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*THE BEGINNINGS OF NUCLEAR PHYSICS INVESTIGATIONS AT THE PHYSICS INSTITUTE OF THE ACADEMY OF SCIENCES AND CERTAIN CONTEMPORARY PROBLEMS OF NUCLEAR STRUCTURE**

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FIVE years ago, on the seventieth anniversary of S. I. Vavilov, the honorable duty fell to me to present a lecture prepared in conjunction with V. L. Levshin and A. N. Terenin.[1] That lecture contained a sketch of the development of Vavilov's work in the field of physics. It included only those aspects of his investigations which were most important in his activity and could not pretend to be complete. Much could be added to it and above all those things which have been done in the past five years and which constitute a further development of Vavilov's work or ideas. P. P. Feofilov's lecture of this session threw light today on a number of problems. One should however remember that the creative legacy of such physicists as Vavilov includes not only works signed by him, but also the work of his co-workers and students who continue to work out the same problems. There is also another, just as important, aspect to which no reference in the published literature can be given. That is the direct or indirect influence of a scientist's ideas. It manifests itself in the peculiarities of the creative method, in the nature of his research, and sometimes even in the specific choice of topics. It is precisely this influence which should be considered to be the scientific school of a scientist, which should not be identified simply with the aggregate of those who worked or work under his immediate direction. Here I have in mind something more than the organizational help in the work, although the latter is under the conditions of contemporary science of great importance. Important is also another aspect—the personal influence of a scientist, inseperable also to a large extent from his human qualities. This personal influence of Vavilov was so strong that all of us who had personal contact with him still feel his influence now, fifteen years after his death.

It seems important to me to tell in this connection what Vavilov did for the development of work outside the circle of his immediate scientific interests. Here one could speak of many things and it is very difficult to do so in a single lecture. I therefore restrict myself to a specific example, closest to my mind—the beginnings of the development of nuclear physics work which was carried out in the Physics Institute of the U.S.S.R. Academy of Sciences on Vavilov's initiative. I would also like to speak of certain contemporary investigations in this field. Vavilov influenced strongly those who started this work. It is natural to speak in this connection not only of the results of this work, but also of work of younger physicists connected with it.

In 1932 Vavilov was appointed director of the physics section of the Physico-mathematical Institute of the Academy of Sciences located, as the Academy itself, in Leningrad. Just before this, the institute experienced one of its most difficult periods in its 200 years of development. There was a time when its entire staff consisted of a director, two section leaders and four scientific workers.* The time when the Academy of Sciences would become the leader in science and unite within it the main institutes of the country had not come yet, but it was already approaching. At the time Vavilov was appointed, the Physics Section still did not have many workers and the nature of the work was still most patriarchal. Unlike what we are used to nowadays, no watchman met you at the entrance, but instead there was the cosy ring of the bell hanging at the door. However, the bell rang but rarely. There were not many people, and by far not all who belonged to the staff came to the Institute. There were, of course, also some who came and some who worked, which, as is well known, is not one and the same thing. I remember that during my first visit to the Institute, Vavilov pointed out to me a young man who ran quickly through the corridor: "Here, look, he is always here and also always works." This was said of Leonid Vasil'evich Groshev.

At the time Vavilov came, the outlines of the future Physics Institute were as yet by no means determined. Vavilov himself notes that there was a plan to convert it into a purely theoretical institute. There were reasons for this. The Institute included a mathematics section headed by Academician I. M. Vinogradov and

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*Lecture presented on March 24, 1966 at the session of the Presidium of the USSR Academy of Sciences, General and Applied Physics Sections, Nuclear Physics Section and the P. N. Lebedev Physics Institute of the USSR Academy of Sciences, on the occasion of the 75th Anniversary of Academician S. I. Vavilov's birth.

*This is recounted in detail in Vavilov's book.[2]

a wonderful library* was available, but there was little modern physics apparatus. If we recall that there were at the time in Leningrad such first-class institutes as the Physico-technical, Radium, and Optical Institutes established by Ioffe, Vernadskiĭ and Rozhdenstvenskiĭ, then the conversion of the physics section into a theoretical one could have turned out to be most natural. A second, no less natural, possibility consisted in developing here those trends of optical studies with which Vavilov himself was most closely connected. However, Vavilov proceeded differently. His foresight was manifested in the fact that he planned from the very beginning to convert the Institute into a diversified institute including various branches of physics research. After about two years, the government decided to transfer the Academy of Sciences to Moscow; the Physics Section of the Physico-mathematical Institute was converted into the Physics Institute which was, at Vavilov's suggestion, named the P. N. Lebedev Physics Institute. Moscow physicists joined the institute and Vavilov's aim in setting up a physics institute for diverse research was not only fully justified, but turned out to be the only possible aim. Vavilov's foresight was also manifested in the fact that from the beginning, as soon as he came to the Institute in Leningrad, he thought it essential to develop in it research in the field of nuclear physics.

Nuclear physics was at that time entering a period of remarkable discoveries. The positron and then the neutron had been discovered. These were events of great significance for physics and they were widely discussed. In September 1933, the first All-union Conference on the Atomic Nucleus took place at the Physico-technical Institute in Leningrad. Among those who assisted A. F. Ioffe in organizing it was, as far as I remember, also the young I. V. Kurchatov. The conference was not large. About one half of the papers were presented by foreign scientists: F. Joliot, P. Dirac, F. Perrin, and L. Gray. In addition there were lectures by D. V. Skobel'tsyn, S. E. Frish, D. D. Ivanenko, G. A. Gamow, K. D. Sinel'nikov and A. I. Leĭpunskiĭ. Generally they were either theoretical or of a review nature.[3] If I am not mistaken, only D. V. Skobel'tsyn's paper included original, experimental data. A number of communications gave rise to lively discussions. Other speakers, in addition to those mentioned above, were A. F. Ioffe, V. Weisskopf, I. E. Tamm, Ya. I. Frenkel, V. A. Fock, M. P. Bronshteĭn and others. Undoubtedly, interest in nuclear physics increased gradually and an influx began of new people who grouped themselves around those few who had studied it previously.

