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Igor' Evgen'evich Tamm

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This centenary provides an occasion to recall once again the unique character of Igor' Evgen'evich Tamm, interesting for a number of reasons. My intention is to reminisce about Igor' Evgen'evich as a theoretical physicist of exceptionally high stature and about his work. I shall describe the condition of science at the time he was working, and the relationship of Tamm's work to the development of the whole of our physics, which has grown in his lifetime from a provincial level to that of high science that has gained world recognition. My intention will also be to include some personal reminiscences.

Igor' Evgen'evich was born on 8th July 1895 in Vladivostok, but during his early childhood the whole family travelled the long way via Japan (the Trans-Siberian Railway had not yet been completed) to Elisavetgrad (renamed Kirovograd by the Soviet Government). Here, his father became a city engineer: he built an electric power station, a water supply system, etc., and he managed all these establishments. His position ensured that the family was not rich but comfortable. Here, Igor' Evgen'evich finished high school in 1913 and even before that he decided on his main life's interest: science, and more precisely, physics. He was so serious about it that when in the summer of 1913 he went to study at Edinburgh University, he was sufficiently prepared to ignore physics lectures and to attend only laboratory sessions. He started his mathematics directly from the second and third-year courses. In addition, he studied cursorily chemistry and languages, and was engaged in social activities. By December 1913 he passed an examination on the second-year mathematics. He wrote as follows in a letter to his future wife Nataliya Vasil'evna Shuiskaya: "I am therefore studying not engineering, but pure mathematics. During the third term (April – June) I will take a course on the theory of oscillations in physics and probably also logic and introduction to philosophy". In fact, for some reason he was listed formally as studying engineering and he complained: "It looks silly that I am studying to be 'an engineer'. My interest is in pure science only and a practical worker I will never be . . . At this stage I cannot change from engineering to university [science]. I will try to do it next year".

However, as in Russia, he was equally interested in social problems, which he called 'politics'. He attended political meetings and gatherings of socialists, became acquainted with the life of the poor, read Russian literature forbidden in his own country, and studied Marx's *Kapital*. Life was of great interest to him in all its aspects. He became closely acquainted with students from India and other countries. He joined a socialist students' circle and the Fabian Society. He earned some money by teaching Russian at courses of foreign languages (complaining that "this took up an enormous amount of time") and was preoccupied with many other matters.

His own decisive judgement, particularly sharp in his youth, led Igor' Evgen'evich to this estimate of his teachers: "There is no point in attending the lectures: one professor presents the subject at a level so elementary as to be funny, another slips up . . . so that† the students correct him. In my opinion, only one professor deserves to be heard".

Tamm was going to return to Russia and he asked about the possibility of joining the 'Moscow Technical College' (he asked whether his year in Edinburgh would be counted), but after his return at the beginning of summer 1914 he nevertheless joined the Physicomathematical Department of Moscow State University. This determined the whole of his professional life.

This is not the place to describe all the remarkable events in the life of Igor' Evgen'evich, and the characteristics of his personality, which manifested during his university years and in the years of the Civil War.‡ He completed his studies at the University at the end of 1917 and only then he moved sharply away from politics (in 1917 his political activities were so intense that he was even a Elisavetgrad delegate to the first All-Russia Congress of Soviets; his political affiliation was with Menshevik Internationalists, i.e. he was quite close to the Bolsheviks. However, the October Revolution largely repelled him from the Bolsheviks).

The next few years were wasted in his scientific career. True, for a time he lectured at Simferopol University, where he associated with many remarkable scientists (physicist Frenkel, mathematicians Smirnov and Krylov, biologists Gurvich and Lyubishchev, and others). Then, in 1921 he went to Odessa to Mandelstamm (Mandel'shtam), who

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† In his letters, Tamm abbreviates the Russian words 'kak' ('as') and 'tak' ('so') to 'kk' and 'tk', obviously to save time. Even when old, he walked rapidly in small steps as if he tried to overtake himself.

‡ See, for example, a collection of papers entitled *Reminiscences about I E Tamm* (Moscow: Nauka, 1st edition 1981, 2nd edition 1986, 3rd edition, all in Russian). The third edition is supplemented by materials which, for a number of reasons, have been hitherto forbidden (in English: published by Nauka, Moscow, 1987).

later became his lifetime elder (by 16 years) friend and one can say a teacher. Before that the disorderly years of the Civil War were filled with much travelling, frequently very dangerous because war fronts had to be crossed.

Only in 1921 in hungry and cold Odessa did Tamm really began his scientific activity. It is a suitable place now to depart from the biography and say a few words about the position of physics in our country at the time.

Before the Revolution, our physics was on the whole weak. In chemistry there were such names as Mendeleev, Butlerov; in mathematics there were Lobachevski (Lobachevskii), Ostrogradskii, Chebyshev, Markov, Lyapunov; in physiology we had Sechenov, Pavlov, and Ukhtomskii. However, in physics after Lomonosov at the beginning of the nineteenth century, there was a soon-forgotten Petrov, then Lents and Yakobi, the inventor of radio. Popov shone briefly, and among the most important were Umov and Stoletov, little known in the West. The really remarkable physicist, Lebedev, had just became active. The point to stress is that Lebedev was not just an excellent experimentalist, the first to observe and measure the pressure of light (which was work probably of Nobel Prize level). He was a man who was always full of physics ideas and the first who, at the beginning of the twentieth century, founded in Russia a physics school of the same nature as those in Western Europe. He fostered his pupils (Vavilov, Andreev, Arkad'ev, and others). However, in 1911, together with over a hundred other professors of Moscow University, Lebedev left it as a protest against the reforms of the Minister of Education Kasso, who greatly limited the traditional university autonomy in order to suppress revolutionary student movements.

This action (as usual in Russia, the intelligentsia did not distance itself from the liberal movements and from social and political problems) generally bled white the University and was suicidal for university physics. A year later, Lebedev died of a heart disease at the age of 46 and physics at the University withered, falling far behind the world level. Igor' Evgen'evich recalled that the theory of electricity was presented by a certain professor who on reaching the Maxwell equations announced that this was a very complex theory and he would not deal with it. A few more serious young lecturers (Andreev, Landsberg, and others) did not set the tone.

The situation in St Petersburg was different: in 1907 – 1912 an outstanding Austrian theoretician, Paul Ehrenfest, was working there because there was no place for him in his own country. A regular theoretical seminar organised by Ehrenfest had an enormous influence on the formation of a group of young theoreticians and theoretically educated experimentalists, which became obvious in full measure later, after the Revolution. The remaining physicists of the older generation were as conservative here as in Moscow. The exception to the rule was Khvol'son, whose five-volume course of physics not only appeared in Russia in the form of several editions which were being continuously revised, but was also translated abroad.

One could say that if Russia was 'pregnant with revolution', then Russian culture was 'pregnant with big science'. In physics there was a researcher Eichenwald, who demonstrated experimentally the equivalence of the convection and conduction currents (Eichenwald effect), there was Fedorov who classified the crystallographic symmetry group, and there was Friedmann (Fridman), who, however, perhaps should be regarded as an applied mathematician. Friedmann had just made (1922 – 23) an outstanding discovery in physics: he found nonstationary solutions in Einstein's cosmology and thus proved the possibility that the Universe might be expanding. However, the absence of scientific schools was the problem, with the only exception of the school of Lebedev, which soon fell apart.

Almost all the important physicists of the pre-Revolution generation (with the possible exception of Umov) in their youth studied for many years and worked abroad, almost exclusively in Germany which until Hitler's destruction of science was an undoubted world leader in natural sciences. For example, Lebedev studied under Kundt, Kohlrausch, and Helmholtz; Ioffe studied under Rontgen; and Eichenwald, Papaleksi, Rozhdestvenskii, Andreev, Golitsin graduated from German or Swiss universities or at least worked there for a few years after graduating from Russian univerisities. After the Revolution, almost all of them established their own schools, organised institutes, etc.

Mandelstamm was not an exception. Driven out of the Odessa (Novorossiisk) University for participation in a student movement, he was studying from 1899 and then worked under Braun in Strasbourg, where he became a professor and his work gave him world recognition. He returned to Russia only in 1913. Mandelstamm had an enormous influence on Igor' Evgen'evich. It was under Mandelstamm's leadership that, at the age of 26, Tamm began his scientific work.

* * *

Igor' Evgen'evich was fully ready for such work. His mathematical training was particularly advanced. He demonstrated this when in 1922 – 1925 he published his first three papers.

The first brief paper, coauthored with Mandelstamm, was printed after the others abroad [1], but essentially it was a predecessor of more extensive papers [2, 3], which were published later and only in Russian. These investigations of the electrodynamics of anisotropic media in the theory of relativity were interesting from the general and fundamental viewpoints. However, even now one does not encounter in the literal sense an anisotropic body moving at relativistic velocities. Not surprisingly, these topics have not been discussed in the literature for a long time. Only after a quarter of a century did Jauch and Watson become interested in the problem and, unaware of the work of Mandelstamm and Tamm, reproduced some of the results of the latter [4 – 6].

In reality, these investigations do apply to quite concrete physical problems. The point is this: even in the case of isotropic media moving at a velocity $|u| \sim c$ a situation arises which is of anisotropic nature. For example, the permittivity ε and the permeability μ in the presence of spatial and/or temporal dispersion become (for example, in the case of a plane wave) dependent not only on the frequency ω and the wave vector $\boldsymbol{k}$ of the wave, but also on the 'anisotropic' combination $\omega - \boldsymbol{k} \cdot \boldsymbol{u}$. Therefore, the equations of electrodynamics become effectively of the same nature as in the case of an anisotropic medium. Another example is a plasma bunch moving relativistically in a magnetic field. Such situations can be tacked usefully employing the main result of Mandelstamm and Tamm [1]:

the Maxwell equations for the strengths of the electric and magnetic fields $\boldsymbol{E}$, $\boldsymbol{H}$ and their inductions $\boldsymbol{D}$, $\boldsymbol{B}$ are reduced to a single tensor equation. In the absence of field sources, this equation is

$$F_{ik} = s_{ijkl} H^{jl} ,$$

where $F_{ik}(\boldsymbol{E}, \boldsymbol{B})$ and $H_{jl}(\boldsymbol{D}, \boldsymbol{H})$ are the usual second-rank Minkowski tensors, and s_{ijkl} is a fourth-rank material tensor derived in a certain manner from the components ε_α and μ_α ($\alpha = 1$, 2, and 3 along the three axes) and the vector $\boldsymbol{k}$. Consequently, the strengths can be replaced with the potentials, the Green function can be found, etc., which greatly simplifies the problem. A review of the whole of this situation and the bibliography of an enormous number (over 400) of publications (up to 1975) on specific 'anisotropic' relativistic phenomena can be found in an extensive paper of Bolotovskii and Stolyarov [7], which also includes their own original work.

