A conceptual history of the Vavilov-Cherenkov radiation

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Usp. Fiz. Nauk **143**, 111–127 (May 1984)

The evolution of ideas on the nature of the Vavilov-Cherenkov radiation is discussed. The period between Vavilov's ideas, advanced in 1934, and the formulation of a quantitative theory of the phenomenon in 1937 is surveyed.

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1. INTRODUCTION

Nineteen eighty four will be the fiftieth anniversary of the publication of the original papers by P. A. Cherenkov[1] and S.I. Vavilov.[2] The work reported in these papers must be regarded as fundamental to the discovery.[1)] The first experimental data were discussed by Vavilov,[2] who showed convincingly that the universal radiation emitted by liquids exposed to γ rays, which was discovered by Cherenkov, was essentially different from luminescence. The ideas advanced by Vavilov were very important both for the development of further experiments and for their subsequent interpretation. Thus, the search for an explanation of the nature of the phenomenon, begun by Vavilov in his first paper, proceeded in parallel with experimental studies. The theory of Tamm and Frank[4] was the outcome of these researches and was based both on the original data and the subsequent experiments by Cherenkov.[2)]

The author of these lines happened to take part in numerous discussions with Cherenkov and Vavilov. Sometimes, these discussions took place in unusual circumstances. Cherenkov's experiments demanded complete adaptation of the eyes to darkness, which meant that about an hour had to be spent in complete darkness before measurements could begin. Vavilov sometimes used this hour to consider experimental results and to discuss plans for future experiments. It so happened that, on some occasions, I was with Cherenkov in the darkened room. Sometimes he needed assistance in measurements and, when no one else was available, I undertook these duties. Thus, my participation in the original work on the elucidation of the nature of the Vavilov-Cherenkov radiation was facilitated by the fact that I had an intimate knowledge of what was going on at all times. This was not difficult to achieve in such a small and closely knit scientific community as FIAN was at the time. There is no doubt that I. E. Tamm was also familiar with Cherenkov's experiments, and my constant discussions with him definitely helped in this. He was not immediately interested but, as soon as his interest was aroused, our collaboration began. This review is a summary of the development of our understanding of the Vavilov-Cherenkov radiation as it evolved in the interval between the publication of Vavilov's first paper and the appearance of the Frank-Tamm theory.[4]

I tried to recall my collaboration with Tamm, and some of its consequences, in an earlier paper[5] devoted to his memory. The first part of that paper is reproduced with some editorial alterations in Sections 3 and 4 below, but I can now see some gaps in that account. One of the intriguing questions is: why was it that the new form of radiation had not been discovered before 1934 despite the fact that an emission induced by radium rays had been seen as far back as the beginning of this century by Pierre and Marie Curie who did not regard it as significant?[3)] The emission was often seen subsequently, and one must ask why many of its unusual properties had not been discovered. Of course, this was not fortuitous. Luminescence phenomena were widespread and well known. Many forms of luminescence were described, and the presence of radio luminescence among them would not have been surprising. We now know that it was, in fact, present. An enormous amount of experimental data had accumulated over the three hundred years of observation of luminescence, and a review of this work occupies an entire volume of the Encyclopaedia of Experimental Physics. However, the experiments were mostly concerned simply with establishing the fact, the source, and the conditions of luminescence. The emission under the influence of radium did not give rise to any difficulty in this context. Studies of the nature of luminescence and of the methods for its investigation began to develop in the 1920's, and most of the credit for these advances is, of course, due to Vavilov and his

[1)]The papers by P. A. Cherenkov and S. I. Vavilov[1,2] were received by the editor of Doklady Akademii Nauk SSSR on 27 May 1934 and were published on 11 June 1934.

[2)]We shall be referring to the original papers by P. A. Cherenkov. The results were summarized by him in his doctoral thesis.[3]

[3)]Marie Curie has described how she and Pierre Curie wanted radium, which they discovered, to have an attractive color. Once, having entered their famous laboratory in which they isolated radium, they were fascinated by the blue emission from test tubes containing the radium. I think that, even in our time, anyone who has seen the mysterious blue glow of radium will never forget the sight. This is hardly the Vavilov-Cherenkov radiation alone, but it undoubtedly contains it.

 0038-5670/84/050385-11$01.80

school. It was indeed Vavilov who suggested that one of the basic features of luminescence was the finite duration of emission, determined by the lifetime of the excited state of the radiating atom or molecule. This lifetime is usually quite short (10^{-7}–10^{-9} s) and could not be measured before the late 1920's. It is therefore clear that the discovery made by Vavilov and Cherenkov was not actually greatly delayed relative to the time at which it could have been made. The participation of Vavilov in this was not fortuitous either: he knew how to investigate luminescence, and was well aware of how it differed from other types of emission. Another question then arises: why was this phenomenon not predicted and looked for as happened, for example, in the case of transition radiation? As a matter of fact, the prerequisites were in place, but it did not happen. This, too, will have to be commented upon.

2. PREHISTORY OF THE DISCOVERY

The theoretical paper by Sommerfeld,[6] brought to our attention by A. F. Ioffe, was mentioned in a footnote to our earlier paper.[4] The analogy between Sommerfeld's theory, which was concerned with the retarding force on a charge moving with velocity greater than the velocity of light in a vacuum, and the Frank-Tamm theory was examined by Tamm[7] in 1939. Vavilov found that Sommerfeld's work was also anticipated by Lord Kelvin,[8] who pointed out in 1901 that an atom traveling in a vacuum with velocity greater than that of light should produce an electromagnetic wave similar to the Mach wave in acoustics. Kelvin's suggestion is cited in my later papers, where it is also noted that, in reality, an atom, i.e., an uncharged system, traveling with velocity greater than that of light, should typically emit not the Vavilov-Cherenkov radiation, but radiation resulting from the spontaneous excitation of the atom (anomalous Doppler effect). The question of how it could possibly have happened that Kelvin's suggestion was totally forgotten was also discussed. In 1961, I wrote about this as follows:[10] "Any pronouncement by a major physicist such as Kelvin could not have been forgotten purely by accident. In fact, his prediction contained an important error that became clear soon after. This arose because Kelvin did not take his analogy with acoustic waves to its ultimate conclusion. The point is that elastic waves are possible only in a material medium filling some volume in space. In a certain range of wavelengths that depends on its properties, the medium can be considered to be continuous. The situation is precisely the same for the Vavilov-Cherenkov effect. Here the essential requirement is that the electromagnetic waves must propagate in a medium, and the theory is formulated on the assumption that the medium is continuous. Moreover, the medium is characterized by macroscopic parameters (permittivity and permeability) that are functions of the frequency of light and determine the wave propagation velocity and absorption in the medium."