As regards the Physics Institute of the USSR Academy of Sciences, headed by Vavilov, there were no prior favorable conditions for the development of nuclear physics within it—there was no prepared staff and no apparatus. Vavilov himself did not occupy himself with nuclear physics and did not propose to do so. There was also no external stimulus for the development of work in this field. Nuclear physics was at the time, from the practical point of view, considered to be one of the most useless branches of physics and was not among its chief theoretical problems. Nobody would have considered its presence in the subject matter of the Institute essential.

If Vavilov himself undertook to organize the work in this field under such conditions, then it was of course the result of his very deep understanding of the fundamental significance of past discoveries and, therefore, also of the prospects of the future development of nuclear physics. Vavilov attracted to this work a number of young physicists. I, too, was at the time among those physicists who were totally inexperienced and unprepared for work in nuclear physics. It would, of course, have been simpler for Vavilov to present me with a problem in optics or luminescence for which I was to some extent prepared. Among others, he recommended that I transfer from the Optical Institute, where I worked at that time for A. N. Terenin, to the Academy of Sciences precisely in order to start work in the field of nuclear physics.

It is well known that Vavilov was an enemy of any fashionable trends in science and did not approve of those who chase after spectacular discoveries. He regarded of paramount importance the explanation of the physical essence of phenomena, the investigation of their mechanism, and believed that discoveries should appear precisely in this way, even though they may be unexpected. He was attracted by the fundamental aspects of physical phenomena. In the discovery of positrons, he was naturally above all interested in the process of pair production by gamma rays. He noted that the properties of light manifested in this process have no analog in linear wave optics. His attitude to the fundamental significance of the transformation of light into particles of matter is perhaps best expressed in his words in the book "The Eye and the Sun."[4] He compares it there with the fairy-tale transformation of a melody into a violin. It was no accident, therefore, that on Vavilov's initiative, L. V. Groshev and I started approximately in 1935 to investigate the mechanism of pair production by gamma rays. Our problem was to investigate the elementary act of this process and to observe to this end pair production in a cloud chamber filled with a heavy gas, for instance krypton or xenon. Vavilov was interested here in particular in the wave charac-

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*On being transferred to Moscow the library was supplemented with the book fund remaining from the Institute of Physics and Biophysics headed by P. P. Lazarev. Subsequently, it was enriched not only with current literature, but, to a large extent because of Vavilov's efforts, also with rare scientific publications. The FIAN library remains until now the best physics library in the country. As before, it is directed by I. O. Vreden-Kobetskaya who was already working in the library in Leningrad.

teristics of light waves, and one of the problems he wanted to clarify was the effect of the polarization of light waves on pair production. In a letter, which Vavilov wrote to me in September 1936, when after an illness I was absent from the Institute for a considerable period of time, he informed me: "We have drawn up the plan for 1936. We have left for you and Groshev as the main problem the effect of the position of the electric vector of the gamma wave on the distribution of pairs in space. I thing that we shall not succeed in getting polarized light soon. However, experiments with natural gamma rays are also very interesting." Further he adds: "As regards equipment, the situation is quite good; I have brought from Paris a liter of xenon; we will, apparently, have heavy water; polonium has been ordered; there is hope of getting radiothorium." One sees from this Vavilov's direct support of work in nuclear physics. Vavilov in fact succeeded in ordering and obtaining a small imported preparation of radiothorium which we used to start our work. However, we soon succeeded in solving the problem of gamma sources radically. At the end of 1936 the appropriations available to the USSR Academy of Sciences turned out, as sometimes happens, not to have been completely committed and Vavilov succeeded in obtaining what was at that time a large sum of money for the acquisition of radium for the physics Institute. These were radiomesothorium preparations and to solve the problem of utilizing them most rationally he held a small meeting in which A. F. Ioffe and V. G. Khlopin participated. A number of ampoules containing different amounts of radium (including one of about 500 mg radium equivalent) were retained as sources of gamma rays. The major portion (about one gram) was, on the other hand, dissolved in 1937 and used for obtaining radon. V. G. Khlopin carried out this work personally, extracting at the same time the radiothorium contained in the preparation which we used subsequently with L. V. Groshev. The emanation device was in existence until the beginning of World War II. At that time this was the only setup available to Moscow physicists (Fig. 1). The radon was used chiefly in the form of radon-beryllium neutron sources and was in part given to medical institutions in which the amount of radon used in radiation therapy was always insufficient.*

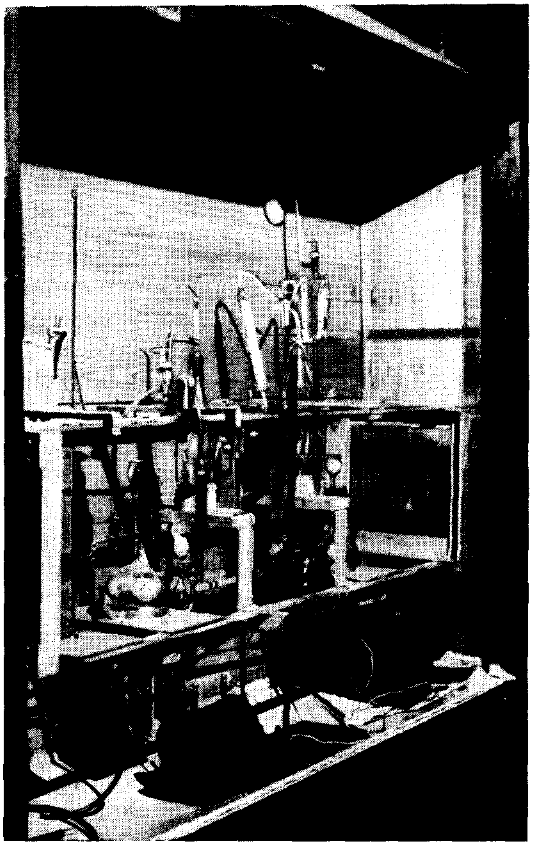

FIG. 1. Emanation setup of the Physics Institute.