However, almost all of this was done without any reference to the work of Mandelstamm and Tamm. The first paper openly connected with their work was that of Ryazanov [8]. The work described above had been carried out by Igor' Evgen'evich already in Moscow, where he went in 1922 since the situation had become somewhat more normal after introduction of the New Economic Policy. However, he lived in a very unsettled manner. Now that he had a family, he supported it by both lecturing at uninteresting higher educational establishments, writing popular papers on physics, and translating books.

On the other hand, the situation in physics in the country as a whole was rapidly improving. In Petrograd (formerly St Petersburg, later called Leningrad) there were now major physics institutes of a kind never before found in Russia. This was, after all, the Physicotechnical Institute organised by Ioffe, which became the nursery of many other institutes that gradually developed from it, beginning in the thirties. Moreover, this did not happen just in the native city of the Physicotechnical Institute. New institutes appeared in Kharkov, Dnepropetrovsk, Sverdlovsk (formerly Ekaterinburg), and Tomsk. The kernels of these institutes were complete teams which were alumni of the Ioffe Institute (the Kharkov and Sverdlovsk institutes later became strong scientific centres). Another major institute, Rozhdestvenskii's Optical Institute, worked largely on applied problems (essentially, only because of this Institute could an industry mass-manufacturing a very wide range of optical devices later appear out of nowhere). However, the Optical Institute combined this with research work on the most fundamental problems in optics. It was not accidental that apart from Rozhdestvenskii himself, the following physicists worked there as well: Fok (Fock), Vavilov, and Terenin. Finally, one should mention the X-ray and Radiological Institute of Khlopin where subsequently (1937) the first cyclotron in Europe was commissioned.

In Moscow such fast growth began somewhat later. An important stimulus was provided by Mandelstamm who went to Moscow in 1925 to head the Chair of Theoretical Physics at the University. Igor' Evgen'evich became an unestablished lecturer in the Physics Department back in 1924 and in 1930 he succeeded Mandelstamm to the Theoretical Physics Chair. The best part of the old professorship joined Mandelstamm, as did also young assistants: Andronov, Vitt, Leontovich, Gorelik, Khaikin, Rytov, etc. The invitation to Moscow came to Mandelstamm after a long fight by these people against the old professors. An important role was played here by the youngsters, who were entering the governing bodies of the University and who had now a major influence (among them especially active was the student Andrionov, who later became a member of the Academy of Sciences). Among the teachers a major role was played by young Vavilov.

However, physics was also developing in Moscow at new technical institutes, similar to that established by Ioffe in Leningrad, and reporting to the National Commissariat (i.e. Ministry) of Heavy Industry. The Electrical Engineering Institute was established in this way and in the late twenties Tamm worked there in the Theoretical or Physics Divisions, as well as at Moscow University. It is interesting to note that his most important works on quantum theory of radiation [9, 10], described below, were published as from this Institute.

Well-known specialists in optics began to work there at approximately the same time. They were Fabrikant, Granovskii, and Vul'fson, pupils of Mandelstamm and Landsberg. Other topics in physics were investigated, for example, under Predvoditelev at the Heat Technical Institute, which was headed by a major and widely educated engineer, Ramzin (later tried as the alleged head of a mythical 'Promparty').

The state supported, as much as possible, the development of science and even directed funds (as can be seen from above) through an industrial ministry. However, research instruments were not yet being produced in the country. Research team members (in 1930, I was one of them, then the auxiliary laboratory worker at the Heat Technical Institute) searched second-hand shops for damaged Hartmann – Braun or Siemens – Halske ammeters and voltmeters, which could still be repaired, looked for objectives from old still and cine cameras, etc. When Mandelstamm, who did not have a spectrometer with a sufficient resolving power, suggested to the Leningrad optics physicist Gross that he should try to detect the doublet splitting of an optical line as a result of scattering, which Mandelstamm predicted (this is known now as the Mandelstamm – Brillouin doublet), he sent him (as a scatterer) part of a crystal glass decanter, which was bought in a second-hand shop. Physicists sent on official trips abroad frequently bought the necessary materials at their own expense. Naturally, the state itself, although poor, bought instruments extensively abroad.

In 1931 two pupils of Mandelstamm — Andronov and Gorelik — together with their close colleagues in Leningrad — Grekhov, Gaponov, and others — went to Nizhny Novgorod and established there an institute which has developed nowadays into a great complex of institutes, known to the scientific world. It should be stressed that all this spread (of people from Leningrad and from Moscow) to the provinces was largely based on the civic understanding of the need to spread science from the capital to the whole country. The traditions of the Russian intelligentsia were still alive. These people had to overcome enormous difficulties, but the job was done.

The shortage of equipment was being overcome slowly even in the early thirties. I saw with my own eyes how initially empty cupboards began to fill at the Institute of Physics of Moscow University. For example, enormous numbers of FI mirror galvanometers, made at the Leningrad Physics Institute, appeared at that time. Half of them failed to operate almost from start, but with each year the quality improved. They were inexpensive and they worked. Optical instruments also appeared. The construction of the cyclotron began in 1932 at the Radium Institute in Leningrad and even earlier a Van de Graaff was started in Kharkov. The industry now fulfilled special orders.

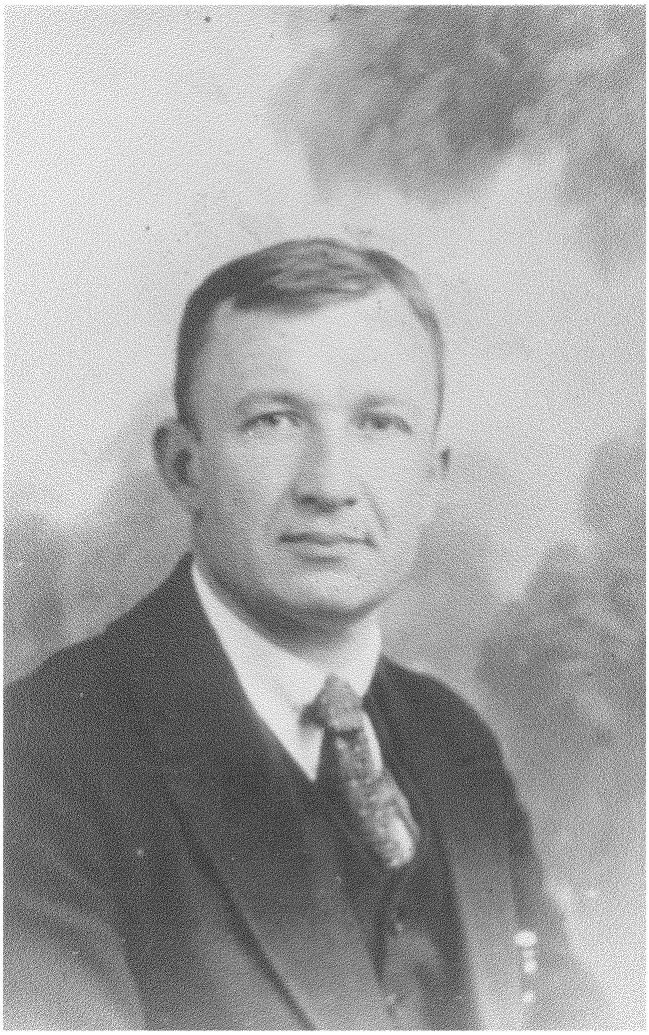
However, the living conditions of the scientists, particularly the young ones, continued to be hard for a long time. Up to 1935, Igor' Evgen'evich lived in an 'apartment' constructed from a stable at the University. The floor was at the ground level and the 'apartment' was frequently flooded; the toilets were outside. However, this did not embarrass us. Dirac, a friend of Igor' Evgen'evich, twice came to the Soviet Union and lived in this 'apartment'. There is a well-known account of Dirac's second visit. Embarrassed, Igor' Evgen'evich, rapidly and volubly apologising that nothing had changed since Dirac's first visit, was answered by the precise and laconic Dirac as follows: "Nothing changed? We had to carry a candle last time and now you have an electric lamp".

* * *

We must now return to Tamm's work during this period. He began his scientific work very late (for a theoretician), at the age of 26, but he worked very intensively. Initially, after the papers discussed above [1 – 3], there were few other papers still based on the old Bohr quantum theory (before the appearance of quantum mechanics) which were not of great importance. However, one should mention a paper on the paramagnetism of atoms [11] in which the contribution of the orbital momenta was considered for the first time. This result naturally remains valid even now, but is regarded as elementary. The work reported in Ref. [12] suffered a different fate: in this case Igor' Evgen'evich tried to find a general quantitative criterion of the validity of the correspondence principle, which then was the only method for the calculation of the phenomena in electrodynamics based on quantum ideas, and he calculated the intensities of atomic spectral lines. The work sent to press on 15th June 1925 was basically too late. The appearance of quantum mechanics made it unnecessary. However, we shall turn to the correspondence principle again. Finally, during these years he worked on his course *Fundamentals of the Theory of Electricity*, which appeared first in 1929. This course, remarkable for the clarity of its physics, became very popular. It went through many editions, each almost always supplemented and reworked during the life of Igor' Evgen'evich. The course continued to be published also after his death. In view of the above description of the level of teaching of the theory of electricity in the pre-Revolution Moscow University, it is not difficult to understand how refreshing and revolutionising was the role played by this book. The course is still valuable today.

In a sense, the turning point in the life of Igor' Evgen'evich came in 1928, when as an established scientist he went abroad for half a year. He visited Born at Gottingen, but he spent most of his time with Ehrenfest in Leiden. Here, he fully embraced quantum mechanics which had just been proposed and established close relationships with Ehrenfest, whom he regarded highly, and with Dirac who came to Leiden.

Tamm went home and finished work, which he started in Leiden, on the electrodynamics of a spinning electron [13]. It would be difficult to understand now why in 1928 – 29, when quantum mechanics and the Dirac theory of an electron with spin were already in existence, it was necessary to develop a complex relativistic nonquantum theory of the magnetic moment of an electron. However, the very fact that this work was done mainly at Leiden and that acknowledgement was made to Ehrenfest and Fokker 'for many valuable discussions' shows that the problem of the correspondence between the Dirac theory of spin and the classical picture of a spinning charge was regarded as topical by the best physicists of the time.



Igor' Evgen'evich Tamm (Leiden, Holland, 1928)

Igor' Evgen'evich was devoted to physics in an all-absorbing manner. When at that time his interests turned to the unified field theory of Einstein, he published in 1929 alone five (!) communications (two of them coauthored with Leontovich). The aim was to explain the behaviour, in this theory, of a Dirac electron and, more widely, to try to show, as he writes in the first of these communications, "that the new Einstein field theory has certain quantum-mechanical features". In particular, Tamm tried to determine whether the field of a charged particle is spherically symmetric in the unified field theory.