"We now know that the analogy between the Vavilov-Cherenkov effect and Mach waves is, in fact, valid. However, in Kelvin's time it was extremely difficult to justify. Light was said to propagate in a medium called the ether which was endowed with rather peculiar elastic properties. It was natural to seek an analogy between the ether waves and the elastic waves in a medium. But there was no reason to analyze the motion of particles in a dense medium, the more so since the motion of an atom in such a medium did not seem physically realistic."

After the advent of the theory of relativity, the idea of the ether had to be abandoned and it became clear that velocity greater than that of light in vacuum could not be attained, so that Kelvin's suggestion and Sommerfeld's paper were, to some extent, justifiably forgotten. However, this cannot be said about an even earlier and very striking anticipation of modern theory. It has recently become clear[1,12] that, as far back as 1888, Heaviside considered a purely imaginary situation in which a point charge traveled through a dielectric with velocity greater than the velocity of waves, and obtained the well-known relation between the characteristic angle of emission of radiation propagating with the velocity of light in the medium and the velocity of the particle. This is what A. A. Tyapkin[11] wrote about it in 1974: "Recently, in the course of an examination of the paper by O. Heaviside[14] entitled "On the electromagnetic effects due to the motion of electrification through a dielectric," I found that it contained a section specially devoted to the motion of a charge q moving with velocity v greater than the velocity of light u in the dielectric. The author makes the following fundamental conclusion":

$$\sin\theta = \frac{u}{v}».$$

"The question now suggests itself: What is the state of things when $v > u$? It is clear, in the first place, that there can be no disturbance at all in front of the moving charge (at a point, for simplicity). Next, considering that the spherical waves emitted by the charge in its motion along the z-axis travel at speed u, the locus of their fronts is a conical surface whose apex is at the charge itself, whose axis is that of z, and whose semiangle θ is given by $\sin\theta = u/v$." The point charge was considered by Heaviside only for the sake of simplicity, since the electron had not yet been discovered, i.e., the point charge had not been known and nothing was known about the existence of fast charged particles (nothing was known about radioactivity or cosmic rays either). Thus, Heaviside was indulging in a pure "thought experiment" that, however, was striking by its farsightedness. At first sight, it is difficult to understand why Heaviside was writing about the motion of a charge in a dielectric rather than in a vacuum. However, he supplied an answer to this, which actually demonstrates the depth of his understanding of the problem:

"To avoid misconception I should remark that this is not in any way an account of what would happen if a charge were impelled to move through the ether at a speed several times that of light, about which I know nothing; but an account of what would happen if Maxwell's theory of the dielectric kept true under the circumstances, and if I have not misinterpreted it."[4)]

[4)]These phrases by Heaviside are taken from Kaiser's paper[12] who, in turn, cites from: O. Heaviside, Electrical Papers, Macmillan, London, 1892, Vol. 2, p. 49. Kaiser points out that Heaviside's first paper[13] on the subject appeared in 1888. This problem obviously worried him and he frequently returned to it.

[4a)] See Kaiser's paper.[12]

Thus, as early as 1892, Heaviside was being cautious about applying the theory to velocities greater than the velocity of light in vacuum. His approach was consequently more penetrating than that of either Sommerfeld or Kelvin. It was therefore not fortuitous that his thought experiment was concerned with the motion of a charge with velocity exceeding the velocity of light, but only in a dielectric. Heaviside was not opposed to electrodynamics, but he did qualify his discussion by pointing out that it was valid if Maxwell's theory of dielectric was valid. This remark also deserves further attention, and I shall return to it later.

In 1888, the above thought experiment was so far removed from reality that it could not have attracted the attention of experimentalists. Several decades had to elapse before detailed studies of the passage of fast particles through matter could begin. On the other hand, specialists in radioactivity and luminescence were well insulated from theoretical problems in electrodynamics. Narrow specialization in physics had already become commonplace. The consequences of Maxwell's theory were still largely the province of theoreticians who were probably not too interested in the potentialities of experiments. Even a theoretician such as I. E. Tamm, the author of an excellent textbook, "Fundamentals of the Theory of Electricity,"[15] first published in the 1920's, totally ignored Heaviside's suggestion. It was therefore not surprising that Heaviside's work was not mentioned either in connection with Mallet's experiments[16] (they will be discussed later) or in connection with Cherenkov's experiments. It was definitely not a stimulus to new experiments. On the other hand, it is not clear why Heaviside himself did not consider the possibility of experimental detection of the effect predicted by him, especially since β rays had already been discovered by Rutherford and were being investigated. The last publication of Heaviside's papers, made in his lifetime, was produced in 1922 by which time quite a lot was known about fast electrons. One can only assume that his concerns were by then too far removed from this new area of research.

We must now say a few words about Heaviside's doubts, mentioned above, concerning the validity of the theory. It may be that, in some way, these doubts prevented Heaviside from seeking an experimental confirmation, but, at any rate, they prevented him from obtaining the final Tamm-Frank formula even though he came pretty close to it. In 1892, he said that the general idea that he put forward was "of sufficient likelihood, but I cannot find a solution that would satisfy all the necessary conditions."[4a] What stopped Heaviside from obtaining the essentially elementary formula for the energy of the emission that was finally found by Tamm and Frank? I already had occasion to write that, sometimes, points of view that change with time and vanish without trace can give rise to difficulties that are subsequently hard to understand.[5)] It seems to me that this was precisely the situation here. In actual fact, Heaviside did not take into account the dispersion of light. He characterized the difference between fields in the dielectric and in vacuum by the permittivity ε which he considered to be a constant. It is now hard to understand why he did this. On the other hand, this was quite natural, and was the normal procedure at the time when the field due to a charge was considered., (However, this did not prevent the use of a refractive index for light of a given frequency when the field of an electromagnetic wave was considered.) In order to pass to modern ideas, we must isolate the time-dependent field component, i.e., expand the field into a Fourier integral. This is not at all a trivial operation if we recall that the size of the moving charge is constant and we consider uniform rectilinear motion. To obtain the spectrum of the field produced by the charge, we must proceed as in our original paper, i.e., represent the charge density by the δ function $\rho = e\delta(z - vt)\delta(x)\delta(y)$, and then expand this function into a Fourier integral. Surprisingly, Heaviside knew how to use a function essentially the same as the δ fuction but, in this case, he clearly did not consider this to be important. Moreover, it was necessary to deduce from this the component E_ω and to use the vector $\mathbf{D}_\omega$ expressed in terms of the permittivity ε_ω at the given frequency ω. Unless the frequency-dependent permittivity ε_ω is introduced, one arrives at a number of contradictions. The Vavilov-Cherenkov radiation cone then corresponds to a discontinuity in the field which becomes infinite on the surface of the cone. Moreover, radiation losses and the force retarding the motion of the particle become infinite. As Heaviside himself pointed out, "there can be no disturbance at all in front of the moving charge." It seems to me that these and similar difficulties were appreciated by Heaviside when he said that he could not find a solution satisfying all the necessary conditions.