Our investigation of pair production in conjunction with L. V. Groshev took several years[5] but we never did get around to elucidating the effect of the polarization of the light which was of special interest to Vavilov.

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*Recalling the work with radium, one cannot but mention a few names. V. G. Khlopin helped us often; one should remember here that all the work of processing radioactive substances was carried out at the time under conditions very harmful to one's health. He was assisted by N. A. Samoĭlo who, until her untimely death in 1940, directed the emanation setup. In 1941, after the war began, Vavilov charged me with ensuring the safety of the radium. The threat of air raids in Moscow became real and it was dangerous to leave the radium in the solution. A hit by a bomb would not only result in the loss of the radium, but also in the radioactive contamination of the surrounding area. After consultations, we decided to evaporate the solution and seal the radium salt in ampoules. Since no specialized premises were available, the solution had to be evaporated on an electric hot plate placed directly in the courtyard of the Institute (this was at the beginning of July and the weather was beautiful). This was done by N. P. Strakhov and I fulfilled the function of a laboratory technician. Although the work was at times disturbed by air-raid warnings, it was successfully completed and the ampoules containing the radium were transported to a safe place.

N. P. Strakhov did much for the nuclear physics laboratory also after the war and now one cannot but recall him and his selfless work with great admiration.

I would like to add a few words of a completely personal nature. About the mysteriously glowing vessel containing radium, which was locked in a safe and from which a no less mysterious gas — the emanation — escaped through a thin tube, I heard already from my father as a school boy. He was a man with a broad education and, although he was a mathematician, he apparently knew of the work of Marie Curie. At the time I of course altogether did not think and in no way dreamed that I would be closely associated with it. Nowadays, all this probably no longer appears mysterious even to a school boy. The romantic aura has been dispelled, and we sadly remember those whose lives were shortened by radium, some of whom I named here.

Vavilov ascribed no less significance to the discovery of the neutron. He emphasized that this discovery destroyed the conception that the electric charge is an inalienable property of particles, which was until then generally accepted. Vavilov considered the explanation of the fundamental properties of this particle, and in particular its wave properties, extremely important. Neutron diffraction which has now become a working method for investigating the structure of matter was at that time still beyond the experimental possibilities; it became possible at a later date.

Ascribing considerable significance to an investigation of neutrons, he suggested to his graduate student N. A. Dobrotin to start work in this area. This resulted in Dobrotin's investigation with a cloud chamber of the angular distribution of protons knocked out by neutrons from a layer of parafin.[6] Few remember now that Dobrotin's work removed completely the contradictions which were then current concerning the angular distribution of neutrons scattered by protons—a fundamental problem of neutron physics.*

It is characteristic of the extent of Vavilov's scientific interests that during his stay in Italy in June 1935 he visited Fermi's laboratory in Rome and in a letter sent from there described in detail the first experiments on the direct measurement of the velocity of thermal neutrons.

The third nuclear-physics subject, which evolved just as naturally on Vavilov's initiative, was that entrusted to the graduate student P. A. Cerenkov. The problem was quite specific—to compare the luminescence mechanism of uranyl salts under gamma irradiation with that obtained under excitation with visible light and x rays. This task was successfully completed, [8] but now, of course, everyone is familiar not so much with this work as with the result of the new discovery made by Cerenkov in the course of the investigation of the gamma luminescence. I remember very well the significance which Vavilov attached even to the first results of Cerenkov. At the very beginning of the investigation, even before the first publication in 1934 [9], he was saying that Cerenkov measured the polarization of the emission and that against all expectations the preferred orientation of its electric vector is the direction of the gamma beam. If this is so, he would say, then the only explanation of this can be that the emission is in fact not due to the gamma rays, but that the source of the radiation are the electrons themselves which produce these gamma rays. Vavilov advised me to acquaint myself with Cerenkov and his experiments on the polarization of the emission, which I, of course, did. This was the first time I saw this emission and I convinced myself, of course, that Cerenkov's assertion concerning the sign of the polarization was correct.

Returning to Vavilov's letter, about which I have already spoken, I will note that it also mentions the plans of the remaining participants in the nuclear physics investigations during 1936. In it he mentions that "Dobrotin is preparing to think about a 'Fizeau experiment' with slow neutrons; Vernov will study cosmic rays; Cerenkov will study as before the emission under the action of gamma rays. With Skobel'tsyn we are coming to a final agreement." Here he had in mind the problem of the periodic visits of Dmitriĭ Vladimirovich Skobel'tsyn from Leningrad to Moscow, even prior to his transfer to the Physics Institute of the Academy of Sciences which was successfully effected at that time. V. I. Veksler is still not mentioned in this letter; he began working at the Physics Institute somewhat later and immediately joined the cosmic-ray investigations.* Summing up his statements about nuclear physics, Vavilov writes: "On the whole, I believe that the laboratory is on the right path and that in a year or two it will work out as it should."