However, this work, which required much time and effort, as well as complex mathematical procedures, suffered a bad fate. The unified field theory — in which Einstein tried to unify electromagnetic and gravitational fields in the same way as the electromagnetic field unifies electric and magnetic fields, and to which Einstein devoted the last three decades of his life — failed to solve the problem. We now know why this was so: electromagnetism should be included in the unified theory together with weak (as well as strong) interactions, which were not yet known. Practically the whole physics world then regarded Einstein's stubbornness as a peculiarity permissible to a genius. It took several decades for Einstein's *idee fixe* to be reborn at a new level and become a generally accepted key problem.

Summarising this stage of the first eight years of serious work of Igor' Evgen'evich in theoretical physics, when he demonstrated his high 'technical' professionalism and knowledge of subjects, one must admit that it did not bring him real success. Of course, one must not forget his excellent course on the theory of electricity. Nevertheless, there were no perceptible scientific results.

Here, Igor' Evgen'evich makes a sharp turn in his research. He moved away from consideration of the most general problems (relativistic electrodynamics of anisotropic bodies, limits of validity of the correspondence method, and unified field theory) to a study of specific phenomena within the framework of quantum mechanics. In a few years he obtained very significant results.

He was close to Mandelstamm, to Landsberg, who was almost his age, and to the younger friend Leontovich. This had a direct influence on the selection of the subject of the first work done during this period [9]. These physicists began to study in a very fundamental way the scattering of light in solids both experimentally and within the framework of the classical theory in which this phenomenon is regarded as the scattering of an optical wave by elastic vibrations of a crystal. It was already fully understood that the frequency of the scattered light could be higher or lower than the initial frequency. Igor' Evgen'evich provided a quantum theory of the process, but the importance of this work goes well beyond a simply consistent theory of a specific process. He quantised elastic vibrations by analogy with the quantisation of an electromagnetic field carried out by Heisenberg and Pauli. As a result, he represented collective vibrations of the lattice particles as a gas of 'elastic quanta' regarded as quasiparticles, each of which includes the motion of all the particles in the crystal lattice. Frenkel suggested that they should be called phonons. The specific formulas obtained also revealed some departure from the results of the classical theory. They were confirmed immediately by the experiments of Landsberg and Mandelstamm.

However, the main point is that for the first time in physics the motion of many interacting particles was reduced to the behaviour of a gas of quasiparticles. It is difficult to exaggerate the importance of this step.† After all, since then the various quasiparticles, and in particular phonons, have become such a fixed feature of physics and a generally accepted concept that any specialist physics or general encyclopaedia under the headings 'phonon' or 'quasiparticle' will fail even to mention that they were introduced into physics by Tamm in Ref. [9].

Immediately afterwards another important work was published by Igor' Evgen'evich on the scattering of light by a free electron, i.e. a theory of the Compton effect [10]. This is again a particular process, but once more the results are of fundamental theoretical importance.

† See the paper of Savrasov and Maksimov in the present issue.

This importance arises from the fact that up to the late twenties the majority of leading physicists had been assuming that in calculation of quantum electrodynamic processes it is sufficient to quantise the motion of electrons and to consider their interaction with the field in accordance with the correspondence principle. For example, the mutual scattering of two electrons, when one goes over from a state $\psi_1^0(r_1)$ to a state $\psi_1^{\mathrm{f}}(r_1)$, and the other goes over from state $\psi_2^0(r_2)$ to state $\psi_2^{\mathrm{f}}(r_2)$ is dealt with as follows: let us construct the charge densities for the transition $\rho_1^{0\mathrm{f}} = e\psi_1^{\mathrm{f}^*}(r_1)\psi_1^0(r_1)$ and $\rho_2^{0\mathrm{f}} = e\psi_2^{\mathrm{f}^*}(r_2)\psi_1^0(r_2)$ together with the transition currents $j_1^{0\mathrm{f}} = e\psi_1^{\mathrm{f}^*}(r_1)\boldsymbol{\alpha}\psi_1^0(r_1)$ and $j_2^{0\mathrm{f}} = e\psi_2^{\mathrm{f}^*}(r_2)\boldsymbol{\alpha}\psi_2^0(r_2)$. According to perturbation theory, considered in the lowest order in respect of the charge, the scattering amplitude is then (Moller formula):

$$A_{0\mathrm{f}} = \mathrm{const} \int \frac{\rho_1^{0\mathrm{f}}(r_1)\rho_2^{0\mathrm{f}}(r_2) - j_1^{0\mathrm{f}}(r_1) j_2^{0\mathrm{f}}(r_2)}{|r_1 - r_2|} \, \mathrm{d}r_1 \, \mathrm{d}r_2$$

($\boldsymbol{\alpha}$ are the Dirac matrices; const is a well-known factor). Here, second quantisation is completely unnecessary. (Even Landau held this view right up to the last war.)

Similar considerations yield the transition amplitudes for other processes. However, the justification for this approach is in doubt. Moreover, even at that time there were known cases when this approach gives different results in different specific applications. Igor' Evgen'evich himself referred in Ref. [10] to the difference between the results of Schrodinger and Klein, who dealt with the Raman scattering of light by atoms.

Therefore, Igor' Evgen'evich reviewed the problem of the Compton effect and quantised the field consistently in accordance with Pauli and Heisenberg, i.e. he applied second quantisation. His final formula was identical with that obtained by Klein and Nishina a year earlier by applying the correspondence principle. It would seem that Tamm simply confirmed their result and that his work belongs to the category which Landau called contemptuously 'Verklarungen und Neubegrundungen' (explanations and new derivations). However, this would be completely wrong. The correspondence principle involves only the initial and final states of the particles, whereas intermediate states are considered in second quantisation. Igor' Evgen'evich discovered that intermediate states, in which a Dirac electron is in a state with a negative energy, play a fundamental role. Even in the limiting case of the scattering of long (infrared) wavelegths, when the result is the classical Thomson formula, these negative-energy states are absolutely essential.

It should be mentioned that at that time the existence of the negative-energy states in Dirac's theory was a headache to physicists. After all, this theory predicts that all the real electrons with a positive energy should drop to a level with an infinitely large negative energy. As is known, Dirac himself suggested an improvement in the situation by filling all the levels with negative energies by electrons, so that the observed objects can only be the 'holes' in this background and they would have the property of positively charged particles. Essentially, this was a prediction of the existence of positrons, but they were discovered later in 1932 and at the time both Dirac and Tamm assumed that the holes were protons, although in fact the mass of a hole should be equal to the mass of an electron. The majority of theoreticians regarded the filled background and the holes to be an absurdity.

In this foggy atmosphere many found it to be absolutely essential to banish somehow the negative-energy states from Dirac's theory. Therefore, the result of Igor' Evgen'evich was of enormous importance. He destroyed the hope for the banishment of negative energies.

In the same paper, Igor' Evgen'evich considered the probability of annihilation of an electron and a hole, which he again assumed to be a proton, and discovered a new serious difficulty; an electron in an atom should become annihilated by interaction with a nuclear proton in a negligibly short time of $\sim 10^{-3}$ s. Only two years later, when the positron was discovered, this formula helped to make clear why positrons are so difficult to observe under terrestrial conditions: when they reach a medium with a large number of electrons, they become very rapidly annihilated by interaction with electrons.

The instructive point of the history of this paper was that both Tamm and Dirac regarded their 'terrible' conclusion on the fast annihilation of an electron with a proton in its own atom only as a theoretical 'difficulty', but not a catastrophe. An ordinary theoretician not working at such a high level would not have dared to look so calmly at this result, which seemed to make the whole theory useless. However, both Dirac and Tamm saw in this theory a convincing harmony and beauty, and not only a successful explanation of the spin, so that difficulties of this kind did not disturb them.

A specific event is related to this paper and perhaps we should digress to describe it. A letter from Tamm to Ehrenfest of 24th February 1930 has been preserved. It follows from this letter that Igor' Evgen'evich had initially sent the paper with a small error. Here is an excerpt from this letter:

''Dear Pavel Sigizmundovich! I am sure you have received my telegram. I am very, very ashamed. As I wrote to you, I checked my calculations three times before sending you the note. Then I sat down to write the paper in full for publication and, as always, I derived everything again without looking at what was written. And now I found that right at the beginning I everywhere gave the wrong sign to the sine! If all this is correct, then the final formula does not differ in any way from the Klein–Nishina formula!

''The whole story is especially painful, because I have now been able to carry out calculations in an elegant form which I find pleasing. If these calculations are modified only slightly, it is possible to calculate — for example — the probability of a spontaneous transition of an electron from a state with a positive energy to a negative-energy state. I am working on this at present and will finish the work in a few days.

''I find it terrible that I am turning to you for the second time with a request to publish and the second time with such errors (last year I did not symmetrise the wave equation) ...

''Yours sincerely, Igor Tamm

''P. S. Naturally, all that I said about the dominant role of transitions via a negative-energy state remains valid. Igor Tamm''.

In my reminiscences about Igor' Evgen'evich (see the footnote on the first page of this paper) I wrote that he did not publish a single paper with an error (he put forward hypotheses, which were not confirmed experimentally and were addressed primarily to experimentalists) and that he used to submit only very carefully and repeatedly checked manuscripts. However, there were obviously moments when he avoided publication with an error at the last possible moment. It is evident from this letter that the corrected paper was supplemented, in particular, by a formula for the annihilation cross section.

Two more specific communications by Tamm were devoted to Dirac's theory of the electron: in 1930, on the scattering of light by two electrons, and in 1934, after the discovery of the positron, on the density matrices in Dirac's theory (instead of the name of the institute where the work was carried out, Tamm wrote: ''Teberda, Caucasus'', i.e. the work was carried out during a holiday in a mountain resort [14]).

In 1931, Tamm travelled to work in Cambridge with Dirac, whom he called a genius and whom he admired. On the way there, he visited Ehrenfest in Leiden, and on his way back he met Jordan in Rostock. The contact with Dirac developed into a true friendship. In a letter to Mandelstamm, Igor' Evgen'evich wrote: ''I feel very well in Cambridge ... In the scientific sphere ... the most interesting is a new work of Dirac finished 'in front of my eyes' ... He demonstrates that the existence of isolated magnetic poles is possible in quantum mechanics''. Igor' Evgen'evich adds in this connection that he ''wrote a mathematical (sic!) work, reporting an investigation of amusing properties of the eigenfunction of an electron in the field of a magnetic pole''. Tamm means here his paper cited as Ref. [15].

Dirac taught Tamm to drive a car and they went to Scotland where at a suitable place Igor' Evgen'evich introduced Dirac to mountaineering (before that ''As a preliminary course, I personally climbed trees with him'', as he writes in the same letter). Later, when Dirac came to the Soviet Union, Tamm went mountaineering with Dirac in the Caucasus.

However, let us now return to physics.

After his work on the scattering of light and on Dirac's theory of the electron, Igor' Evgen'evich tackled a new topic which was just emerging: this was the quantum theory of metals. He investigated three important problems.