By the time our 1937 paper was published, the first step that had not been taken by Heaviside was already made, and the field due to a moving charge in a vacuum was commonly expanded at the point of observation into a Fourier integral as a way of calculating the slowing-down of fast particles by atoms.[6)] The second point, namely, that the frequency-dependent permittivity ε_ω had to be used, was not new since the work of Lorentz but, even in 1937, it still had to be justified. This is given by Eqs. (2) and (3) and the preceding discussion in our original paper. In our draft of that paper, this was done in still greater detail. The argument was as follows. Since we consider the electric field component E_ω, we have to take into account the dynamic component of polarization P_ω of the medium. This, in turn, is the sum of the polarizations p_s of the atoms with proper frequencies ω_s, the number of which is N_s. We then have

$$\frac{\partial^2 p_{s\omega}}{\partial t^2} + \omega^2 p_{s\omega} = \frac{e^2}{m} E_\omega N_s, \tag{1}$$

i.e.,

$$p_{s\omega} = \frac{e^2}{m} E_\omega \frac{N_s}{\omega_s^2 - \omega^2}; \tag{2}$$

and hence

$$P_\omega = \sum p_{s\omega} = \frac{e^2}{m} E_\omega \sum_s \frac{N_s}{\omega_s^2 - \omega^2}. \tag{3}$$

We note that this is essentially different from the polarization in a stationary field, $P = \alpha E$, where α is a constant.

On the other hand, it was known that

$$D_\omega = n_\omega^2 E_\omega = E_\omega + 4\pi P_\omega \tag{4}$$

5) See my previous paper.[5]

6) The significance of this analysis is discussed in my previous paper.[5]

and this introduces n_ω into the Maxwell equation. Although this now seems obvious, it needed an explanation not only in the last century, but even in the 1930's. It is thus clear that, in actual fact, Heaviside was virtually unable to advance beyond a qualitative picture of the effect. And this was the point at which Tamm and Frank began to develop their quantitative theory. On the other hand, until the expression for the energy of the radiation became available, we considered it premature to publish our paper. Heaviside's doubts as to the validity of the qualitative argument occurred to us as well. It is thus clear that, although Heaviside was well ahead of his time, he was still unable to take the solution of the problem to its final conclusion.

Mallet[16] is often credited with the experimental discovery of the Vavilov-Cherenkov effect. We now know that he did actually observe the Vavilov-Cherenkov radiation, but can this be regarded as a discovery? Mallet established two properties: firstly, that the radiation was possibly universal, since the emission was observed to be produced by not one but several liquids and, secondly, the spectrum of the emission was continuous and was the same in all the cases that were investigated. However, this could have been completely explained by the luminescence of the same (even though slight) impurity in the liquid. It is well-known that a similar blue emission due to an impurity is observed in liquids exposed to ultraviolet light and was investigated by S. I. Vavilov.[7)] It was natural to try to establish experimentally whether this was the case or not. On Vavilov's advice, Cherenkov immediately proceeded to the experiment and, in a paper published simultaneously with Cherenkov's publication Vavilov suggested that this type of emission was not, in fact, luminescence. Mallet, on the other hand, was engaged in standard experiments on luminescence that were confined to the description of the phenomenon. He noted its unusual character, but did not try to explain its origin. Could he have done so? Undoubtedly, he could. Vavilov's work on luminescence was widely known, and Mallet must have been familiar with Perrin's work in France. Mallet must therefore be credited with an observation rather than a discovery. In point of fact, the discovery was difficult to make. Mallet used the photographic method to record the radiation, and it was not a simple matter to perform a quantitative determination of intensity. Many prolonged exposures had to be made and the characteristics of the photographic plate had to be known. However, the fact that the luminescence could not be quenched, and that the radiation had unusual polarization, was undoubtedly discovered by Mallet.

In contrast to Mallet, Cherenkov used the quenching method developed by Vavilov and Brumberg in the early 1930's, in which the measured intensity was compared with the visual threshold which is remarkably constant for each dark-adapted observer. This calls for considerable effort on the part of the experimenter (who has to put up with long periods in the dark and with eye strain), but produces quantitative results in a relatively short period of time. There was no other quantitative method at the time. Vavilov's program for investigating luminescence and his quenching method ensured final success in this case.

[7)]This is discussed in my review paper in Usp. Fiz. Nauk.[9]

3. DISCOVERY BY VAVILOV AND CHERENKOV

We shall now try to summarize the situation at the beginning of 1936, which was a decisive period for the understanding of the phenomenon. Much light was thrown on this in the original 1934 papers of Cherenkov[1] and Vavilov.[2] In the course of his studies of the luminescence emitted by solutions of uranium salts under γ-ray excitation Cherenkov discovered a weak visible radiation emitted by the solvents, the nature of which was unclear in many respects. This was the beginning of the study of radiation emitted by pure liquids under excitation by radium γ rays (ordinary luminescence could not be eliminated in the case of solids). The radiation turned out to be universal. All liquids, without exception, were found to emit this radiation, and the intensity was the same to within 30%. The use of light filters revealed that the spectrum emitted by different liquids was the same to within experimental error. It covered a broad range of frequencies and, whenever its color could be seen (there is no color vision at low intensities), it appeared to be blue. And although color could not be seen at the time, Vavilov[9] correctly entitled his paper (published simultaneously with Cherenkov's first paper) "On a possible reason for the blue emission by liquids irradiated by γ rays."[8)]

The papers of Cherenkov and Vavilov are essentially two parts of the same paper: one experimental[1] and the other theoretical.[2] Moreover, we have already noted that the experimental part was mostly concerned with the implementation of the program of measurements that was typical for the luminescence studies performed in Vavilov's laboratory (Cherenkov was Vavilov's graduate student). The result was, however, unexpected, and it was decided to continue the study of this emission.