However, the first steps in nuclear physics at the Institute were not easy. The Institute was often inspected and criticized. If the commission was bureaucratic, then it noted that inasmuch as nuclear physics is a useless science, there is no basis for developing it. In the discussions at the Academy of Sciences the reasons for criticism were different. No recognized authority is working here in nuclear physics, and of the young ones nothing will come. As a matter of fact, the only specialist in the field of nuclear physics, Professor L. V. Mysovskiĭ, who participated in the work of the laboratory during the Leningrad period, did not transfer to Moscow, and contact with him was gradually lost. Vavilov himself was criticized on account of Cerenkov's work. I remember very well the biting comments on the fact that at the Physics Institute work is being done on unwanted emission of who knows what under the action of gamma rays. At that time, a very deep conviction that nuclear physics is of fundamental significance and the entire authority of Vavilov were required to defend the development of nuclear physics at the Institute.

As regards the young physicists, they did in fact need help, and Vavilov's help was always very concrete. There would be the advice of a experienced experimental physicist, a man of unusual breadth of knowledge. At the same time, he would often recommend: "You should speak to this one," or "you should find out about the work of that one." His

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*Subsequently N. A. Dobrotkin and K. I. Alekseeva carried out a number of investigations on artificial radioactivity induced by neutrons. In particular, K. I. Alekseeva discovered a number of long-lived radioactive isotopes, for example, the long-lived isomer state of silver.[7]

*It is sad to realize that Vladimir Iosifovich Veksler is no longer with us. He passed away on September 22, 1966.

memory was exceptional and he always remembered who studied what and when; he had a splendid knowledge of the scientific literature, and not merely in his own specialty.

Of course, having started work on the study of pairs, we learned from D. V. Skobel'tsyn, long before he came to the Physics Institute, both the cloud-chamber method and the technique of working with gamma rays. On his advice we copied for our work a construction of the cloud chamber analogous to that worked out by Joliot-Curie (Fig. 2). As has already been noted, Vavilov took active care to ensure that such regular assistance was available to us. It is difficult to understand why there were so many who would consider the situation catastrophic and why it is often generally thought that scientists have to be led by the hand like small children. It is apparently to some extent the fate of all generations of physicists who start to work independently to hear the same remark: "Nothing will come of it!" Much success in life is in store for him who does not say "nothing will come of it," but is instead able to give advice which will direct the work in such a way that it will be successful. Vavilov was always able to give such advice, and this ability is by far more valuable than merely a favorable attitude.

In finishing my review of the initial stage of work in nuclear physics at the Physics Institute, I would like to add a few words about one more problem connected with it to some extent. At that time, flights into the stratosphere began in this country, and interest in investigations connected with high altitudes was increasing. Vavilov was the chairman of the Commission for the Study of the Stratosphere and organized a conference on this problem (in 1934).

In 1934 preparations started, on the initiative of my brother G. M. Frank, for the first high-altitude Elbrus combined expedition. This initiative was immediately supported by S. I. Vavilov and A. F. Ioffe and even in its first year a series of institutes of a most diverse type participated in the expedition. The first director of the expedition was the Professor of the Electrotechnical War Academy, A. A. Yakovlev. Then, for some years, the expedition was directed by G. M. Frank and V. I. Veksler. A group from the Physics Institute consisting of Dobrotin, Cerenkov, and Frank participated in the first year of the expedition's work. We carried out at that time the first observations of cosmic rays by means of a cloud chamber at various heights from 2000 m (Terskol) up to 4300 m (Priyut odinnadtsati). In addition, following a proposal of Vavilov's, we joined a group from the State Optics Institute consisting of Academician A. A. Lebedev and I. A. Khvostikov in a study of the glow of the night sky.[10] The working conditions, especially for studying cosmic rays, were at the time still most unfavorable. In order to decrease the radioactive background, we had to work directly on the ice of the glacier, even without a tent (Figs. 3 and 4). As a light source we used the sun, directing its light from a heliostat mirror into the cloud chamber. The cloud chamber nevertheless worked, and we even succeeded in obtaining photographs. This was the beginning of work on cosmic rays which was carried out in the Elbrus expedition in subsequent years mainly by Veksler and Dobrotin. Approximately at that time, Vernov employed the method of radiosonde balloons invented by Molchanov to observe cosmic rays.[11] Some years later, he went with a naval expedition to equatorial latitudes. As a result of this work, Vernov discovered the existence of a strong latitudinal cosmic-ray effect in the stratosphere.[12] I recall that during the discussion of this work at the Academy of Sciences, Vavilov defended the results obtained by Vernov from attacks with reference to foreign authorities who did not obtain such a result. I note that Vernov was accompanied on this expedition by N. L. Grigorov who is now a professor at the Moscow State University and who, at the time, was a laboratory technician at the Physics Institute. Of course, a large

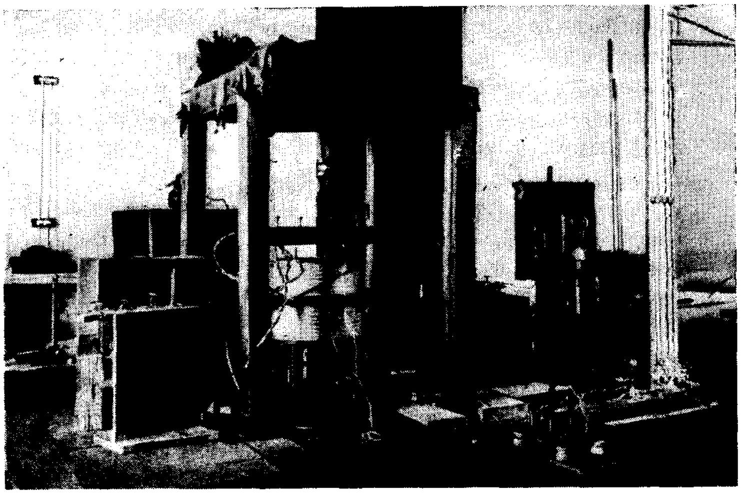

FIG. 2. Hermetic cloud chamber for investigating gamma-ray pair production in the gas.