First, in 1931 together with Shubin, a pupil of Mandelstamm and an exceptionally talented theoretician (who was arrested in 1937 and killed a year later), he proposed a theory of the photoelectric effect in metals. This work became a classic and the starting point for many subsequent investigations by other workers. Two different physical bases for the two components of the effect were clearly distinguished: the surface effect, i.e. the knocking out of an electron from a surface layer in which the main role is played by a jump of the potential at the metal–air interface, and the bulk effect, i.e. the knocking out of the conduction electrons (Bloch waves) moving inside the metal. On his way to Cambridge, Igor' Evgen'evich presented this work in a lecture in Leiden [16].

Second, together with his student Blokhintsev, he reported an investigation of the work function of electrons emitted from a metal [17].

Finally, the third and perhaps the most important in this cycle was Tamm's discovery of the possibility of existence of special surface states of electrons in a metal. The electrons in these states cannot escape outside or enter the interior of the metal [18]. These 'Tamm levels', as they have since been called, proved to be exceptionally important in the physics of surface phenomena and, a quarter of a century later, in transistor technology, etc. By the sixties this topic grew so much that it merited a review book by Davison and Levin entitled ''Surface states'' [19], published in Russian translation in 1973.†

Finally, ending this period in 1933, I must mention that Igor' Evgen'evich for the first time turned to the methodological problems of physics in a philosophical journal [20]. He defended the new physics, i.e. the theory of relativity and quantum mechanics, from ignorant attacks of party philosophers and conservative physicists, who called themselves Marxists. Igor' Evgen'evich explained to them the true meaning of this new stage in the development of science, but he was rewarded only with naked hatred which fixed his reputation as a 'bourgeois idealist'. This reputation was the reason why he later encountered much dangerous unpleasantness.

* * *

Let us pause for a moment and consider the situation in physics at this time, which was at a turning point both on the world scene and in our country.

In the world dimension the year 1932 saw the experimental discovery of the positron (which confirmed strikingly Dirac's predictions, strengthening the authority of quantum mechanics and theoretical physics in general), and of the neutron (it became clear accordingly that the nucleus consists of protons and neutrons, which opened up a new era of nuclear physics). In April of that year, Cockcroft and Walton used a high-voltage accelerator which they built to split, for the first time, atomic nuclei by accelerated protons. All this (and many other successes of physics) changed the whole research atmosphere and initiated rapid growth of the scale of research. This change is described well in the novel of Mitchell Wilson (physicist by education) called *Life with Lightning* (for some inconceivable reason entitled in the Russian translation *Life in a Fog).*

In our country this coincided with the period of transformation of physics into an advanced branch of science which has acquired international recognition. By that time we could boast of several discoveries of the kind that would deserve Nobel prizes. For example, in 1928, Landsberg and Mandelstamm discovered the Raman scattering of light in crystals, usually known by this name because Raman observed it in India and reported this by telegram to *Nature* three months before Landsberg and Mandelstamm sent their paper for publication, and it was Raman who received the Nobel Prize.‡. This delay proved decisive, although Raman's understanding of the effect he observed was less complete than that of Landsberg and Mandelstamm who accounted clearly for the physics of the effect.

Moreover, in 1927, Skobel'tsyn discovered, at the Leningrad Physicotechnical Institute, that cosmic rays observed on the surface of the Earth were high-energy electrons (and, therefore, could not be, for example, products of radioactive impurities in the atmosphere) and in 1929 he proved that they arrived in the form of showers of electrons (it is interesting to note that up to that time the abstract journal *Physikalische Berichte* classified work on cosmic rays in the geophysics section). This discovery can be regarded as the beginning of high-energy physics. However, the Nobel Prize went to Walther Bothe, who confirmed this result by another method, using a system of counters connected to a coincidence circuit, instead of observations in a Wilson chamber, as was done by Skobel'tsyn. It is interesting that the Prize was formally awarded specifically for the method.

In 1926 – 32, Semenov (initially working with Khariton) discovered branched-chain chemical reactions and a quarter of a century later he received the Nobel Prize for this discovery jointly with Cyril Hinshelwood.

Finally, somewhat later in 1933, Vavilov and Cherenkov observed unusual emission of light by electrons in a medium, now known by their names (their results were published in 1934), which was explained several years later by Tamm and Frank. A quarter of a century later they also received the Nobel Prize.

In addition, much other remarkable work was done. I shall quote just a few examples. Leontovich and Mandelstamm demonstrated theoretically that quantum mechanics predicts the possibility of the tunnel effect and explained all its main properties. George Gamow used this effect to develop his theory of α decay of nuclei. A Van de Graaff accelerator was constructed at the Kharkov Physicotechnical Institute and it was used (in the same year 1932 which saw the discovery of Cockcroft and Walton) by Val'ter, Latyshev, Leipunskii and Sinel'nikov (all young scientists) to repeat the experiment on the splitting of the nucleus. One could mention also other important work.

All this is evidence that in the early thirties there existed a new generation of theoreticians (including the still young Landau) and experimentalists, brought up within one decade, able to produce work at world level. Even greater numbers of such high-level papers appeared in the years immediately after. The schools created by scientists of the older generation, mentioned above, gave birth to other new schools.

At the same time, as I mentioned earlier, scientific instrument production was being established. New institutes multiplied rapidly. Earlier papers were traditionally sent to *Zeitschrift fur Physik* and *Physikalische Zeitschrift*,

† Enormous numbers of experimental and theoretical investigations of the multiplicity of the physical phenomena published so far are rooted in Tamm's surface levels, but of course the treatment is now much more complex and there is a greater variety of these phenomena. One example of this is the paper by Volkov in the present issue.

‡ In fact, Landsberg and Mandelstamm observed the effect even a little earlier than did Raman, but they took a long time to check further their experiments. For these and other reasons, they delayed sending their manuscript [for details, see Fabelinskii I L *Sov. Phys. Usp.* **21** 780 (1978)]. For a long time it was thought in our country that Soviet scientists were excluded from consideration for the Nobel Prizes because of the anti-Soviet attitude of the Nobel Committee (and that Born resigned from the Committee by way of protest). This attitude could have been a serious factor, but now the situation looks simpler because, in accordance with the rules of the Committee, all the confidential documents were published after more than 50 years from the events in question. It turned out that in 1929, nobody recommended Landsberg and Mandelstamm, whereas Raman was nominated by two outstanding physicists, including Bohr. However, the Prize was awarded to de Broglie. Next year, in 1930, both our scientists were put foward by Khvol'son while Mandelstamm was additionally supported by Papaleksi, whereas Raman's candidature was supported by many more well-known physicists. This was probably decisive. Raman received the Prize. In simple words, our physics did not yet have sufficient authority abroad. Moreover, the procedure for putting forward a candidate, as is well known, requires special 'organisational activity' on behalf of the candidate.

I E Tamm with Paul Dirac (Caucasus, 1936)

where they were rapidly published. Besides we had only the *Zhurnal Russkogo Fiziko-Khimicheskogo Obshchestva* where papers were published in Russian after a delay (of up to one year). However, in 1931 an international journal *Physikalische Zeitschrift der Sowjetunion* was started in Kharkov and papers were published rapidly in German, English, and French. True, this journal was mostly only nominally 'international'; nevertheless, papers appeared there also by foreign authors, even by Dirac. The main point was that Soviet work now had a direct channel abroad and the new journal was read there.

An International Conference on Theoretical Physics was held in 1934 in Kharkov with just 30 attendees. However, they included Bohr, Rosenfeld, Waller, and Gordon (of the Klein–Gordon equation), a total of 8 foreigners. The interest in our science was growing.

Only a few years before the simple fact of the existence of physics research in our country was not widely known. I can quote here my own example. I grew up and went to school in central Moscow. I was attracted to physics and I visited the Rumyantsev (later Lenin) Library to read books describing the work of Einstein, Bohr, and Rutherford. I was certain that research was only for people of genius and not for ordinary people. When I finished school I applied to a chemical institute, which at least dealt with electrochemistry. When next year (1930) I learned of the existence of a physics department about a kilometre from my school and joined it, some acquaintances of my parents asked whether physics was physical training.

Such ignorance disappeared soon. The announcement about the splitting of the atomic nucleus in Kharkov was printed in *Pravda* on the first page as the main news of the day in the form of a "Report to Comrade Stalin", as a major achievement of Soviet science. Knowledge of physics grew rapidly.

* * *

The scientific revolution of 1932 sent also Igor' Evgen'evich in a new direction. He switched to nuclear physics.

The fate of his first work in this field (carried out jointly with his postgraduate student Al'tshuler) is interesting: in this work an analysis of the experimental data on the magnetic moment of the nuclei was used to conclude that a neutral particle (neutron) had a magnetic moment. It is now very difficult to understand why this minor work attracted such very great condemnation at the Kharkov Conference on Theoretical Physics, mentioned above and held in 1934. This was because at the time it was regarded as self-evident that elementary particles are elementary in the limiting sense and, therefore, are point-like. Moreover, some authors were of the opinion that a particle cannot be not point-like because then the interaction in a collision would have travelled across the particle at a velocity exceeding that of light. In other words, a nonpoint particle was regarded implicitly as undeformable and absolutely hard. Therefore, there was no possibility that within a neutral particle there could be a nonzero charge or current distribution, which would give rise to a particle which is neutral only as a whole. However, Igor' Evgen'evich did not regard these objections as convincing arguments and he insisted on his interpretation. Not much time was needed before his view was accepted as correct.

Extensive and important work of Tamm on the nature of nuclear forces was of a very different character. Naturally, the proton–neutron structure of the nucleus raised the question of the forces holding these particles together. After all, at the time only gravitational and electromagnetic forces were known. Even the very hypothesis that there are some other forces seemed to be an improbable bold fantasy. Igor' Evgen'evich wrote thus in his paper: "... directly after the discovery of the neutron in 1932, Heisenberg put forward the hypothesis that the interaction of a proton with a neutron is due to an exchange of electric charge". Please note, only the electric charge is mentioned. Nothing was known about the exchange mechanism. However, in 1934, Fermi put forward his remarkable theory of β-decay, as a process of emission of an electron–neutrino pair by a nucleon. This was sufficient for Igor' Evgen'evich to suggest immediately that nucleons (actually this term was not then yet used) interact by exchange of such pairs and their antipairs. This was a hypothesis of the existence of completely new forces, additional to gravitation and electromagnetism. Tamm immediately started calculations, continued them at night during the Kharkov Conference, and obtained a discouraging result: this exchange indeed represented a new type of force and, as required, of very short range decreasing with distance r as r^{-5}, but the force was too weak by many orders of magnitude to account for the stability of nuclei. Igor' Evgen'evich published his formula for the interaction potential $V = ar^{-5}$ in the form of a letter to *Nature* [21] and he was extremely unhappy about his failure.† In this formula the constant a is proportional to the square of the β-decay constant g^2 (i.e. as we would say now, the square of the weak-interaction charge) and the formula applies when $r \ll \hbar/mc$, where m is the mass of an electron, i.e. if $r < 10^{-13}$ cm, we have $V(r) \propto g^2 r^{-5}$.