It was particularly surprising that the radiation was appreciably polarized and the direction of the electric-field vector lay preferentially in the direction of the γ rays. This sign of the polarization and the fact that the radiation intensity could not be varied by varying the temperature, or by adding a luminescence quencher, were reliably established even in Cherenkov's first paper.[1] This led Vavilov[2] to the following important conclusion: the emission cannot be interpreted as luminescence by excited molecules of the liquid, but is due to a Compton electron which radiates as a result of its interaction with the medium. The only seemingly possible mechanism for this process was bremsstrahlung. This was, in fact, proposed by Vavilov. The assumption immediately explains the universality of the emission and the polarization, since in the Compton effect the electron is preferentially emitted at an acute angle to the direction of the photon beam. There was also no problem with the fact that the spectra emitted by different liquids were more or less the same, since the spectrum was obviously determined by the retarding mechanism.

[8)]Vavilov then did not know that the spectrum of the emission had already been established. As we have already mentioned, its photographs were obtained by Mallet[16] (1926–1929). Nowadays, the blue glow of water is usually shown to visitors to nuclear reactors.

If we adopt this point of view, certain other unavoidable conclusions follow. The fact that the radiation is polarized indicates not only that the emission of light is due to moving electrons, but also that the emission is determined by the initial segment of the path and, mostly, by high-energy electrons (the electrons are highly scattered in the liquid, especially the slow electrons, and their motion ceases to be directional). It was possible to suppose that the effect as a whole was confined to the time of ejection of the electron by the γ rays (double Compton effect). From the classical point of view, the effect is determined by the sudden acceleration of the electron at the time of its emission. If the energy of the second photon is small in comparison with the electron, the intensity of this radiation can be readily calculated within the framework of classical electrodynamics and, as we now know, it turns out to be much lower than the observed intensity.

However, already then experiment clearly showed that the emission originated mainly from a portion of the path traversed by the electron after it was ejected from the atom. Actually, the number of Compton electrons produced in a liquid should be proportional to its density, whereas the range is inversely proportional to the density. If the emission originates at the time of ejection of the electron, it should be determined by the number of electrons, i.e., it should increase in proportion to the density. This is definitely not so. On the contrary, the intensity is independent of density, and this shows that both the number of electrons and their range are important. It follows that it is the electron itself that radiates during its motion, and the dependence of this on the energy of the electron had to be elucidated.

For Compton electrons with energies in the MeV or even keV range, the energy is still very high in comparison with the energy of photons of visible radiation (eV range). There was therefore no basis for considering that the probability of emission per unit length was greater for fast than for slow electrons. The reverse could, in fact, have been expected, since the probability of scattering, and hence of deceleration, increases with increasing velocity. Experiment, on the other hand, is clearly in favor of fast electrons. In his first (1934) paper, Cherenkov reports the results of x-ray experiments which demonstrated that the universal emission was absent[1] when the voltage across the x-ray tube was 32–34 kV. I think I am right in saying that, in 1936, there was clear evidence for the fact that, in the case of γ rays, low-energy photons, at least, do not provide an appreciable contribution to the emitted radiation. It actually turned out that, when the γ rays were passed through a lead filter, the emission was attenuated in accordance with the same law as the hard component of the radium γ rays, although the intensity (i.e., the energy absorbed by the medium, which is roughly proportional to the total range of the electrons) of the hard and soft components was[9)] in the ratio of 1 to 3. Of course, it would have been premature to conclude from this that the emission had a threshold (this would have seemed paradoxical), but there was no doubt that fast electrons played a preferential role in the excitation of the emission. This presented a difficulty for the bremsstrahlung hypothesis.

Another difficulty was the absence of an appreciable dependence on the atomic number Z of the atoms in the liquid. For example, when the measurements were performed on $_6C[_{17}Cl]_4$ and $[_1H]_{14}[_6C]_6$, the emission intensity was practically the same (1.04 and 1.09, respectively, in relative units). Vavilov himself[18] came quite close to an explanation of the phenomenon by supposing that, during its motion, the electron experiences small perturbations due to its interaction with the atoms and, since the distance between them is constant on average, the perturbations must be quasiperiodic. When the electron velocity has a suitable value, this can give rise to the emission of visible radiation. We now know that this type of effect does, in fact, occur during bremsstrahlung in crystals (it was considered by Ter-Mikaelyan[19] independently of Vavilov). It became clear later (after the development of the quantitative theory) that the weak point of the bremsstrahlung hypothesis was that the resulting intensity was relatively too high as compared with the observed emission. In a footnote to Tamm's paper, it is stated that "the intensity of visible light should then be lower than the intensity measured by Cherenkov by a factor of 10^4." The optical part of the bremmsstrahlung spectrum has not actually been adequately investigated even now, so that this theoretical estimate is not entirely reliable (in preparing our paper for republication, I have removed this footnote). Nevertheless, it may be concluded that the contribution of bremsstrahlung to the emission observed by Cherenkov was, in fact, negligible. The importance of qualitative results will be clear from the foregoing. The radiation yield was first measured by Cherenkov,[26,27] but there were no theoretical predictions for this prior to the appearance of our paper (Ref. 4).[10)] Thus, Vavilov's bremsstrahlung hypothesis was unsatisfactory in many respects right from the start, and gave rise to progressively increasing doubt. However, I had no doubts about his suggestion that it was *not* luminescence by the liquid but radiation by the electron that was responsible for the emission. This was not at all generally accepted, and Cherenkov's work attracted no interest outside the small circle of people associated with Vavilov. It became clear that direct demonstration of the connection between the radiation and fast electrons were essential before further progress could be made.