FIG. 3. Rehearsal of the work with the cloud chamber under "field conditions" on the roof of the USSR Academy of Sciences in Leningrad in 1934. (On the left – N. A. Dobrotin, on the right I. M. Frank).

FIG. 4. Assembly of the cloud chamber on one of the glaciers of Elbrus.

distance in terms of years and standards of development of science and technology separates all this work from the cosmic station "Proton." Nevertheless, this was the beginning of the road which had to be traveled and the name of Vavilov who actively aided the investigations in their very first stages should not be forgotten. Vavilov's role in the development of nuclear physics did not, of course, end with this. It was also very considerable in later years. It is sufficient to say that he was the chairman of the council attached to the President of the USSR Academy of Sciences, which directed the coordination of work on the peaceful use of atomic energy. Here one could recall many aspects of his activity. However, I would like to touch, at least briefly, upon some contemporary problems of nuclear physics.

First of all a few words about the radiation discovered by P. A. Cerenkov. Vavilov, in his first paper,[9] called it correctly "blue luminescence," although it was absolutely impossible at the time to see its color. At such small intensities the eye is no longer capable of color vision. Now it is easy not only to see the color of this radiation, but to photograph it* and even to obtain in a pulsed reactor instantaneous color photographs. The luminescence of water in the reactor is readily seen, but there it is difficult to investigate it. Nobody even attempts this. I have often said[1] that looking at such photographs one cannot but think of what would have happened without Cerenkov's experiments based on the methods of analysis of the nature of the radiation developed by Vavilov. Would not even now the glow of the water in reactors be considered an inessential phenomenon, the result of some luminescence? After all luminescence is a very common phenomenon and there is nothing surprising in the glowing of the water.

I have shown a photograph obtained with a pulsed reactor intending to say a little about the problems of neutron physics, connected with the study of nuclear structure, for which the use of pulsed reactors is very fruitful.

A real nucleus is a system whose structure is complex, and here physics has encountered problems whose solution is unusually difficult. We can ask ourselves: is it right to study these problems at present when there is still so much that is not known about the problems of elementary interactions of individual particles essential both for the structure of the nucleus and for the nuclear forces?

Here, perhaps, an analogy with the work of Vavilov is to the point. In fact, giving due significance to the problems of atomic spectroscopy, he himself studied the optical properties of very complex molecules. He proceeded from the fact that the laws governing complex system may be specific for them,and, at the same time, because of their statistical nature might even be simpler than those of atoms. The success of his work showed that this is really so, and the procedure of going from a study of the simplest systems to complex ones is not always the shortest path. A somewhat analogous thing occurred in the study of the atomic nucleus. The cheerful hopes that the problem of nuclear forces will soon be solved by elementary particle physics, and that it will become possible to calculate the nucleus like the Bohr atom was calculated have not been justified. On the contrary, with the years the problem of elementary particles appears to have become more and more complex, so that the very term elementary particle already appears to some extent somewhat obsolete.

Naturally, without denying the timeliness of other parts of nuclear physics, in its fundamental significance, the problem of elementary particles remains, of course, as before the central problem of physics. However, whereas initially there was an attempt to

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*The original article includes an instantaneous color photograph of Cerenkov radiation obtained during a pulse of a powerful Triga-type pulsed reactor, and sent to the author from the USA by Dr. Mark, and a color photograph of the radiation of 660-MeV protons in a crystal of calcite obtained by V. P. Zrelov.[13a]

reduce the problem of nuclear structure completely to the problem of elementary particles, a different trend has now appeared; namely, to consider these problems quite independently. Both of these extreme points of view are unjustified. There exists, of course, a deep connection between these problems, but at the same time the development of the physics of nuclear structure differs in many aspects from that of the physics of elementary particles. Present-day nuclear theory has developed independent methods which make it possible to understand a great deal about the properties of nuclear matter.

Without touching upon all the problems of nuclear research, I would like to dwell on the problems of neutron spectroscopy. Here there are quite a few analogies with optical spectroscopy, since use is made of radiation that carries no charge and which excites the nucleus effectively; this is analogous to the action of light on the electron shell of atoms and molecules. Just like light, a slow neutron does not impart to the nucleus an appreciable momentum, whereas the excitation energy of a nucleus in neutron capture is large. However, unlike in the case of light, we have no prism by means of which one could resolve the neutron beam into an energy spectrum. The possibilities of the diffraction methods, if one goes outside the framework of thermal neutrons, are also limited on account of the short wavelength of the neutron. Therefore a basic method of neutron spectroscopy is the so-called time-of-flight method, whose principle is unusually simple. A pulsed neutron source is used, which emits at some instant a "white Spectrum" of neutrons. If one observes the neutrons at some distance from the source, then they will be resolved into a spectrum of times of flight—each neutron velocity will correspond to its own time during which the neutron will be in flight to the detector. If one observes some action of the neutron, for example the appearance of gamma rays in neutron capture, as a function of the time of flight, one determines thereby the dependence of the probability of this process on the neutron energy. Just as in any spectrometer one must, of course, take into account its resolution and luminosity which depend on a number of factors. The resolution is, obviously, the larger, the shorter the neutron flash, i.e., the more accurately the instant of emission of the neutron is given and the longer the flight path.

A comparison of neutron spectrometry with its optical counterpart is possibly also useful for understanding the significance of the former. In fact, optical spectrometry by far does not exhaust the methods of the physics of the structure of matter. However, everyone knows how many problems can and actually were solved by spectral methods.

This has some analogy in nuclear physics. Contemporary nuclear physics has at its command a wealth of various methods of investigation. Among these neutron spectroscopy, like optical spectroscopy, opens up in certain cases possibilities which are unattainable by other methods, and at times it is an essential supplement of the other methods. At the same time neutron spectroscopy is but one branch, not only of nuclear physics, but also of nuclear spectroscopy which also employs other methods.