In the next two years Tamm searched unsuccessfully for other ways of implementation of his concept. When in 1935 Konopinski and Uhlenbeck put forward a modification of the Fermi β-decay theory [23], Igor' Evgen'evich grasped this variant, which for a year or two charmed some physicists by the fact that it seemed to describe better the β-ray spectra. In this variant the forces turned out to be several orders of magnitude *greater* than required. In an extensive paper, published in 1936, Tamm showed [24] that the required force can be obtained as a linear combination of the two variants. But the actual approach of Konopinski and Uhlenbeck admits the possibility of many variants so that Igor' Evgen'evich ends his paper with a melancholy sentence: "It seems senseless to investigate further the enormous range of possibilities of this kind without knowledge of some general principles, which have not yet been discovered". I remember his feelings at the time. He sent his paper for publication unwillingly.

However, he was wrong. First, in fact the "enormous range of possibilities" which the variants of the theory of Konopinski and Uhlenbeck provided disappeared very soon. Right from the beginning this theory met with fundamental objections of many theoreticians. The interaction Hamiltonian of this theory contains terms with higher-order derivatives (of the operator wave function) with respect to time. This is not permissible, because then the change in the wave function with time is not govened by its value at moment 'zero', but requires knowledge at the same moment of the time derivatives of this function. This is in conflict with fundamental principles of quantum mechanics.

Second, it soon became clear that the experimentally observed deviation of the β-ray spectra from predictions of the initial Fermi theory can be explained excellently by the superposition of several β-ray spectra and by the cascade decay of radioactive nuclei. Therefore, the initial variant (i.e. that based on the Fermi β-decay theory) of the Tamm theory of β forces describes real weak forces between nucleons (these forces were detected experimentally several decades later), but these forces do not determine the stability of nuclei.

Very soon, in 1935, Yukawa quoted directly Tamm's work and put forward a bold idea, according to which the nuclear forces are due to exchange with an as yet unknown hypothetical particle with a mass of ~ 300 MeV c^{-2}, which he called the meson. We now know that this particle is the pion. The starting point in Yukawa's work was Igor' Evgen'evich's idea of interparticle forces due to exchange of particles with a finite mass.

Thus, over a period of some six or seven years, Tamm published three series of papers: on the scattering of light by solids (introducing the concept of a quasiparticle in the form of the phonon) and by electrons (including the electron–hole annihilation, demonstrating the inevitability of for the Dirac negative-energy levels); on quantum theory of metals (predicting the existence of the surface 'Tamm levels' and explaining for the photoelectric effect in metals); on nuclear physics (putting forward the idea that the neutron has a magnetic moment and, most important, proposing a theory of nuclear β forces). This range of work immediately gave him the recognition and respect of the world community of physicists. I was told by Bruno Pontecorvo that Fermi had a great regard for Tamm. Dirac was Tamm's friend and worked with him. Ehrenfest suggested that he be his successor for a professorship which was held by Lorentz before Ehrenfest. Our own physicists also recognised his work: in 1933, Igor' Evgen'evich was elected Corresponding Member of the USSR Academy of Sciences, he established good relationships with Landau and Fok (Fock), and an old friendship linked him with Frenkel. But he also gained a reputation of a 'bourgeois idealist' among our 'Marxist' physicists and reactionaries.

It seemed very natural that, when Vavilov organised (after the Academy of Sciences of the USSR was relocated to Moscow in 1934) the Lebedev Physics Institute and invited there the best Moscow physicists, one of them should be Igor' Evgen'evich who organised and headed the Theoretical Division of the Institute. This was the position he occupied until after his death. The Division is now named after Tamm. Very soon Igor' Evgen'evich transferred his weekly seminar from the University to the Lebedev Institute and concentrated his activity there.

* * *

A completely new event occurred at that time. In 1933, Cherenkov, an exceptionally careful and thorough experimentalist and a postgraduate student of Vavilov, was

† Ivanenko also published a letter in *Nature* in the same issue [22]: he put forward exactly the same exchange concept, but without any formulas, calculations, quantitative results, or even estimates.

investigating the radiation of uranyl salts dissolved in a liquid, which represented the fluorescence excited by β electrons emitted by radium. Cherenkov was horrified to find that the liquid emitted luminescence almost as strong, even without uranyl salts. This made the study of these salts a pointless task. However, Vavilov immediately became interested in this 'parasitic' radiation and rapidly established that this was a new hitherto unknown effect. The subsequent studies, very difficult at the level of the then available experimental techniques, enabled Cherenkov to determine the detailed characteristics of this radiation (it was later found that these details were determined surprisingly accurately). Discussion of the various possibilities by Vavilov, Frank, and Tamm led them increasingly to the conclusion that a uniformly moving electron was emitting such radiation. This seemed to be a wild suggestion and it caused much amusement, because 'everybody knew' that a uniformly moving electron could not emit radiation. Everybody knew it was sufficient to go over to a frame in which an electron is at rest and it becomes clear that it can emit nothing. However, this misses a 'tiny' detail. The conventional thinking quoted here applies to an electron moving in a vacuum. However, we are discussing here the motion in a material medium (in this case, a liquid solvent). Then, in a frame in which an electron is at rest, there appears the effect of the moving medium which now begins to emit. Frank and Tamm proposed in 1937 [25, 26] an explanation and a theory of the Cherenkov – Vavilov radiation (in the West it is known simply as the 'Cherenkov radiation', ignoring the important role that Vavilov played in this discovery). In a paper published in 1939, Igor' Evgen'evich developed his theory more fully. As mentioned above, it was precisely for this discovery that Tamm, Frank, and Cherenkov received the Nobel Prize in 1958 (Vavilov died well before and the Nobel Prizes are not awarded posthumously).

It is amusing that, in a scientific biography of Igor' Evgen'evich, this work (recognised by a very high award) was essentially regarded as a temporary side line. After all, the physics of nuclei and elementary particles was Tamm's main preoccupation at that time and subsequently. One could regard also as a side line a very good paper, which helped greatly the physics of cosmic rays. This was the paper that Tamm published in 1939 jointly with his pupil Belen'kii (Belenky) [27] and in which the theory of electromagnetic showers was greatly improved by including the losses of the particle energy in the process of ionisation of the medium in which a shower is developing.† Only then is it possible to apply the theory to the main group of shower particles, namely those with relatively low energies but representing the bulk of a shower. Later, the same authors returned to this problem [28] in order to refine the treatment and to compare their results with the experiments published in 1945.

Another paper published in 1940 [29] was in the mainstream of Tamm's interests and deserves to be specially mentioned. At this time the discovery (in cosmic rays) of muons and detection of their decay led to the erroneous opinion that they each decay into an electron and one neutrino, so that their spin was assumed to be unity. It was not yet known that this was not the meson proposed by Yukawa as the carrier of the interaction between the nucleons (the pion was discovered only in 1947). Only gradually a recognition came of a fundamental conflict which arose when attempts were made to identify the Yukawa nuclear meson with the with the particle observed in cosmic rays: in fact, it was known that cosmic-ray mesons represent what is known as the penetrating component, i.e. that they interact weakly with the nuclei of atoms in the medium and, therefore, cannot be the carriers of nuclear forces. Nevertheless, possible properties of charged particles with a spin of unity were the subject of many theoretical investigations. This usually amounted to calculations of the parameters of various electromagnetic processes involving these particles (such as their scattering by Coulomb centres or electromagnetic bremsstrahlung), carried out on the basis of perturbation theory. The results of such calculations led to unlimited rise of the cross sections with the energy at high values of the latter, which was incompatible with perturbation theory.

Tamm obtained a very interesting and important result: he found that the exact and complete system of wave functions of such a particle in the field of a Coulomb centre must include states with a singularity at this centre, i.e. such equations describe the particle falling on the centre [29]. One can solve this problem by attributing, as suggested by Landau, finite dimensions to the particle and by considering its scattering by a Coulomb centre in the Born perturbation theory approximation, in order to avoid an infinite rise of cross sections with the energy. Landau assumed that the radius of a meson with a spin of unity should be $r_\mu \propto e^2/m_\mu c^2$, where m_μ is the meson mass [30].

Naturally, this was only a rough estimate. After all, nobody knew how to introduce a relativistic particle of finite dimensions into a local theory. Nevertheless, this rough estimate, together with the conclusion of Igor' Evgen'evich of the falling down on a centre in the exact theory, was used by Landau and Tamm to put forward a hypothesis [31] that such a meson approaches a proton only to a distance of the order of r_μ. At this distance the energy of the *electromagnetic* meson – proton interaction becomes of the order of hundreds of megaelectron volts, i.e. of the order needed for the nuclear forces. According to this hypothesis, a neutron consists of a proton and a negative meson with spin of unity.

This work of two major theoreticians is a clear demonstration of the deep fog that had to be penetrated by physicists in order to understand the nature of the nuclear forces, and how far from reality were the hypotheses they put forward. In the prewar years, Igor' Evgen'evich published several more papers on the problem of nuclear forces, which were more in the nature of reviews of the state of the art and discussions of various proposed ideas, but they did not move the topic essentially forward. The theory of nuclear forces was in a blind alley until the discovery of the pion.

* * *

In the period from the mid-thirties right up to the War, the scientific work of Tamm had thus three milestones: the first was the 'Nobel' work on the theory of the Vavilov – Cherenkov radiation, which — in spite of the high professionalism it needed — paradoxically could not be regarded as particularly outstanding among all the investigations of

† The influence of the nuclear medium on quark – gluon jets is at present under active discussion (see the paper by Dremin and Leonidov in the present issue).

Igor' Evgen'evich, although naturally it was one of the better ones. The second was the work, carried out jointly with Belen'kii, on an improved theory of electromagnetic cosmic-ray showers. The third and final work of fundamental importance was on charged particles with spin of unity in the Coulomb field. Is this much or little? Obviously, this is the work of a high-level theoretician. However, one could still say that the author of the phonon concept and of the theory of the β forces could do better. What was the problem? The explanation could not be an age-induced falling-off of creativity. At the same time, Igor' Evgen'evich was 40–45, which — according to the ancients — is the 'acme', the flowering period. Moreover, Tamm's subsequent work is sufficient proof to reject this supposition.

In fact, one could only be surprised how much Igor' Evgen'evich achieved during that time. After all, these were the years of Stalin's awful terror. The friends and colleagues of Igor' Evgen'evich were killed off or sent to Gulag camps. Monstrous false trials followed one another. At one of these trials Leonid Evgen'evich Tamm, a beloved brother of Igor' Evgen'evich, appeared as a 'witness' who publicly 'admitted' fantastic sabotage (he occupied a high engineering position in the Donbass region) on the instructions of Pyatakov, who was his boss and one of the central figures at the trial. The confessions of Leonid Evgen'evich were published under large headlines in national newspapers. One could only guess what kind of interrogation could produce these confessions. Leonid Evgen'evich soon died in prison. As usual, this resulted in additional terrible misfortunes to the family of the convicted. Igor' Evgen'evich himself was subjected to humiliating censures at public meetings at the University and at the Lebedev Institute, at which he was accused of the 'loss of vigilance' against the 'enemies of the nation' (including his brother, his childhood friend Gessen, Director of the Institute of Physics at the University, shot in 1936, and so on). All this was accompanied by hints and threats of serious consequences. Even when not said aloud, there were implications of his Menshevik past, etc. The whole atmosphere was terrifying and of itself it was very depressing even if there were no personal censures.

