. From the very beginning of our studies of field theory we were imbued with the physical meaning of our formulas and appreciated experimenters' possibilities of «putting questions to Nature». We were thus enabled to feel the goal when performing calculations and learning the complicated notions of quantum field theory. On the other hand, we found out some gaps in our education and felt the need in the monographic literature in the domain of field theory. At the same time, there appeared thorough articles and books by N.N.Bogoliubov and his school. First of all, it is necessary to mention here the papers of A.A.Logunov, L.D.Soloviev and A.N.Tavkhelidze on the dispersion relations using which it became possible to consider our results on nearthreshold photoproduction as quite rigorous relations. Easy contacts with experimenters in the field of elementary particle physics and nuclear physics, as well as with outstanding specialists in the area of quantum field theory led me in 1958 to the Laboratory of Theoretical Physics at Dubna. I returned to particle acceleration problems in 1968 when I became, unexpectedly for me and others, director of the Laboratory of High Energies and responsible for the fate of « my first love » the synchrophasotron. Thus it is impossible to say that all the time I was purposefully choosing objects of my studies. Most likely, it should be spoken of a fate, circumstances and luck.

As far back as 1963, M.A.Markov suggested the idea that the sum of the cross sections for elastic and inelastic lepton scattering had to behave like the cross section of scattering on a point-like charge. This idea forestalled by five years the classic experiments on deep inelastic electron-nucleon scattering. It is generally agreed that it was just these experiments and their subsequent interpretation that were the first to show that the scattering on a proton behaves like that on a point-like charge. They initiated the interpretation of the quarks as real point-like constituents of the hadrons. Markov's idea , that was published in due course, is justly cited as a starting one in the well-known papers on automodelity (self-similarity) of Matveev, Muradian, and Tavkhelidze. These papers had greatly stimulated the beginning of the research in the field of relativistic nuclear physics and the development of the plans of modernization of the synchrophasotron. It became clear to me that the self-similarity laws are consequences of the fundamental symmetries and the self- similarity parameters are the invariants of the appropriate groups. It has become obvious that the self-similarity is not only a distinctive feature of the lepton-nucleon interaction cross section, but it is a common property of nuclear matter that should be revealed in relativistic nuclear collisions. This idea was formulated in my papers in 1971. There it was emphasized that the nearest tasks of experiments were, first, the discovery of the self-similarity laws of relativistic nuclear collisions and ,second, the study of the boundary energy starting from which the asymptotic regime of the nuclear interaction self-similarity was setting in. In the asymptotic region, nuclear matter is expected to behave like a continuous medium. These ideas were shared by an experienced group of experimenters headed by V.S.Stavinsky. In less than a year (by the end of 1971) I had an opportunity to report the results of the discovery of the scale invariance of nuclear collisions and the cumulative effect at a session of the American Physical Society (that is, one year before relativistic nuclear beams were obtained at the Bevatron). Somewhat later Stavinsky's group established the relativistic nuclear energy for which the limiting regimes mentioned began to be accomplished. This nuclear energy of 3.5 - 4 A.GeV was found to be achieved on the synchrophasotron alone which had enabled Dubna to occupy the dominating position in the area of relativistic nuclear physics during almost fifteen years. The establishment of this boundary made it possible to reject the original version of the nuclotron design rated at an energy of 12-15 A GeV and to use efficient iron-shaped superconducting field magnets invented at the Laboratory of High Energies. This made it possible to reduce essentially the cost of the project and to accomplish it under the conditions of a serious economic crisis. My earlier work done in the domains of acceleration technique, nuclear physics and particle physics as well as the investigations in the field of symmetry approaches intertwined mystically in the nuclotron and the research program on it.

I will try to draw away from mysticism and formulate my answer to the questions about the purpose and the values of scientific work («What is to be done ?»). If we return to the beginning of my considerations we find there the general answer of the classics of natural sciences - reductionists: the goal of science is the creation of the world 's picture starting from a minimum set of notions and assertions. Ideally one implies the construction of a mathematical model. The physical processes are described in terms of observables, operations linking physical objects. Complicated real situations require simplified descriptions on the basis of the criteria of validity of the models embracing a class of (abstract, symbolic) mathematical objects such as the numbers or the vectors and the relationships between these objects.

The standard model in elementary particle physics claims to be capable of describing electroweak and strong interactions and is a great achievement of experimental and theoretical physics of the second half of the 20^{th} century. However the standard model contains only those defining axioms which concern the lagrangian symmetries, but this is not enough for the description of physical processes. Some additional conditions (hypotheses) are needed : initial and boundary conditions, assumptions about the constants entering the lagrangians (masses, charges and so on). For example, the assumption about the existence of a renormalizaton group (this is the symmetry of the solutions rather than that of the lagrangian!) made it possible to introduce a running coupling constant and the concept of asymptotic freedom which made chromodynamics a quantitative theory in a definite domain of parameters (hard processes).It is impossible to deduce nuclear physics, including relativistic nuclear physics, from quantum chromodynamics without using auxiliary hypotheses needed to be checked experimentally. The verification of such hypotheses of a rather general nature is not less important than the verification of quantum chromodynamics. For instance, a detailed probe of the correlation depletion principles and the self-similarirty is a task of paramount importance.

However both the lagrangian properties and the self-similarity are consequences of symmetry. Symmetry was considered by ancient Greek philosophers as a particular case of harmony i.e. the concordance of the parts within the whole. The modern methods of systematic - structural studies based on the construction of models and on the group theory are now applied also to the analysis of works performed in literature, art, architecture and music. « The ability to feel symmetry where others do not feel it is, in our opinion, the whole of aesthetics of scientific and artistic works» . (A.V.Shubnikov)