I will speak mainly of the results obtained on the neutron spectrometer employed in the neutron physics laboratory in Dubna. A fast pulsed reactor of very peculiar construction[14] is used there. A special mechanism changes periodically its multiplication factor, rendering the reactor supercritical for a short period of time. As a result of this the reactor produces 40—60 μsec neutron pulses with a given repetition rate. Although it is also referred to as a pulsed reactor, its principle of construction and operating mechanism differ considerably from the usual pulsed reactors. In analogy with accelerators used for the same purpose, one should refer to it as "flashing." It generates periodically repeating radiation pulses. The large neutron flux makes it possible to use for the analysis of neutron velocities a flight path (i.e., a reactor-to-detector distance) of record length—1000 meters. For work requiring the greatest resolution, the reactor is recently being used as a pulsed multiplier of neutrons produced by a flashing injector accelerator. Photoneutrons from an electron accelerator—a 30-MeV microtron—are used as the injector. The principle of such an accelerator has been proposed 20 years ago by V. I. Veksler.[15] The accelerator found subsequently extensive application, a fact due mainly to the work of S. P. Kapitza.[16]

Figure 5 shows as an example one of the experimental curves obtained in Dubna by means of the setup of the fast pulsed reactor with the microtron with a flight path of 750 m. The curve shows the absorption of neutrons in germanium as a function of the neutron energy.[17] The gamma yield occurring in neutron capture is plotted on the ordinate axis, a quantity inversely proportional to the neutron velocity is plotted on the abscissa. On first sight the picture reminds one of that obtained with a spectrophotometer for optical spectra. Obviously, each peak on the curve should correspond to a given excited state of the nucleus. The majority of the resonances are excited states of the nucleus of the germanium isotope with the mass number 74. Figure 5 shows the results for relatively slow neutrons with an energy between 300 and 2000 eV, whereas the excitation energy of the nucleus is very large. A binding energy of 10 million 140 thousand electron volts is released in the neutron capture. In order to find the excitation energy, one must add to this binding energy the kinetic energy of the neutron, which is noted above each peak. It is, as a matter of fact, surprising that for such high-energy states of the nuclei the excitations are so narrow. The widths of the levels are here in all some tenths of an

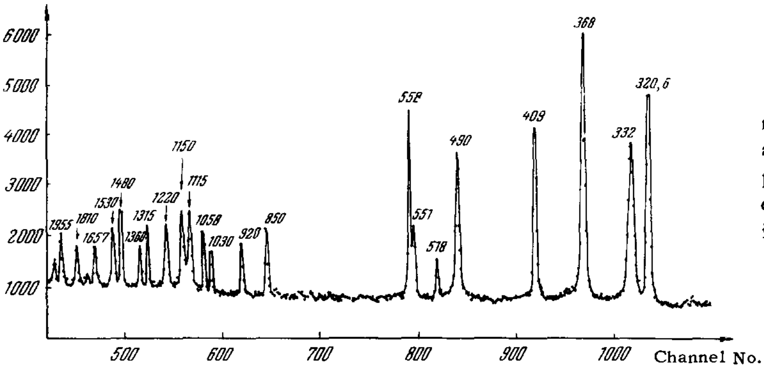

FIG. 5. Gamma-ray yield in neutron capture by germanium nuclei as a function of the neutron energy. Channel number – a quantity proportional to the time of flight, i.e., inversely proportional to the neutron velocity. Numbers – the kinetic energy of the neutrons in electron volts, corresponding to the individual resonances.

electron volt and this at an excitation energy above 10 million electron volts. One sees also that neutron spectrometry makes it possible to study the peculiarities of these spectra in all their details. In this respect this method has no equal.

Two peculiarities of such spectra are striking. First, the positions of the resonances do not follow any obvious law. Some levels are close and some widely separated. For instance, the 551- and 558-eV levels differ in energy by only 7 eV, and then there are no peaks until 850 eV, i.e., within an interval of 300 eV there are no appreciable resonances. A more detailed study of the positions of the levels also does not reveal any law with the exception of a statistical one. Nothing like the spectral lines of the optical spectra has, at least thus far, been observed.

The second peculiarity consists in the fact that the resonances can be of quite different intensity. There are very large peaks and also very weak ones, for example at 518 and 558 eV. The magnitude of the resonance is determined by two factors: the probability that a neutron will be captured by a nucleus, and the probability that this will be followed by emission of gamma rays. It is possible to determine each of these probabilities separately—they are characterized by quantities called the neutron and gamma width of the resonance. It turns out that the probability of gamma emission, i.e., the gamma width of the resonance, is almost constant for various resonances of the nucleus. This means that the time of emission, i.e., the duration of the gamma luminescence excited by the neutrons does not depend on the neutron energy. As a rule, the gamma spectrum also does not change very appreciably. We have here a full analogy with the photoluminescence of complex molecules investigated by Vavilov's school, where the duration of the emission and its spectrum are within wide limits independent of the photon energy.

Quite a different matter is the probability of neutron absorption. It has turned out that the neutron width changes from level to level very strongly and at the same time irregularly. We cannot explain why the neutron width of a given resonance is large or very small, but we can determine experimentally the statistical law governing the distribution of the resonance widths. Under certain assumptions, the theory makes it possible to predict what distribution should be expected and experiment apparently confirms this. The data so far are not exact and not very complete, but nevertheless this is a real success attained through the work of numerous physicists.