It might seem surprising that people still remained human and even worked reasonably in this atmosphere. Even now, it is difficult to understand how the minds gripped and twisted by fear and ideological pressure could at the same time work independently and creatively in a professional field. Obviously, such work was a form of salvation and a kind of 'inner emigration', which made it possible to retain one's humanity. How could one otherwise explain the fact that Landau after a year in prison — under the then ruling hard investigation conditions, which were in fact brutal interrogations ('conveyor-belt questioning' etc.) — was able to publish (in the next one and a half or two years) some ten papers, including the fundamental theory of liquid helium for which he later received the Nobel Prize.†

During that period Igor' Evgen'evich was weighed down and tired out by persecutions, threats, loss of his nearest friends, and transformation of the regime which promised socialism (the dream of his life right from his young days) into a despotic pitiless dictatorship. Tamm was a man of strong will and did not reveal his state of mind in front of others at the Institute. One could guess what he was living through only when frequently the stern concentration replaced his usual lively face, adding more and more wrinkles. To such a creative man the pure atmosphere of honest scientific work was that breath of fresh air which made it possible to survive with the 'noose around the neck'. This was true of the majority of those who devoted their life to science.

* * *

When the Second World War started, nuclear theoreticians and experimentalists faced a quandary. A feeling that nuclear science was unnecessary was superimposed on the general grief and horror. This state of depression was unconcealed when on the second day of the War, on Monday 23rd June 1941, Igor' Evgen'evich brought together the few (at that time) colleagues working in the Theoretical Division of the Lebedev Institute. Perhaps only one of them, Leontovich, recently working on the theory of propagation of radio waves, knew that for him the transition to defence research (radar!) would be a natural step. However, the dominant subjects in the Division were fundamental problems in the theory of elementary particles, of nuclear forces, etc. Nobody now needed these subjects. Feverish search therefore began for topical applied fields. At the Leningrad Physicotechnical Institute the work on the physics of nuclei and particles was wound down immediately by Kurchatov who was heading a team that had just carried out the first experiments on the recently commissioned cyclotron (the first in Europe) at the Radium Institute. Kurchatov started work on the means of safeguarding warships from magnetic mines. He was joined by Aleksandrov and other colleagues from the same Leningrad Institute. At the Lebedev Institute, evacuated to Kazan, Blokhintsev attacked the problem of reducing the noise of aeroengines (initially, before radar was developed, the antiaircraft defence was based on noise direction finding). This required a serious study of acoustics. Pomeranchuk (working at the time at the Lebedev Institute) also tackled this subject for a time. Others selected very narrow applications. For example, Markov began to construct antitank shells with improved aerodynamic properties (this was naturally a bit naive because the designers at the defence institutes were better professionals in this field), and so on. However, the passionate desire to help in some way the soldiers at the front was deep and sincere. Nuclear physicists proved essential only two or three years later.

These are the circumstances under which Tamm (jointly with Ginzburg) worked on layer cores for magnets [32] needed in radio engineering (and Papaleksi working at the Lebedev Institute made use of this work); they also worked on variations of the terrestrial magnetic field, which was of no use to defence. At the request of Kurchatov, Igor' Evgen'evich calculated complex magnetic fields of ships, etc.

Nevertheless, Tamm continued to work intensively in Kazan on the problems of the theory of particles and nuclear forces. Like Ginzburg, in addition to his work of practical value (on propagation of radio waves in the

† One must recall how, during the several years of his terrible illness which led to his death, Igor' Evgen'evich was partly paralysed and connected to an artificial respirator, but nevertheless continued his scientific work actively. Somebody who expressed surprise about this in the presence of Leontovich received the answer: "And what else could save him in this situation?"

I E Tamm with his wife N V Tamm (dacha in Zhukovka, 1967)

ionosphere), Tamm continued to pursue his interest in the theory of a particle capable of assuming states with different spins and the two of them began to cooperate. The result was a paper [33] in which they proposed a relativistic equation for a particle with a variable spin. There were some unsatisfactory elements of this work, which were eliminated later when the approach was generalised by Gel'fand and Yaglom who proposed their own equation. Igor' Evgen'evich, not satisfied with the results, held up the publication of this work till 1947.

But what did Tamm do for the four years of the War? Little on the scale of this outstanding theoretician, and narrowly applied work was mentioned above. One should add also some participation in the work of the laboratory of Landsberg, as a result of which the Academy in Kazan designed 'stiloscopes', which were instruments for fast spectroscopic analysis of the composition of a metal (very badly needed at the front for rapid sorting of metals from the damaged equipment, so as to separate the valuable kinds of steel from the general scrap). Igor' Evgen'evich helped in the design of optical systems, etc. We have seen that, apart from this 'small change' which he tackled with enthusiasm, there was only the joint paper with Ginzburg on particles with higher spins. What else? I can only state as an eyewitness that all the time Igor' Evgen'evich was working at full tilt, in spite of the cirumstances. One should quote here from the reminscences of V J Frenkel, then a young boy, whose parents were close friends of Tamm. He describes ''One of the evenings with the Tamm family'' [34]: ''Igor' Evgen'evich sat on some tiny child's bench, Natal'ya Vasil'evna, his wife, took care of the housekeeping, and her father, a very old man with a broad and thick beard, repaired women's shoes (this was nothing unusual in the families of scientists — ELF)... When we came with mother, Igor' Evgen'evich jumped, greeted us, said a few words, and then excused himself and sat down again on the bench with an exercise book on his knees. 'We will not disturb you if we talk?' asked my mother. 'No, no, no, talk please, pay no attention to me!' 'Gora (this was what his wife and childhood friends called Igor' Evgen'evich — ELF) can switch off completely' explained Natal'ya Vasil'evna to my mother''. In other reminiscences one can find that a pile of potatoes was lying on the floor in the corner of the room, a characterisic detail typical in the hard life during evacuation, even in the case of outstanding scientists.† (However, the Lebedev Institute had returned to Moscow in the autumn of 1943 and the situation was easier there. At least the food ration cards were not useless bits of paper and one could indeed buy some products, although in very modest amounts.)

One may assume that the result of that intensive work was a long and very important paper [35], published immediately after the War (submitted on 27 August 1945). Igor' Evgen'evich proposed in that paper an approximate method for a relativistic approach to the nuclear forces effected by exchange of pions of different types. In the world literature this is known as the Tamm – Dancoff method (because some five years later it was rediscovered in the USA by Dancoff [36]). Igor' Evgen'evich personally called this the method of truncated or cutoff equations. The method consists of the following: in a system of equations obtained by expanding as a series in terms of the constant of the exact equation, only (for example) the first two equations are retained and the term in the second equation coupling it to the later equations is dropped. The retained system of two equations is then reduced to one integrodifferential equation for the wave functions and the energies of states. This equation is solved exactly without recourse to perturbation theory (Igor' Evgen'evich did not know and nobody told him that, back in 1934, Fock adopted a similar approach to electrodynamics.) This equation was used by Tamm to investigate the stability of a coupled proton – neutron system for different types of forces (i.e. for different types of pions transferring the interaction).

This method attracted the attention of many authors. For example, Cini [37] proposed a covariant form of the method and discussed the problem of renormalisation, and Dyson devoted three papers to its relativistic generalisation, the result of which was the 'new Tamm – Dancoff method' [38]. Obviously, the method is imperfect, but it is somewhat better than perturbation theory. Its relativistic generalisation is essentially equivalent to the Bethe – Salpeter equation, which appeared later. Igor' Evgen'evich returned to the problem much later in two papers published in 1952 and 1955. They were published jointly with young (then) Silin and Fainberg, members of the staff of the Theoretical Division of the Lebedev Institute. The first of these papers dealt also with a new topic, which will be discussed later, of introduction of nucleon 'isobars', which are unstable particles (for the first time a relativistic equation for particles with spin 3/2 was used here because an excited state of a nucleon was regarded as such a particle). The second paper was concerned with the method of truncated equations applied to the scattering of pions by nucleons.

† In an excellent novel *Life and Fate*, Vasilii Grossmann — who was at the front and obviously had a poor understanding of the life behind the front — describes some prominent physicist who was evacuated with his institute to Kazan. In one beautiful scene the physicist's daughter brings two kilograms of cream butter from a special shop for outstanding scientists. This is an improbable fantasy, at least in the case of Kazan. Apart from the Tamm family, I knew closely one other family of a corresponding member of the Academy and can state categorically that there was no special shop for such scientists. All of them were as hungry as other scientists and they never set their eyes on a luxury such as butter. Additional goods for scientists of all specialities, and for those in the arts, writers, composers, etc. became available only in 1945.

* * *

Having reached this point, the reader will be justified in asking the question why, having formulated this method published in 1945, Igor' Evgen'evich only returned to its application 7 years later? What happened in the intervening years? The answer is: much happened.

Among Soviet theoreticians of any significance, Tamm was one of the very few who was not immediately assigned to the work on nuclear weapons. In spite of the participation of many remarkable physicists in this work, there was an acute shortage of scientists. Even postgraduate students returning from the front and any specialists in related fields, who mastered quickly the new subject, were taken on. However, Igor' Evgen'evich was ignored. The reason for this is easy to find. I think that the following data in his personal file played a role: his brother was an 'enemy of the nation' and shown to be such to the whole country; those near to him, friends, were also 'enemies'; he himself was a former Menshevik, etc. He was 'bad' and was not worthy of trust. Even in 1946, when many new academicians were elected, he was not. The efforts of the then President of the Soviet Academy of Sciences, Vavilov, resulted in the election as full academicians of the excellent physicists Landau, Leontovich, Landsberg, and Skobel'tsyn. But not Igor' Evgen'evich, although he had a strong and internationally very high reputation and by that time he had published all his major work. He was a specialist in the theory of the atomic nucleus, but readily tackled a great variety of other subjects. In reminiscing about Tamm, I had already written that he always tackled the most difficult and most topical problems and, frequently, after intensive work, he failed. After many weeks or months of such work, he would come disappointed to the Division and request: "Would you please give me some little specific problem because I need a hair of the dog that bit me after hard drinking!" This was the origin of two papers written jointly with Brekhovskii [39, 40]. Such a little problem he solved with the ease of a first-class professional. In fact, the number of problems of this kind to be solved in the atomic field was uncountable. However, Igor' Evgen'evich was left outside.