Of course, the most direct approach was to observe the emission from a source of β particles. This may now seem strange, but it was not a very simple matter at an institute which did not have a radiochemical laboratory. The experiment was performed in 1936, using a preparation of radium in a thin-walled glass ampoule.[17] It was shown that the radiation had all the properties established for it when it was emitted under the influence of γ rays. Moreover, as expect-

[9)]Unfortunately, neither Cherenkov nor I can now remember with any degree of certainty when this experiment was performed. However, in a paper completed in December 1936 and published in the issue of Dokl. Akad. Nauk SSSR containing the paper by Tamm and myself, Cherenkov refers to unpublished experiments in which the significance of the hardness of γ rays mentioned here was established.[17] It would therefore appear that these results had already been obtained and became topical in connection with our paper.

[10)]The problem of bremsstrahlung and its relation to the Vavilov-Cherenkov radiation is not as elementary as it originally appeared to us. It was partially examined by Tamm in 1937.[7] Several of the related questions are discussed in the second part of Ref. 5.

ed, the intensity produced by the β particles turned out to be inversely proportional to the density of the liquid, i.e., proportional to the electron range. Cherenkov even makes an attempt to compare the results with theory, which indicated that there should have been a dependence on the refractive index as well. I shall return to this point later.

A further important advance was made at the beginning of 1936 in the course of an indirect experiment in which the role of electrons was verified: quite by chance, a characteristic feature of the emission, namely, its directionality, was discovered. This could easily have been missed in experiments with electrons because the necessary collimation of the electron beam was not easy to achieve at the time. The aim of the indirect experiment was to show that, when the emission was produced under the influence of γ rays, the polarization was essentially connected with the direction of motion of the electrons. Clearly, this could be verified by placing the emitting liquid in a magnetic field that was strong enough to transform the rectilinear path of the electrons into an appreciably curved arc of a circle. The resulting plane of polarization should have then rotated through an angle in the direction in which the electrons were deflected.

I recall that we carefully discussed with Cherenkov the plan of the experiment and, later, the results. There was some suggestion that the electrons could not be controlled by the magnetic field because of considerable scattering. However, the experiment was not at all pointless: on the contrary, it was successful, but the result was unexpected. Neither Cherenkov nor I can now recall the original plan of the experiment, but we clearly remember the results. When the magnetic field was turned on, the main effect was not the rotation of the plane of polarization (which apparently did occur), but a change in the intensity of the emission, which was considerable.

This could have been explained by a number of different hypotheses (Cherenkov writes about the proposals that were considered in his 1936 paper[20]). However, it was noticeable even in the first experiment that both the magnitude and sign of the change in intensity depended on the angle to the γ-ray beam and the direction of the magnetic field. It was therefore natural to assume that the emission exhibited angular anisotropy and that, when the direction of motion of the electrons was rotated by the magnetic field, the angular distribution of the emission was also rotated. This could have been anticipated because any polarized emission (dipole or any other) is not spherically isotropic. However, the size of the effect would have been expected to be so low that it could hardly lie within the range of precision of the experiment. At the same time, the radiation flux emitted by any point multipole should be symmetric with respect to the center of the multipole (i.e., it should not vary when the wave vector is rotated through 180°). Careful examination of the experimental results, on the other hand, led to a different and actually paradoxical conclusion: the two opposite directions were not equivalent for the emission, and more light was emitted into the forward hemisphere relative to the direction of motion of the electron. This directionality must have been considerable because, when the electron was deflected by the magnetic field toward the observer, the intensity was found to rise appreciably, whereas deflection in the opposite direction was accompanied by a fall in intensity. I recall that Cherenkov agreed with this conclusion. It seems that we readily accepted it because of our relative unfamiliarity with optics. Vavilov, on the other hand, who had a deep understanding of optics, considered that this conclusion could not have been correct. Direct experiment soon showed, however, that the emission was actually asymmetric. The tube containing the liquid was sealed at its ends with flat mirrors, which enabled us to observe emission in the two opposite directions. When the radium source was placed near the center and alongside the tube, the emission observed through the two windows was found to have the same intensity. When the magnetic field was turned on, the intensity observed through the window toward which the electrons were deflected was found to be higher, whereas that observed through the other window was lower.[20]

4. THEORY OF THE PHENOMENON

Of course, even then, it was well known that bremsstrahlung emitted by relativistic electrons was concentrated in the forward direction and, indeed, it was natural to consider that it was this that was being seen in the above experiments (the analogy with bremsstrahlung is mentioned in Cherenkov's paper). However, Vavilov maintained (basing this, if I recall correctly, on Sommerfeld's work) that, for low photon energies, bremsstrahlung should not be concentrated in the forward direction. In fact, not a single directional source of visible radiation was known at the time, and it was considered that this was not fortuitous. It is now difficult to explain the basis for this mistaken view, which appears to have been more or less generally accepted. Whereas, today, the discovery of radiation emitted predominantly in a particular direction would probably be regarded as evidence in favor of the bremsstrahlung hypothesis, at the time, it served as a stimulus to searchers for another explanation that would result in the correct understanding of the phenomenon. In point of fact, the only way to ensure that radiation will be emitted mostly in a particular direction would be to have a source of radiation with linear dimensions comparable with the wavelength. A radiator of this kind can be looked upon as a set of point multipoles that are mutually coherent and distributed over a certain length. This is precisely the way the directed emission of radio waves is achieved. This is why, when I reported to Tamm on the conclusion drawn from Cherenkov's experiments, he immediately responded with, "This means that coherent emission is taking place over an electron path length that is comparable with the wavelength of the light wave." This statement is correct, but now that we have a good understanding of the idea of the so-called coherence length, we would be more cautious in drawing any particular conclusions from the mere fact of mostly forward-directed emission. At the time, however, this statement was very helpful to the development of the well-known picture in which the emitted waves combine at an acute angle to the direction of motion of the electron (nothing was known experimentally about this angle at that particular time).

