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Reports of scientific activities for 1946-1955

P L Kapitza

On 17 August 1946 Stalin signed a resolution of the Council of Ministers of the Soviet Union No. 1815-782 s "On the manufacture of oxygen by the method of Academician Kapitza". Only a year after Kapitza was given the title of Hero of the Socialist Labour 'for successful scientific development of a new turbine oxygen-production method", he was removed from the posts of head of Glavkislorod (M ain Administration of the Oxygen Industry) and Director of the Institute of Physical Problems 'for failing to carry out the government directives on the development of the oxygen industry in the Soviet Union, to use the existing advanced foreign oxygen technology, and to take heed of the proposals of Soviet specialists".

This was the punishment for the refusal to participate in the development of the Soviet atomic bomb and for strong attacks, in Kapitza's letter to Stalin, on Beria who led the atomic project. Beria asked for the arrest of Kapitza, to which Stalin answered as follows (according to A V Khrulev, who was an accidental witness of the exchange between the two comrades): 'I shall remove him [from his posts], but don't you touch him'. Stalin explained why Kapitza should not be 'touched': there would be a major international scandal Stalin kept his promise.

Kapitza learnt about this conversation of Stalin with Beria only after the death of the former and the arrest of the latter. He was told about this by General Khrulev, whom he knew very well.

The period of disgrace of the great Russian scientist and engineer began on 18 August 1946. What he did in these years in his 'hut laboratory' at Nikolina Gora is reported by himself in his 'personal reports of an academician' which he sent to the Division of Physicomathematical Sciences, in accordance with the statute of the USSR Academy of Sciences. These reports have not been published before, even as excerpts.

P E Rubinin

Year 1946

Nikolina Gora Moscow Province

6 July 1947

Up to 18 August 1946, I was still Director of the Institute of Physical Problems. I carried out the following work, apart from general supervision of the research at the Institute:

1. In cooperation with a team of colleagues, I finished developing apparatus for the generation of gaseous oxygen by a low-pressure method I have proposed. An installation was built for this purpose which is capable of producing 80 m^3 of 95% gaseous oxygen per hour. This prototype was investigated and the parameters necessary for the design of large installations of this type were obtained. I demonstrated the feasibility and advantages of this low-pressure method for the large-scale production of gaseous oxygen, needed in metallurgy, the chemical industry, and other industries.

2. I developed an instrument for continuous measurement of the percentage content of oxygen in a gas. This instrument operates on the basis of a difference between the magnetic susceptibilities of oxygen and other gases. The instrument was called an 'oximeter' and an author's certificate [Soviet patent] was obtained.

3. I investigated galvanomagnetic effects in a number of metals subjected to strong magnetic fields, right down to ultralow temperatures. A new relationship was found for the change in the resistance of a metal, manifested in the cubic system of a crystal and capable of yielding the link between the effects of temperature and crystal lattice distortions (caused by plastic deformation and the presence of impurity atoms). This work has now been interrupted, but all the results obtained for lithium and aluminium, as well as for some other metals, are sufficiently complete for the publication of a part of the work.[†]

4. I also led the work on the methods for rectification of gaseous mixtures at low temperatures. The work has been interrupted and is incomplete. The results obtained are interesting but insufficient for publication.

From 18 August 1946, in view of my inability to continue the experimental work and after an illness lasting two months, I turned to theoretical investigations of hydrodynamics. The

[†] Kapitza did not return to this work. However, the direction set out by his investigations had been continued, to some extent, by his postgraduate student R A Chentsov. Chentsov published a paper "On the change in the electric resistance of tellurium in a magnetic field at low temperatures" [Zh. Eksp. Teor. Fiz. 180 (4) 374 (1948)], in which he described the reduction in the electric resistance of tellurium in a magnetic field at liquid helium temperatures.



The physical problems hut at Nikolina Gora in the late forties.

first paper on this subject "Theoretical and empirical expressions for heat transfer in two-dimensional turbulent flow" was published in *Dokl. Ak ad. Nauk SS SR* (Vol. 60, No. 7) and was sent for publication on 10 January 1947.[†]

I have been working on a monograph *Osnovy Tekhniki Glubokogo Kholoda* (Fundamentals of Deep Cooling Technology), and in the last year I wrote an equivalent of about 200 printed pages.

In 1946 I reported the following inventions related to the construction of installations for the production of gaseous oxygen by the low-pressure method:

(i) method and instrumentation for improving the operation of heat exchanges in refrigeration installations;(ii) a method for heating a refrigeration system;

(ii) a method for heating a renigeration syste

(iii) instrumentation for the improvement of the trapping of the gas escaping from the cold ends of regenerators, in the form of components precipitated in the solid state;

(iv) an electromagnetic gas analyser (oximeter).

[†]In a reprint of this paper, sent in August 1948 to Stalin together with reprints of papers reporting other work carried out in the hut laboratory at Nikolina Gora, Kapitza wrote "'Not a scientist who is doing research work, but a scientist who cannot do this work'. To comrade I V Stalin with profound respect and best wishes from P Kapitza. 6 August 1948."