The reader might also be surprised about the statement made above that certain academicians were elected "due to the efforts of Vavilov". It is now difficult to believe that the lists of candidates had to be approved by the Central Committee of the Communist Party. The list used to be divided by the Committee into 'desirables' and 'undesirables'. The leaders of the Academy should have disputed these or others and should have presented arguments in favour of those 'desirable' or 'permissible'. However, after the resolution of the Central Committee no member of the Party or even academicians not belonging to the Party would have dared to vote against the Central Committee's resolutions (although the vote was secret). In Tamm's case it was known that in 1946 his name was crossed out personally by Politburo member Zhdanov. Zhdanov was in charge of ideological matters and Igor' Evgen'evich was branded as a 'bourgeois idealist'. This was the situation until Stalin died. After his death the guidance of the Central Committee remained a permanent feature, although in a slightly softer form. On the election day in each division about an hour before the meeting the academicians and corresponding members belonging to the Party met and in the presence of a representative of the Science Division of the Central Committee were informed who, in the opinion of that Committee, should be supported and who should be rejected. These recommendations were carried out without fail and they were decisive. However, let us return to the work done by Tamm.

Time was passing, and in 1946 he was asked to participate, on a small scale, in the assessment of some (but not the main) topics in the 'closed' (i.e. classified) subject of the atomic problem. Such was the origin of a paper on the front of a shock wave, published many years later [41] (with an indication that the work was carried out in 1947), and of another paper [42] on high-energy particle accelerators, which at the time was regarded as belonging to atomic physics and therefore secret. In the latter case a footnote indicates that the accelerator work was done in 1947–48.

When a new problem of constructing thermonuclear weapons was faced Kurchatov, who headed all the scientific work on nuclear problems relating to such weapons, was able to convince the 'appropriate persons' of the need to use Tamm's talents. (By that time Zhdanov was dead.) As we all now know from Sakharov's *Reminiscences,* Igor' Evgen'evich was given the task of organising, in the Theoretical Division of the Lebedev Institute, a group for 'support' or 'verification' of the theoretical work which was being done at that time by Zel'dovich's team. The Lebedev Institute group included Ginzburg, Belen'kii, Sakharov who had just finished his postgraduate studies, and a postgraduate student, Fradkin; they were soon joined by Romanov and Fainberg who graduated from the Moscow Engineering-Physics Institute. For all of them the problem was completely new. However, it may be because of this, free of the preconceptions that have become established in Edward Teller's team in the United States and here in Zel'dovich's team, that the group at the Lebedev Institute obtained a result which was fantastically unexpected: instead of 'support', Sakharov and Ginzburg in just two months put forward two decisive and completely new ideas. As is well known, Sakharov's idea was that reacting deuterium (or, in a different variant, deuterium and tritium) should surround a detonator in the form of a uranium or plutonium bomb, but not with just one layer (because then due to the slowness of the $d + d$ nuclear reaction and even of the hundred times faster $d + t$ reaction, the reactive material does not react completely and flies apart uselessly), but with layers of $d + d$ and $d + t$, alternating with layers of natural uranium. This layer structure makes possible a productive reaction: deuterium and tritium do not fly apart because fast neutrons cause fission in the uranium layers, which — because of the heating and total ionisation of the atoms — produce an enormous confining pressure. Ginzburg's idea related to the difficult problem of how the decaying radioactive tritium should be introduced into the system. He solved the problem very elegantly: one should introduce, ^{6}Li (in the form of solid lithium deuteride, ^{6}LiD), which — bombarded with neutrons — yields the necessary tritium and helium, and an even greater energy is released.

The simplicity of these basic ideas did not solve many physical (and even more so technological) problems which required intensive complex research. This was work to which Igor' Evgen'evich and the whole of his group devoted all their efforts and time even during the first period, when the whole group was working in Moscow. This was especially true after Tamm, Sakharov, and Romanov were sent in March 1950 to the Nuclear Research Centre (headed by Khariton), now known as 'Arzamas-16'.

It is not possible to give here a full account of the work of Igor' Evgen'evich on this 'main subject' of the years in question, which was the construction of the hydrogen bomb. All that can be said is formulated beautifully in a contribution of Khariton et al. [43]. We learn from this reminiscence that Igor' Evgen'evich played an outstanding role as a leader and a 'guide' of a large team, encouraging others, and yet intervening all the time in a decisive way in a large number of continuously encountered specific problems. The spectrum of the topics he dealt with is strikingly wide: from fine and complex physical problems to those essentially of the organisational nature. By way of example, one can mention that it was he with some of his colleagues who rapidly but carefully analysed all the meteorological data arriving before the first explosion in order to determine the direction that the terrible radioactive fallout would follow. They had an enormous responsibility: the results of their analysis were used to decide the moment of the test. If they had been wrong, the destructive tail of the enormous cloud would have reached a region where many thousands of people were living. As far as physics is concerned, it is sufficient to mention here just one of the problems that Igor' Evgen'evich had to solve [43].

Right from the beginning, in spite of the apparent simplicity of the two main ideas put forward by Sakharov and Ginzburg, there remained an important question: what happens immediately after these ideas operate, as expected, in the layer structure at a singularity formed in this way: how does a δ-like element transform later into a continuous process? The problem was so difficult and obscure that Igor' Evgen'evich decided to consult Academician Fock, a physicist and mathematician of the highest level, but not participating formally in the hydrogen bomb product. He obtained permission for this consultation. However, even this failed. The solution required not only professional knowledge of the highest grade, but exceptional intuition in order to understand the processes taking place. In the end, Igor' Evgen'evich succeeded in solving the problem and thus opened up the way for further progress. Tamm's group, and the group of Zel'dovich working in parallel, had to overcome continuously difficult theoretical problems which they faced again and again.

Working under this enormous stress, Igor' Evgen'evich nevertheless found the time to follow the scientific literature on the fundamental problems in physics. He would come to Moscow and participate in the work of the general seminar at the Theoretical Division and, moreover, he brought in young colleagues Silin and Fainberg to work in cooperation on further developments of his 'truncated equation' method, i.e. the Tamm — Dancoff method. The result was a joint paper [44], published before Igor' Evgen'evich returned finally to Moscow (this happened a few months after the successful test of the first hydrogen bomb in August 1953).

However, this is still an incomplete account of the intensive scientific work of Igor' Evgen'evich, who by that time was 55. There were two more fields in which he was working.

First, there was development of the idea of controlled thermonuclear fusion in a 'magnetic' thermonuclear reactor, now known as the Tokamak. The idea for this reactor was put forward in 1954. It is usual to call it the idea of Sakharov and Tamm, and in the seventies and eighties when Sakharov was in great disfavour, the name of Sakharov was omitted completely. But Igor' Evgen'evich always stressed that the idea belonged to Sakharov. I recall that when the words "The work of Tamm and Sakharov" were used, he would jump up in his place and shout "Sakharov and Tamm, Sakharov and Tamm!" stressing vocally Sakharov's name. Golovin recalls that at the first meeting of a high-level committee chaired by Beria, Sakharov mentioned briefly the suggestion and pointed out that the main calculations were carried out by Tamm. "Tamm was upset by this and, asking to speak, began to explain in an excited manner that the main ideas belonged to Sakharov and the main credit should be due to him. Beria impatiently waved his hand and interrupted with the words 'Nobody will forget Sakharov'" [45]. However, Sakharov was right: the main extensive calculations needed in this case and new specific ideas came from Igor' Evgen'evich.

In 1958 the work on Tokamak became public. A six-volume collection of the research done by that time was published. This collection opens with a paper by Igor' Evgen'evich [46] as Part I of the combined contribution of Sakharov and Tamm; this is followed by Part II written by Sakharov, and Part III, again by Tamm [47]. These detailed investigations made clear the fundamental initial ideas and provided a quantitative formulation. The work was done in 1951.

The first part is entitled "Properties of the high-temperature plasma in a magnetic field". The kinetic equation is used in this paper to consider the role of 'ordinary' diffusion and thermal diffusion. It is shown that the diffusion coefficient is four times larger than the thermal diffusion coefficient, i.e. the simple diffusion process dominates. This conclusion was of enormous importance for the subsquent work. Tamm considered the properties of plasma under the conditions when this conclusion was true and initially he neglected collisions. This approximation was later used extensively. But Igor' Evgen'evich later took account of collisions as well. An important feature of this work was also the discovery of a possible temperature jump at the wall of a chamber. The paper under discussion ends with "approximate small-model calculations". The next paper [47] is on the "Drift and thermal conductivity of plasma in a toroid in the presence of a stabilising current" and it represents further steps in the project towards a realistic system.

Half a century has passed since and we can see that realisation of this idea still requires much work, but physicists working in various countries have made much progress. As is well known, a giant international reactor project is being pursued by Russia, the USA, and Japan. In this connection I would like to mention one account of Igor' Evgen'evich on the psychological feelings of the physicists tackling the problem. All of them were in a state of hypnosis induced by the striking success of scientific predictions. In constructing the uranium and plutonium bombs, they worked through a mountain of very complex nuclear, gas dynamic, chemical, metallurgical, and other scientific problems, and also design problems, and found that everything worked excellently in the first tests, both in the USA and in the Soviet Union. Tackling the construction of the thermonuclear bomb required overcoming a moun-

L A Artsimovich, N Bohr, D A Rozhanskii, I E Tamm, and A P Aleksandrov (P N Lebedev Physical Institute, 1961; photograph taken by D S Pereverzev)

tain of new scientific and technological problems and once again all the result was 'an object' which operated at once, from the 'first presentation', as those in the industry would say. There was certainty that controlled thermonuclear fusion would be achieved as fast and as successfully. The first attempts were made by Artsimovich and a miracle happened! The experimentalists created a high-current gas discharge and achieved a 'pinch' in the discharge filament. They discovered that neutrons were emitted by the filament. The delight was universal. However, this was not shared by Artsimovich himself. He said that these were not the 'right' neutrons. Igor' Evgen'evich told me that for two weeks he tried to convince Artsimovich that the desired success was achieved. It seemed that all agreed with the theoretical estimates. However, Artsimovich held to his view. Finally, he convinced the others of the joyless truth. A period of furious attack on the problem, still continuing, began immediately. I recall also that the well-known Indian physicist Bhabha, who came to the Soviet Union in the middle of the fifties, said that he would lay a bet that the problem would be solved in the next two decades and that this would be done in the Soviet Union. Bhabha's tragic premature death prevented payment of his lost bet.

However, I said that during this period between the forties and fifties, apart from being preoccupied with the hydrogen bomb, Igor' Evgen'evich was active in two important spheres and I have described so far only one. The second was of completely different nature. It had no relation to the bomb, but dealt with the fundamental physics of particles. This was the idea of nucleon resonances, called 'nucleon isobars' by Igor' Evgen'evich himself. Briefly, the idea is as follows.