Although this very graphic explanation of the nature of the phenomenon is now standard in all popular textbooks, it may be useful to say a few words about it. The basis of this explanation is, or course, the Huygens principle: each point on the path of a uniformly and rectilinearly moving charged particle is a source of a spherical wave that is emitted when the particle of velocity v passes through it. When

$$v > \frac{e}{n}, \quad \text{i.e.} \quad \beta n > 1, \tag{5}$$

these spheres have a common envelope in the form of a cone whose apex coincides with the instantaneous position of the charge. The angle between the normal to the cone generators, i.e., the direction of the wave vectors, and velocity of the charge is given by

$$\cos\theta_0 = \frac{1}{\beta n}. \tag{6}$$

The explanation of the Vavilov-Cherenkov effect is often confined to this simplified presentation but—and this was done right from the outset—the picture must be constructed for monochromatic waves. We must therefore consider the expansion into a frequency spectrum of the light pulses emitted at times t' at which the particle passes through the successive points along the path. The frequency spectrum thus turns out to be continuous, and the wave are emitted at all times, but their phase is not arbitrary but equal to $\omega(t-t')$. If we consider the wave surfaces corresponding to a given t, we again obtain the conical envelopes (surfaces of equal phase), and again arrive at condition (2), but for $n = n(\omega)$. It is clear that there is an infinite number of such cones, and only the angle θ_0 that depends on $n(\omega)$ is specified.

A great deal has emerged from this qualitative picture. In point of fact, only fast electrons, for which $v > c/n$, can radiate in this way. The radiation emitted by an electron must be proportional to its range, i.e., inversely proportional to the density of the liquid. Hence, in accordance with experiment, the resultant intensity of the emission for electrons produced by γ rays should not depend on the density (we recall that the number of Compton electrons produced per unit volume is approximately proportional to the density). Finally, this picture predicted that the emission should be directional. As already noted, at the time, Cherenkov's experiments led only to the conclusion that there was more radiation in the forward than in the backward direction. Few people are now aware that the magnitude of the angle θ_0 was not a consequence of the experiment; on the contrary, this was a theoretical prediction that was subsequently fully confirmed by experiment.

It is clear from a quantitative analysis that the spectrum of the emitted radiation must be continuous because the only restriction on the frequency appears through $n(\omega)$ in (5) and, for a transparent liquid, the refractive index is a slowly-varying function of frequency in the visible range. It also seemed probable that the electric field vector of the radiation should be determined by the direction of the velocity of the electron, and thus yield the correct sign for the polarization. Thus, provided only this type of composition of waves resulted in real emission, there was no doubt that the phenomenon was universal in character.

It follows that this qualitative picture provided an explanation of everything that was known about the Vavilov-Cherenkov radiation except for its intensity. It is this that made it extremely attractive. I had the opportunity to share these ideas with a number of theoreticians that were beginning to show interest in Cherenkov's experiments (especially after the directional character of the emission was explained), but they showed no understanding of what was going on. The main reason was probably that they had inadequate familiarity with the properties of the phenomenon. Both Tamm and I knew much more.[11)] Tamm even considered the publication of a paper without waiting for a more detailed analysis. However, this would have been premature. Not only was the problem of intensity an open question, but the very possibility of the emission soon became subject to some doubt. Tamm spoke to L. I. Mandel'shtam about the qualitative interpretation of the emission. Mandel'shtam's response was as follows: it is known that an electron in uniform rectilinear motion does not radiate. The conclusion remains in force when the velocity of light c in the wave equation is replaced with c/n because the one equation immediately leads to the other when the velocity of the particle is changed correspondingly. I was not present at this discussion, but it appears to have been rather brief and, at any rate, no mention was made of the fact that the conclusion is not valid for velocities exceeding the phase velocity of light, i.e., something that cannot be achieved in vacuum.

Mach's experiments with supersonic bullets were, of course, familiar to both Tamm and myself. I cannot now remember whether the analogy with Mach's waves was ignored or whether it was considered that the analogy was not valid in electrodynamics. Either way, the situation now seems more than strange. One way or the other, Mandel'shtam's remark, made "off the cuff", had the effect of considerably reducing the apparent attraction of the qualitative point of view. After this incident, Tamm considered that, before this approach could be developed further, it was necessary to establish whether the phenomenon could not be explained in some other way. On my part, I tried to modify the picture so as to remove a contradiction that was not actually present. The problem remained unresolved between the spring and fall of 1936.

In those years, the situation was dominated by the quantum-mechanical approach to the solution of problems involving the emission of radiation by fast particles. At the same time, a paper by Williams,[21] which laid the foundations for the method now frequently referred to as the pseudophoton method, attracted considerable attention and was frequently discussed among theoreticians. Williams' method was a development of the beautiful work of Fermi (1924) and Bohr (1915), and my attention was drawn to it by D. V. Skobel'tsyn.

Williams considered the time dependence of the electric field due to an incident particle at a given point characterized by the impact parameter. The time dependence of the field was represented by an expansion into a continuous frequency spectrum, and the effect of the field at each frequency ω of this spectrum on an atom or nucleus at the given

[11)]Of course, this applies to Vavilov, as well. With his characteristic physical intuition, Vavilov approached this idea with a lively interest, and expected it to develop further.

point was determined. As far as our particular phenomenon was concerned, the question was: how can a field transported by the particle become a source of waves diverging from each point on the particle trajectory? Following Williams, the problem could be solved by finding the small interaction between the particle and the atoms and nuclei lying along its path, the resulting oscillations of which become the sources of the waves. My attempts to justify the qualitative picture began with searches for a mechanism that could produce this transformation of the particle field in the diverging waves. In modern language, this was an attempt to construct a microscopic theory of the Vavilov-Cherenkov effect.

This situation was typical of the time. When the interaction between fast particles and matter was considered, it was rigidly believed that no approach other than the microscopic was possible. The energy of the particle (γ-ray photon or β particle) was definitely high in comparison with the binding energy of an electron in an atom or the binding energy of atoms in molecules, liquids, or solids. The inescapable conclusion from this seemed to be that atomic structure could be neglected during the interaction of fast particles with matter. The existence of some effect that would depend on the refractive index seemed paradoxical from this point of view. It is likely that this is one of the reasons why the qualitative interpretation of Cherenkov's experiments, which necessarily involve the refractive index, immediately gave rise to disbelief. As far as I was concerned, I had a firm belief in this picture, but was also subject to the common confusion of trying to find a microscopic mechanism for the emission of the waves. It seemed that if uniform motion in an optically homogeneous medium did not produce the emission, then a microscopic mechanism was apparently necessary. The difficulty for the theory as it looked to me at the time was that Cherenkov did not find an appreciable dependence of the emission intensity either on Z or on the refractive index even though (6) clearly showed that the cosine of the angle of the cone should have been equal to $1/\beta n$. Whilst my attempts remained fruitless for several months, Tamm appeared to abandon the problem altogether. On the other hand, Cherenkov's experimental researches, stimulated by the suggested directionality of the emission, were rapidly advancing. Directionality soon became an experimentally established fact. It is now difficult to imagine how surprising this result seemed to be at the time.