With all the 'respect' in Kapitza's dedication, the very fact of sending a 'gift' of scientific work to the man who had driven the scientist out of his Institute and his laboratory, represented in effect throwing down a *challenge* and an especially daring one. The letter sent by Petr Leonidovich to Stalin together with the reprints began with the words: "Two years have passed since I have been prevented from doing scientific work of real value", as quoted in P L Kapitza *Pis'ma o Nauke* (Letters on Science), Moscow (1989), on p. 281.

In the course of the whole of 1946 I continued editing *Journal of Physics*.‡

P L Kapitza

Year 1947

Nikolina Gora Moscow Province

March 1948

Unable to continue my scientific work on the superfluidity of helium, strong magnetic fields, and low temperatures, I occupied myself mainly with the problems of theoretical hydrodynamics.

I completed and published the following papers: "Theoretical and empirical expressions for the transfer of heat in two-dimensional turbulent flow"; "Wave flow of thin viscous-liquid layers. Part I. Free flow. Part II. Flow in contact with gas streams and heat transfer".

They were published in *Zh*. *Eksp. Teor. Fiz.* (Vol. 18, No. 1, 1948).

The last two papers deal with the influence of surface tension on the flow of thin viscous-liquid layers. The theoretical results demonstrate that what is usually regarded as laminar flow of a liquid is in fact wave flow and is more stable. The existence of this steady-state wave flow makes it possible to explain and estimate quantitatively the magnitude of some of the effects which have not

‡From 1942 to 1947 P L Kapitza was the Editor responsible for the Soviet English-language physics journal called *Journal of Physics*.



P L Kapitza at the laboratory at Nikolina Gora. 1948.

yet been understood, for example, an enhanced thermal conductivity of thin liquid layers, easy removal of a liquid by a counterpropagating gas, etc.

In spite of the very modest means available to me, in my laboratory in a room at the dacha (without the help of assistants), I was able to check experimentally the theories of these phenomena and even detect the existence of single capillary waves.

Naturally, the work has been slow, but it is now complete and ready for publication.

I prepared and presented a course of general physics at the Physicotechnical Department of the Moscow State University, where I hold a Chair.

In the last year I was elected Honorary Fellow of the Indian Academy of Sciences and of the Royal Irish Academy.

P L Kapitza

Year 1948

30 April 1949†

Since I have been prevented from continuing my scientific research on the superfluidity of helium, strong magnetic

†This report was sent to the Division of the Physicomathematical Sciences of the USSR Academy of Sciences with the following letter to A F Ioffe, Academician Secretary of the Division: "Dear Abram Fedorovich,

"In answer to the second request from the Division, dated 18 April 1949, No. 21-7, for an account of my scientific work for 1948, I attach the report.

fields, and low temperatures, I have occupied myself mainly with problems in theoretical hydrodynamics ‡

The past years have also shown that the method I developed for the production of oxygen in a low-pressure cycle is entirely dominating world technology. Three years ago, in 1946, my oxygen production method was rejected in the Soviet Union in favour of the German high-pressure method of the Linde Company.

This is what has happened since.

1. The performance parameters of the Linde machines. obtained in the Soviet Union for the installations brought as a war booty and installed and commissioned in Tula, as well as those obtained by the Americans who have investigated systematically these machines in the Western zone [of occupied Germany] and published in The Chemical

"I am somewhat surprised by your persistent requests for reports. After all, you know well that I have been removed not only from contact with my students and colleagues, but also from all access to scientific apparatus for the study of strong magnetic fields and deep cooling, and I have been prevented from continuing the work on the subjects in which my scientific research has been widely recognised. Therefore, I have been forced to seek new directions for my scientific work.

"In the development of that work which I am doing at present, in spite of my modest personal requests to you, I have received no help from the Division (the Academy has now cut by half even the small grant I have asked for). Thus, you are confining yourself to requests for reports on my scientific work without the slightest effort to help in this work.

"Such a formal attitude of the Physical Division of the Academy of Sciences to my scientific work is in conflict with [the part of] the Statute of the Academy [which says] that the Division you are heading should make all efforts to develop physics in the Soviet Union." 30 April 1949

P L Kapitza

[‡] The part of the report, repeating literally the report for 1947, is omitted.

Age [56 835 (1947)], all agree and show that the performance parameters claimed by the Germans for all types of Linde machines have been exaggerated by a factor of 1.5-2: they have been simply dishonest (abroad this is called advertising).

Even three years ago I have been suspicious about these parameters and have unsuccessfully demanded checking them, but these false data have been used as the basis of the resolution of the Council of Ministers of August 1946 stopping my work.

2. On the other hand, American scientists have reconstructed my expansion turbine and published the results of a comprehensive investigation of this turbine in a long paper in the well-known journal *Transactions of the American Institute of Chemical Engineers* (Vol. 43, No. 2, 1947). Their pub-lished results confirm fully the high performance parameters which I obtained and have thus confirmed independently the correctness of my main ideas and scientific work.