A little earlier, American experimentalists (including Fermi) investigated photoproduction of pions from nucleons and the scattering of pions by nucleons at energies up to hundreds of megaelectron volts. They discovered that the greatest contribution to these processses comes from the states of the nucleon — pion system with the isotopic spin 3/2 and the mechanical spin 3/2 or 1/2. This was sufficient for Igor' Evgen'evich to put forward a bold hypothesis of the existence of an unstable particle, which was a baryon with isotopic and mechanical spins of 3/2. He called this particle an 'isobar', decaying into a nucleon and a pion. Tamm, jointly with Gol'fand and Fainberg [48], put forward a relativistic pion – nucleon scattering theory (with an isobar as an intermediate step) and they selected the necessary interaction constants and the energy of the excited state of the isobar so as to obtain the correct (experimentally observed) angular distribution of pions at different energies. By that time the necessary data were available in very large amounts.

Their work required extensive numerical calculations, which in the case of the then available technical means (usually elderly mechanical calculators, supplemented only at the end of the work by Mercedes electric calculators) required a fantastic amount of work to select four unknown constants. However, this work was rewarded by the fact that they were able to describe well all the numerous experiments, but with one somewhat unpleasant result: the energy of the isobar level was only slightly greater than the width of this level.

This aspect made many Moscow theoreticians deeply skeptical. Would it be meaningful to consider such a state as a particle (for example, include this particle together with other stable particles in the Feynman diagrams)?

However, Igor' Evgen'evich thought out the calculations thoroughly and was inspired by the results. The work with isobars grew particularly rapidly after his final return to Moscow at the end of 1953. Several young members of the Division tackled different aspects (pion photoproduction, etc.). Once again, agreement with experiments was achieved with the same constants as before. Now we know that Igor' Evgen'evich was correct. The resonances (which is how the Tamm isobars are now known), with the level width of the same order as the separation of a level above the ground stable state, have now become full members of the enormous family of the known particles. The level (3/2, 3/2), predicted by Tamm and his colleagues, is the now well-known $\Delta(1236)$ resonance. This resonance was introduced by Igor' Evgen'evich, jointly with Silin and Fainberg [44], where — as mentioned above — the relativistic equation for a particle with spin 3/2 was used in a practical way for the first time.

* * *

Reviewing the first postwar period (1945–55) of Tamm's scientific work, we cannot but be struck by the boldness of the intuitively guessed ideas and the wide range of different branches of physics, both fundamental and applied, which he covered by his direct investigations and by organising the cooperative effort of a talented team of theoreticians.

We can see that Tamm's postwar decade was extremely productive in the scientific sense. Although a major place was taken up by applied physics, this physics was on an enormous scale both in the sense of its practical value and in respect of the breadth of the various problems Tamm dealt with. What was done by Igor' Evgen'evich was indeed physics of a very high level.

Turning now to his work on fundamental topics, we must above all mention something not discussed so far, although the work was carried out back in 1944 and published in 1945. This is the result of cooperation with Mandelstamm (who died in 1944), dealing with the time and energy indeterminacy relation [49]. As pointed out in that paper, in contrast to the momentum and coordinate indeterminacy relation, which follows from the Schrodinger formalism of quantum mechanics, the time and energy indeterminacy relation is usually based on Gedanken experiments and trivial time and frequency relationships. This gap was filled by Tamm and Mandelstamm [49]. They showed that the time interval Δt, in which observable physical quantities change in a nonstationary state by an amount of the order of the quantity itself, on the one hand, and uncertainty of the energy ΔE of this state, on the other, are linked by the required indeterminacy relation and that this result follows from the commutation relation between the Hamiltonian operator and the operator representing a given variable. Moreover, they demonstrated that the indeterminacy relations usually considered for a pure state remain valid also for a mixed state. The relationship $\Delta E \Delta t \sim \hbar$ becomes a formal consequence of quantum mechanics.

Two more important contributions to fundamental physics, mentioned above, were made by Igor' Evgen'evich during this period. The first is the development of the approximate truncated equation (Tamm–Dancoff) method and its application to the interaction of nucleons (for details of this method, see Ref. [50]). The second is the bold idea of nucleon isobars, i.e. of resonances representing excited states of a nucleon decaying by pion emission. Igor' Evgen'evich had to support this idea by a series of investigations (together with young colleagues), and defended it with the same ardour and with equally high authorities as in his youth, when nobody believed his idea that the neutron has a magnetic moment. Unfortunately no one now remembers that it was Tamm who introduced resonances into the physics of elementary particles and demonstrated the fruitfulness of this concept in accounting for specific processes.

However, this period is characterised also by an avalanche of honours and gifts, awards and titles which were showered on Igor' Evgen'evich. At last he was elected an Academician. The death of Stalin removed from the country much of the terror in which all lived. Igor' Evgen'evich became *persona grata*. But he did remain a deeply democratic man. Money attached to various prizes he gave away extensively to young scientists in need, he personally sought them out. He began to travel abroad for a variety of reasons, for talks on cooperation with the Academy of Sciences in the German Democratic Republic, to receive the Nobel Prize in Sweden, to the Pugwash conferences (four times) in the USA and England, to the International Conference on Peaceful Uses of Atomic Energy in Switzerland, and once again to Switzerland to a conference on high-energy physics and to a meeting of experts on nuclear disarmament. He travelled also to India, France, Japan, China, ...

For the first time his passion for travelling, for seeing new countries and new people, could be satisfied. He was elected to a number of foreign academies, including such prestigious ones as the National Academy of Sciences in the USA. This was a happy period also because the country at home began to feel slightly happier. However, in practice for some time the purely scientific work of Tamm, done at home at his own table, stopped. Naturally, the love of science ruled him as before. Frank mentions in his *Reminiscences* [51] that even in Stockholm, in the middle of the celebrations accompanying the investiture with the Nobel Prize, when Igor' Evgen'evich heard from others about some new interesting experiments, he sat working at night in order to understand the theoretical meaning (unfortunately he was not able to achieve this because the rumour about the experiment proved incorrect). However, he felt bad when he could not do real scientific work. The main problem was the lack of new ideas and with his nature, he needed major ideas and not a 'small change' which he could still do. He wrote papers reminiscing about his friends. He used his authority to fight for science, particularly against Lysenko. He was attracted by the new discoveries in molecular genetics. He studied related work and tried to do something himself by guessing the genetic code, but he was overtaken by Gamow. Finally, in 1964 a new idea that he needed seemed to arrive and it conquered him.

Before we deal with this idea, we need to consider the situation then ruling in theoretical physics. The inability to go beyond perturbation theory in the physics of strong interactions, on the one hand, and the important observation of Fradkin, and of Landau and Pomeranchuk (1955) on the tendency of the particle charge to go to zero

('Moscow zero'), on the other, undermined the faith in quantum field theory. At the Kiev International Conference on High-Energy Physics in 1959, Landau said (this was repeated in his paper in a collection published to honour Pauli [52]): "The Hamiltonian is a corpse and it should be buried with all due respect". The need to develop a completely new theory, not based on the orthodox quantum field theory, seemed unavoidable to many. Beginning from approximately 1955 for a period of about 15 years the best theoreticians sought unsuccessfully to replace this field theory. Only a few of them regarded the objections against the traditional approach as not solid enough and the 'Moscow zero' as insufficiently rigorous for such fundamental conclusions. Igor' Evgen'evich, probably because such fundamental changes had already occurred in his lifetime, also supported the 'revolutionary' point of view. In 1965, at a conference in Japan to celebrate the 30th anniversary of Yukawa's prediction of the pion, he said [53]: "It had now become clear that the development of physics had led us to a point where it has become necessary to alter some of our fundamental physical ideas and that the change should be as radical as the establishment of the theory of relativity and of quantum mechanics".

This period has seen a large number of 'mad' theories, beginning with the work of Heisenberg (1957), based on the nonlinear Dirac equation, from which Heisenberg deduced reasonable values of the 'charges': electric, strong-interaction, and gravitational. However, Bohr not without reason called this theory 'insufficiently mad'. It had to be dropped. The axiomatic S matrix method, nonlocal theories, etc. were also competing but the target remained unattainable.

Igor' Evgen'evich concentrated his attention on the fact that at high energies the measurement of the coordinates of a particle by means of another (test) particle meets with a difficulty which makes it impossible to measure the particle coordinate: the process of measurement creates many new particles, including those which are unstable and decay far from the measurement (interaction) point. Therefore, a limit is set on the process of measuring the coordinate. Hence, Tamm concluded that there should be an indeterminacy relationship between the components of the coordinates. However, the particle momenta can be measured by determination of, for example, deflection in a magnetic field. Therefore, in the case of the momentum components there is no indeterminacy relation. Consequently, following the work of Snyder (1947), Tamm developed a theory valid in the momentum space. However, Snyder was unsuccesful because of certain specific divergences (in integration over the angles). Igor' Evgen'evich modified this theory by introducing a momentum space with a variable curvature, which eliminated these divergences, but the theory was then unbelievably difficult to investigate. Only the high professionalism, in combination with the excellent mastery of the required mathematics, enabled Igor' Evgen'evich (and his younger colleague Vologodskii who joined him) to make any progress. The difficulties were not only mathematical, but also fundamental. Tamm worked as addicted and with the same ardour as in his young days. In two years he was brought down by a terrible incurable disease. Soon he had to lie down, the nerves controlling his diaphragm became paralysed, and he had to be connected to an artificial respirator in order to save his life. This machine pumped air into his lungs continuously, day and night. However, this did not break Tamm. For more than two years he continued to work intensively. He got up from the bed, sat behind a table where there was a second portable machine, and continued unbelievably complex calculations on the basis of a great many variants of the theory which he developed (I myself saw the numbering of the pages of these calculations: at the end there were more than 3 000 pages!). This enormous work and infinite inventiveness in overcoming continuously appearing difficulties went on for six years.

Igor' Evgen'evich himself cursed his obstinacy, he already felt that the fundamental difficulties could not be overcome, but he could not 'kill' finally his idea, and the calculations continued. Naturally, during the period of his illness this work had a psychologically healing effect and gave reasons for living, but even so the work remained incomplete. The approach was like a blind alley. What was nevertheless achieved, was published only after his death [54].

However, in discussing this period one must mention one more paper which he wrote then. In 1967, the USSR Academy of Sciences awarded the Lomonosov Gold Medal to Igor' Evgen'evich. This is the highest scientific prize given by the Academy. Regulations said that the award winner should follow the receipt of this medal at a general meeting of the Academy with a speech about his work, an overview of the situation in the selective branch of physics, and his predictions on the future investigations. This ceremony and the speech should have taken place in March 1968, when the seriously sick Igor' Evgen'evich was 'chained' to his respirator. Many thought that this speech could no longer be written by Tamm and at best they would expect his colleagues to prepare it. However, Igor' Evgen'evich decided to write it and naturally he did it himself. At a general meeting, his speech was read by Sakharov. All those that heard it were sure that the author was Igor' Evgen'evich. The breadth of his views, the specific points of view, and the optimism throughout the whole text, were surprisingly typical of himself. He ended with the words: "I hope that I shall live to see a new stage in the theory, no matter what that stage might be".

He did not. Igor' Evgen'evich died on 12 April 1971.

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