It is so far not clear whether one can aspire here to anything more than a purely statistical result. In any case, one must attempt to understand the features of the motion of particles within the nucleus with which such a variable susceptibility of the nucleus to excitation by neutrons is connected. To this end, one must clarify whether the neutron width is not related to any other properties of the resonance. So far this search has not been reliably successful, but in-

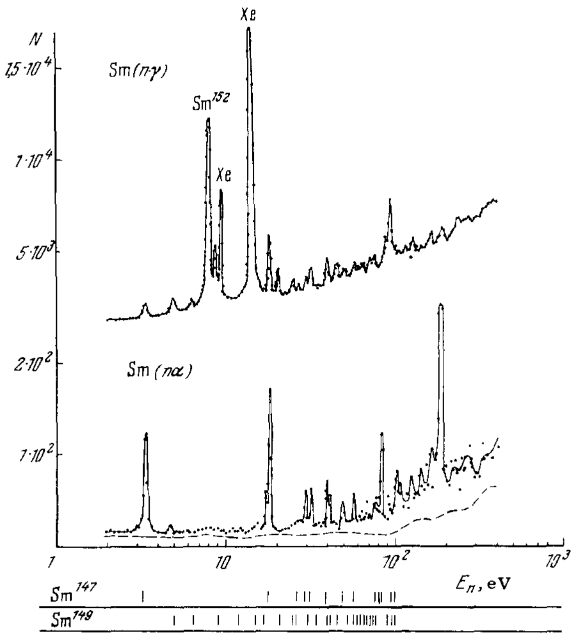

FIG. 6. The graph shows the dependences of the probability of the interaction of neutrons with samarium isotopes on the neutron energy. The upper curve is analogous to that of Fig. 5. It shows the gamma yield as a function of the neutron energy. The lower curve shows the probability of alpha-particle emission from the same nuclei. The course of the curves is completely different, i.e., the probabilities of gamma and alpha emission by excited samarium nuclei are not proportional to each other.

teresting attempts have been made. Popov and Kvitek[18] investigated the absorption of neutrons by samarium and neodymium nuclei. The peculiarity of these nuclei consists in the fact that in this case in the neutron capture there takes place, in addition to gamma emission, emission of alpha particles. The probability of this is negligibly small. One alpha particle is emitted for about one million captured resonance neutrons. Nevertheless, it was possible to measure this probability (Fig. 6). The hope is that from the alpha activity, one of the most investigated processes, one can obtain much information about the states of the nucleus from which the decay occurred. The first results are interesting, but it is too early to say to what extent these hopes will be justified.

Even from what has been said, it is clear that the interpretation of the results of neutron spectrometry is complex. One should bear in mind the following. A slow neutron does not excite all states of the nucleus, but only the part characterized by completely defined quantum numbers; these are different in different nuclei. For example, the germanium resonances which I have shown have quantum characteristics, namely spin and parity of 5^+ or 4^+; for samarium they are 3^- and 4^- etc.* Nor is the excitation energy imparted to various nuclei on capturing a neutron identical. As a result, the nature of the neutron spectra in different nuclei differs even qualitatively. The density of resonances of some nuclei is high—the average spacing is of the order of 1 eV, in the case of others they can be hundreds of times more sparse. It would seem that the characteristics of such spectra are not comparable. This is not so. If we go back to the quantity which is referred to as the neutron width, and we sum the neutron widths of the resonances located in a given, sufficiently large, energy interval, and the obtained sum is divided by the size of this interval, then the average obtained in this manner will have a quite definite value commonly referred to as the strength function (so-called reduced widths are used in the definition). It turns out that in any case the strength function depends little on the density of levels. If the levels are sparse, then their neutron width is on the average large, if there are many levels, then the width of each is on the average smaller. Thus the sum of the widths for nuclei with close atomic weights remains almost constant.

Sum rules are also known in optical spectra and their existence is connected with very general laws. The average characteristic of the resonances, the strength function should also be a direct manifestation of very general properties of nuclear matter. It is important that here we already have some knowledge. One of the most remarkable achievements of nuclear physics is the so-called optical model of the nucleus. This model came about quite independently of slow-neutron spectrometry and is based on another type of experiment—namely the study of the scattering of fast neutrons and protons by nuclei. This model ascribes to nuclei a completely defined and almost identical index of refraction and absorption coefficient for neutron waves. One can thereby predict the average neutron absorption by nuclei, the difference in the absorption depending on the different dimensions and shapes of the nuclei. It follows from the theory, and to some extent this is obvious, that the predictions of the optical model and the magnitude of the strength function should be directly related with each other. Experiment shows that in the so far not very numerous instances when a comparison is possible, the agreement is fully satisfactory.

It is hence seen that in determining average quantities, neutron spectroscopy has in fact achieved a great deal, and that the measurement of these quantities is essential for nuclear theory. In particular, the nuclear laboratory of the Physics Institute has contributed appreciably to the determination of these average quantities by means of an original spectrometry by the slowing down time of neutrons in lead.[19]

However, all these results refer to average quantities whose accuracy is so far not very high. It would be most surprising if, when they are refined, no other more subtle laws were discovered. Data are available, but they are still insufficient for such an analysis. One of the difficulties is that in most cases we do not know the quantum characteristics of the observed resonances. For example in the neutron spectrum of germanium we do not know which of the resonances are characterized by the quantum number 4^+ and which by 5^+. We do not know, thus, that which is referred to as the elementary nature of the emitters which Vavilov studied in optics.

The work of Vavilov and his school showed that a powerful means of investigation is the use of polarized light. This is true to the same extent for both polarized neutrons and polarized nuclei. However, this approach was not developed, because, unlike in the case of light, we had no good polarizer of a neutron beam in the region of the resonance energies. As a result of the work of F. L. Shapiro and his co-workers the problem of obtaining such a polarizer has now been successfully solved.[20] I will show the first result obtained for the holmium nucleus (Fig. 7).[21] The lower curve is the absorption spectrum of the holmium nucleus when unpolarized neutrons are used. The dips are the absorption lines of the holmium nucleus. The upper curve shows the difference in the transmission of a sample of polarized holmium nuclei measured for neutrons with the two signs of polariza-

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*In the capture of a slow neutron (a so-called s-neutron) by the nucleus the spin of the neutron which is ½ is added to or subtracted from the nucleus, and the parity of the state does not change. The neutron excites thus resonances of the same parity as the ground state of the bombarded nucleus, differing from it in its spin by +½ or −½.

tion. Depending on which of the two kinds of neutrons is absorbed, this difference is positive or negative. The dips correspond to resonances of holmium with the quantum number 4, and the peaks to resonances of holmium with the quantum number 3. For a number of resonances this difference is visible at a glance.