I remember that when, in the fall of 1936, Joliot-Curie arrived in Moscow, the Cherenkov experiment, which is now in every popular book, was demonstrated for him. A vertical, cylindrical, glass vessel, filled with a liquid, was surrounded by a conical mirror. By looking at the mirror from the top, one could see the angular distribution of the radiation leaving the glass walls of the cylinder in the horizontal plane. The radium source was placed to the side of the cylinder, and one could clearly see two emission maxima at an acute angle to the direction of the γ rays.[26] Cherenkov's photographs of these rings with nonuniform distribution of blackness with the azimuth angle are now well known, and the experiment itself is easy to understand and unfailingly successful provided, of course, one does not suspect that elementary error bordering on sharp practice is taking place. This thought apparently occurred to Joliot-Curie, who immediately tried to rotate the vessel and mirror around the common axis in order to check that the transparency of the glass or the quality of the mirrors did not have an effect. In a discussion of the experiment, he hinted on an analogy with the Blondlot N-rays.[12)] This is not surprising. The experiment was demonstrated in complete darkness and the emission was at the limit of visibility even for partly dark-adapted eyes. The entire arrangement was, in fact, unusual for a physical experiment and recalled something in the nature of a spiritualist séance or a sleight-of-hand trick.

This experiment was preceded by an interlude during which the theory was still incomplete but the importance of the problem was obvious. This led to a new discussion in which Tamm participated. Various hypotheses, now long forgotten, were considered, and all were found futile. It became clear that the graphic picture founded on the Huygens principle was the only one that gave the qualitatively correct result. Both the quantity $\beta = v/c$ and the range of the most energetic Compton electrons did actually produce the required directional emission of the waves at an acute angle to the velocity of the electron. After this or, probably, after these discussions (I don't recall now how many of these there were), Tamm telephoned me late in the evening and asked me to call on him immediately at this home.

I found Tamm at a table, absorbed in work, with a sheaf of papers covered with formulas in front of him. He at once told me how far he had gotten prior to my arrival. I can no longer remember the details of our discussion that went on right through the night. I think that we discussed the course of the solution proposed by Tamm and the validity of the various intermediate steps, as well as the physical basis of the theory, which was still not entirely clear. I only remember that we sat there for a very long time. I returned home early in the morning on foot as it was too late (or too early) for public transport.

On my way out of Tamm's house, I grabbed an exercise book in which Tamm wrote out in his own hand the derivation of the formula for the energy emitted by an electron. By some quirk of fate, this exercise book has survived. Tamm's writing occupies five and a half pages, inscribed by a careful hand and bearing many corrections. Even so, judging by the fact that some of the intermediate steps are omitted, this could not have been the original version of the derivation, but an attempt to systematize the results. I have already published a photocopy[5] of the page containing the final formula. The following pages of the exercise book, probably written later, more accurately, and with all the details, contain this derivation written in my hand. The final formula in the exercise book is correct (except for the limits of integration), but its derivation is essentially different from that given in our subsequent paper. In accordance with experiment, it was assumed that the particle's path length was limited, the particle velocity was constant within the limits of this length, and

[12)]The misguided experiments of Blondlot, who thought that he discovered a new type of radiation, are now totally forgotten. However, his N-rays were used as a kind of catch phrase at the time. Whenever some kind of "mystery" instead of real phenomena appeared as a result of some mistake, it used to be referred to as being due to N-rays!

the path itself was rectilinear. The field was calculated for the wave zone.

Since the calculation was confined to a trajectory of limited length, we were able to avoid the apparent difficulty that "an electron in uniform motion does not radiate." However, the final result bore no indication that the beginning and the end of the trajectory were significant. Moreover, the result showed, in accordance with (6), that there was not only coherent addition of waves in the direction corresponding to the angle θ_0, but also that these waves actually carried energy. Some further time had elapsed before it was explained why an electron moving uniformly in a homogeneous medium should actually emit radiation at all frequencies for which (5) was satisfied, i.e, for $\beta n > 1$.[13)]

In our published paper (dated 2 January 1937), we extended the derivation to the case of emission by an electron transversing an unlimited path, and removed the invalid assumption of small θ_0 (pointed out by Mandel-shtam). The paper therefore contains a calculation of the energy flux per unit length of the side surface of a cylinder whose axis is the trajectory of the particle. The main reason why we abandoned the case of a limited path length was that we had to prove that the emission of radiation was possible even for uniform rectilinear motion.

I have relatively little recollection of this concluding stage in the development of the theory, or of the writing and editing of the final paper, probably because this was the usual humdrum work. The only exception is the memory of a seminar at which Tamm read our paper immediately after the first results were obtained. In the course of the discussion, it became clear to both of us that the requirement of a limited path length for the electron was meaningless and that we either had to acknowledge that the electron radiated along its entire path, including the beginning and the end, or the whole thing was wrong, which seemed unlikely. This was, in fact, the stimulus to a correct understanding of the problem (Tamm recalls this seminar in his Nobel Prize lecture).

The analysis of the field in the wave zone, given in the original version of the theory involving a finite path length, turned out to be useful. This analysis (minus some of the original mistakes) is given among many other interesting results in Tamm's 1939 paper dedicated to Mandel'shtam, in which he also discusses the validity of the results.[7] I have frequently used this discussion.

The theory[4] turned out to be in complete agreement with experimental data obtained by Cherenkov by the middle of 1936. Additional experiments performed by him in 1936–37 verified the quantitative predictions of the theory as well. By using the conical mirror that we have already mentioned, he carried out an approximate measurement of the angle θ_0 and its dependence on the refractive index, thus confirming[25,28] the expression given by (6). He subsequently determined the energy yield and found it to be in agreement with theory to within experimental error.[26,27]

Cherenkov's results and their theoretical interpretation were initially noted only by Soviet physicists. It seems that foreign scientists rarely read our journals (although Doklady Akademii Nauk was published both in Russian and in a foreign language). In 1937, after the nature of the phenomenon became clear, Vavilov sent a paper by Cherenkov, which took the form of a brief summary of results and their comparison with theory, to "Nature." I can no longer remember what the polite excuse was, but the paper was rejected. However, there was no doubt as to the true reason for the rejection: a journal as solid as "Nature" was unable to publish results that seemed, at the very least, doubtful. In this sense, "Physical Review," to which the paper was sent after the lack of success with "Nature" turned out to be less selective.[28]

A new experimental confirmation of the theory appeared soon after. Collins and Reiling,[29] working in the United States, used a beam of relativistic electrons produced by an accelerator to verify the relations $\cos\theta_0 = 1/\beta n$ for a thin radiator. It may be that these workers regarded Cherenkov's paper without the disbelief prevalent at the time, since they assumed that the radiation was due to the gradual retardation of the electron by ionization losses, which eventually produced directional emission. This was a natural mistake if we recall our own confusion, mentioned above, and the fact that Collins and Reiling were familiar with the theory of the phenomenon apparently only to the extent to which it was given in Cherenkov's paper[28] (this contained only a reference to the theory, the results of which were reproduced only to the extent to which they were necessary for comparison with experiment). Collins and Reiling appeared to be the first to use the phrase, "Cherenkov radiation—a designation now generally accepted.

As far as theory is concerned, this was first developed by Ginzburg,[22] who gave a quantum-mechanical analysis of the phenomenon (1940) and extended the theory to optically anisotropic media (1940).[30] An important generalization was made by Fermi (1940), who considered a light-absorbing medium and showed that polarization of the medium was essential for the correct magnitude of ionization loss.[31] The flow of theoretical papers on the Vavilov-Cherenkov radiation has continued without interruption ever since.

Looking back, it is not uninstructive to recall that emission by a fast electron in a medium was actually the first example of a coherent self-luminous source of light of length much greater than the wavelength of light. The discovery of this type of coherence is now commonly attributed to another example, namely, that of the laser, in which it is actually very clear. On the other hand, this coherence was emphasized even by the very title of the paper by Tamm and Frank.[14)] It was later exploited in interference between light

[13)]This statement is valid only for an optically isotropic medium. In an anisotropic medium, the direction of the phase velocity is not the same as the direction of a ray. Condition (6) for θ_0 is satisfied, but the minimum angle θ_0 is, in general, greater than zero. It was shown in 1958 that the threshold condition given by (5) requires a correction. For the emission to appear, it is necessary for the particle velocity to exceed the phase propagation velocity along the ray parallel to v. Although the Vavilov-Cherenkov effect in crystals was first examined by Ginzburg as far back as 1940,[22] the properties of the threshold did not emerge for several years. They can be very simply explained[23,24] by applying the Huygens principle to crystals.

[14)]It was entitled "Coherent emission by a fast electron in a medium."

emitted by two thin radiators traversed by a fast charged particle (1944).[32]

The Vavilov-Cherenkov effect was the first example in which it became clear that the optical properties of the medium were just as fundamental to the emission process as were the parameters of the fast particle (such as charge and velocity). It subsequently turned out that there was a wide range of phenomena involving emission of radiation by a fast particle that were determined by the optical properties of the medium or for which these properties were important.

We have already mentioned that the question of the energy yield presented a difficulty in the development of the theory. In point of fact, for a given ω, the amount of energy emitted per unit length is proportional to $^2\theta_0 = 1 - (1/\beta^2 n^2)$. A change in the refractive index n is accompanied by a change in the effective magnitude of $\sin {}^2\theta_0$, and the difference in the yield should be fully detectable for the liquids investigated by Cherenkov. However, he found that, to within experimental error, the emission intensity was the same for different liquids. The explanation of this turned out to be simple. He actually measured not the emission yield but the intensity emitted in a particular direction, and this undergoes a change when the radiation is refracted at the liquid-air separation boundary (the solid-angle element is increased by refraction by a factor of n^2).[15)]

The factor $1/n^2$, introduced by Cherenkov at my suggestion into the expected dependence of the emission yield on the refractive index n, ensured that the yield became almost independent of the refractive index in those cases that he investigated. In reality, the effect is somewhat more complicated, and depends on the conditions of measurement. The whole problem was subsequently resolved experimentally.

Measurements performed with the integrating-sphere photometer eventually verified the expected dependence on the refractive index.[27]

I have mentioned the importance of refraction because Cherenkov radiation is always observed outside the medium in which it is produced (for example, even now, when one looks at the emission produced in water in a reactor). Since visual observation was the only method available at the time, this was an important point. I recall in this connection that my 1946 review article[9] includes a footnote based on my own experience which I hope no one is going to try and repeat (at least on purpose): "The only exception is indeed the emission produced in the liquid filling the human eye. This emission can be clearly seen by bringing up the γ-ray source close to the eye in darkness."

It is possible, however, that this experiment is being repeated involuntarily in a less dangerous manner. Thus, it has been reported that astronauts can see flashes of light with eyes closed. Is it possible that these flashes are due to the Vavilov-Cherenkov emission in the interior of the eye, produced by dense cosmic-ray showers or multiply-charged cosmic-ray particles?

Returning to the past, I note that one of my disappointments, discussed with Tamm at the time, was that I could not see any possible application of the radiation discovered by Vavilov and Cherenkov. One of the possibilities that was considered was that this emission could be produced in the atmosphere by cosmic rays. It was found, however, that the contribution of this radiation to the observed emission of the night sky was very small (I considered this in 1934 in collaboration with Cherenkov and N. A. Dobrotin). It was also a hopeless task to try to observe flashes due to cosmic-ray showers entering the eye because of the small area of the pupil (it is possible, in principle, that such flashes could be seen in a telescope, but such observations are impeded by stellar light providing a constant background). Observations of cosmic-ray flashes became possible only many years later, after the advent of photomultipliers, but this could not have been foreseen.

I shall not examine here the present-day situation in relation to the application of the Vavilov-Cherenkov radiation in different branches of physics, or the substantial contribution made later to various generalizations of the theory. As was pointed out at the beginning, this review is devoted to the first basic steps in the development of the theoretical understanding of the Vavilov-Cherenkov radiation.

[15)]Under the conditions of Cherenkov's experiment, the angular distribution of emitted radiation should have been anisotropic and dependent on the refractive index n. Furthermore, a fraction of the emisson, also dependent on n, did not leave the liquid because of reflection. All this produced a complication of the final result.

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Translated by S. Chomet