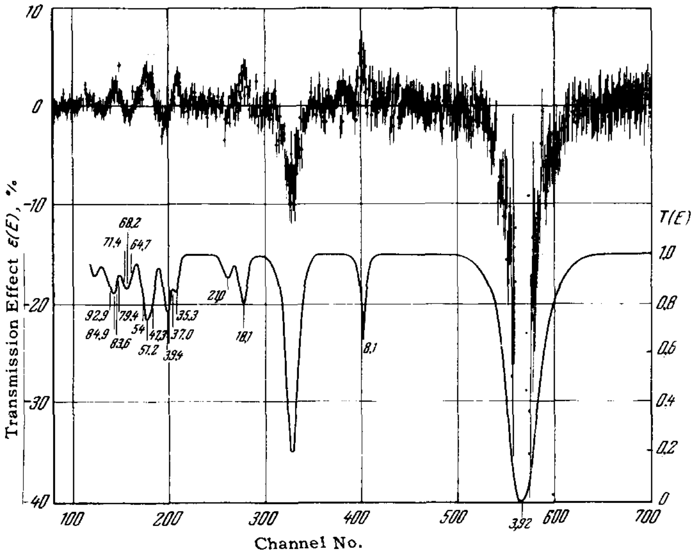

FIG. 7. Interaction of neutrons with holmium nuclei. Lower curve – transparency of a holmium target as a function of the neutron energy. Each resonance corresponds to a sharp increase in the neutron absorption, i.e., a dip on the transmission curve. The upper curve gives the difference between the relative transmission of neutrons with both signs of the polarization with respect to the direction of polarization of the nucleus. When neutrons polarized in the same direction as the nucleus are absorbed, then the difference is negative and a dip occurs on the curve (for example, at an energy of 3.92 eV). If neutrons with spin directed in the opposite direction are absorbed, then the difference is positive (for example, the 8.1-eV resonance). The first case corresponds to the addition of the spins of the nucleus and of the neutron, and since the spin of the holmium nucleus is 7/2, the spin of the resonance is 7/2 + 1/2 = 4. In the second case, 7/2 – 1/2 = 3.

I would like to add a few more words about one problem connected with neutrons in which much progress is being made at present. We know quite well the first excited states of the nucleus. Neutron spectrometry provides information about states above the binding energy of the neutron. However, we know little about what occurs in the intervening region—a vast interval extending over ten million electron volts. Here, too, one of the methods of investigation is neutron physics. After a neutron is captured, gamma rays are emitted. They appear as a result of transitions to a multitude of levels, occurring precisely in the region which is not easily accessible. The number of levels occurring here is enormous. The study of this spectrum is difficult. A very considerable step forward in the experimental methods and subsequently in the methods of investigation was made by L. V. Groshev and his co-workers. I will show the results obtained for nickel.[22] On first sight, the pattern of the gamma spectrum is similar to that observed in the case of neutron spectra; however, the scale is quite different (Fig. 8). Whereas there we were dealing in electron volts and hundreds of electron volts, the lines observed here lie in an interval of about three million electron volts. This is a comparatively simple case. The pattern is often more complex. And, as a matter of fact, it is altogether not trivial that there are such simple cases. Many questions arise in connection with the fact that in the emission of gamma rays considerable preference is given only to transitions to certain levels. What is the nature of these levels and how are they located?

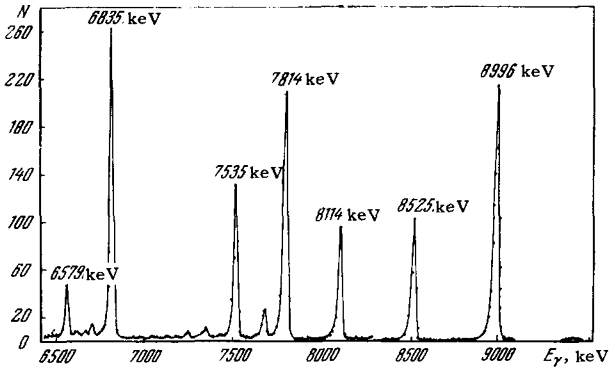

FIG. 8. Gamma spectrum in the capture of thermal neutrons by nickel nuclei.

The analysis of these spectra is already possible and for this reason the data of Groshev's group are to an ever increasing extent passing from the stage of accumulation of results to the stage of their interpretation.

I have encompassed in my lecture the history of two periods of work in nuclear physics separated by about three decades. I think I have acted in the spirit of the tradition of Vavilov, who always noted succession in the development of science. Not without reason is his book on the history of the Physics Institute of the Academy of Sciences, which I have mentioned, entitled "The Physics Study, Physics Laboratory, Physics Institute of the USSR Academy of Sciences During 220 Years."

In my lecture I have turned several times to analogies with optics investigations. We know that the work of Vavilov's school transformed the study of luminescence, which was in a descriptive stage, into an exact study of laws of transformation of light. As regards neutron spectroscopy, its problems have already long ago passed the stage of simple determination of neutron constants essential for practical purposes. Its concern is more and more with the study of the mechanism of processes. Nonetheless, if we continue to compare it further with the study of luminescence, then as far as the possibilities of analysis are concerned we are at present somewhere at the stage Vavilov's work was in the Twenties. And, as usual, progress in present-day nuclear physics is the collective task of physicists of many laboratories.

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