3. This work has evidently led to a rapid changeover of the American technology to my machines in the construction of large-scale installations. Now America, and according to the latest information also England and France, are constructing enormous oxygen installations, all based on my low-pressure cycle with my expansion turbine. For example, in America they are already building units capable of producing $60\,000\,\text{m}^3$ of oxygen per hour (*Scientific Ame rican*, February 1948) and oxygen is being introduced in large amounts in the main branches of the national economy, such as metallurgy, synthetic fields, gasification, etc.

Finally, the clearest proof of the superiority of my direction in the oxygen production technology is that, as has become known recently, the Linde firm itself (more specifically, its division in America) is also going over to the con-struction of large machines based on my low-pressure cycle.

We now have a ridiculous situation: the whole world is constructing our Soviet machines, but here the work on them has been stopped, the inventor has been prevented from working in a useful way, and we are slavishly copying German machines made by the Linde company, while this company is stopping the construction of such machines. Not only are we showing a slavish attitude to the German machines, but also to please Germany we have lost our Soviet, now undoubtedly leading, direction in science. With this attitude to our own progress in science it will be difficult to overtake foreign technology. It should be mentioned here that the oxygen problem is not a small narrow part of modern science, but—as demonstrated recently—this problem in fact determines one of the most fundamental trends in world technology.

Academician P L Kapitza

Year 1949

29 January 1950

1. In the past year I published a paper on "wave flow of thin viscous-liquid layers [Part] III. Experimental investigation of wave flow regime" by P L Kapitza and S P Kapitza [Zh. Eksp. Teor. Fiz. **19** (2) (1949)].

This is a detailed experimental investigation of a new type of wave motion of a viscous liquid in thin layers, which I predicted theoretically and described in a paper published in 1948. In this experimental work not only were we able to develop a method for photographing the profiles of waves of this new type of flow, but also to check experimentally the theoretically derived expressions governing the relationship between the wavelength, phase velocity, and critical number at which this type of flow appears, as a function of the viscosity, surface tension, and density of a liquid.

In addition to the theoretically predicted periodic regime, the experiments made it possible to detect and study the regime in which single waves are formed.

My theoretical and experimental work changes entirely the generally accepted point of view on the flow of viscous liquids in thin layers, which has been so far regarded as entirely laminar. It follows from my work that in fact this is wave flow and the nature of flow is influenced decisively by the surface tension.

Since the flow of a liquid in thin layers occurs in the majority of the most important technological apparatus, such as scrubbers, boilers, etc., the phenomena discovered by me and their theory alter radically our view on the physics of thermal, diffusion, and other exchange processes which occur in such apparatus between the liquid and gaseous phases. The theoretical expressions derived by me open up the feasibility of more rigorous calculations relating to these exchange phenomena, so as to take into account the important role of the surface tension, which has not been done before.

Therefore, in spite of the fact that my work has just been published, I have heard reports of successful applications of my results in more rigorous and complete calculations relating to chemical and thermochemical apparatus, which obviously should improve its design and operation.

2. In the last year I published a paper "On the problem of formation of ocean waves by wind" [Dokl. Ak ad. Nauk SSSR 64 (4) (1949)].

This work deals with the problem of formation of ocean waves by wind. It is interesting to note that the mechanism of the effects of wind on ocean waves proposed so far is insufficiently effective to explain how wind transfers the considerable power necessary to maintain ocean waves. The mechanism proposed by me in this paper is based on the assumption that air does not flow smoothly around waves, but breaks up the waves at the crests. It proves that such a description of the mechanism is amenable to calculation and it leads to expressions which give the necessary power to maintain ocean waves.

Without recourse to any arbitrary quantities, the theoretical expressions give quantitative results that agree with those found experimentally.

3. In the last year I held the Chair of General Physics at the Physicotechnical Department of the Moscow State University and presented a course of general physics (on electricity and optics) in the second year. I am beginning to prepare this course of lectures for publication.[†]

P L Kaptiza

†Kapitza's lectures on general physics, which he presented at the Physicotechnical Department of the Moscow State University in 1947–1949, had not been published. Shorthand records of these lectures are now being prepared for press.



In the workshop of the physical problems hut. December 1954.

Year 1950

13 April 1951

At the beginning of 1950 I ceased to lecture on general physics and left my Chair of General Physics at the Physicotechnical Department of the Moscow State University, because order No. 40 of 6 February 1950 by the Rector of the University stopped my work in this Department "for lack of pedagogical commitment".[†]

From 1 July 1950 I joined the Institute of Crystallography of the USSR Academy of Sciences as a senior scientific worker with the duties of a consultant.

My main experimental work was done at my personal laboratory at Nikolina Gora on the subject of electronics and I obtained significant results. I reported this on 5 May 1950 in a note addressed to S I Vavilov, President of the USSR Academy of Sciences. The development of this work was acknowledged as desirable and the Presidium of the Academy took a number of measures in relation to this work. The work is continuing.

I completed a series of theoretical investigations in mechanics, hydrodynamics, and mathematics

At a general meeting of the Division of Physicomathematical Sciences of the USSR Academy of Sciences I read the

[†]Kapitza was absent from the ceremonial meetings at the USSR Academy of Sciences and Moscow State University in December 1949 on the occasion of Stalin's seventieth birthday. After an angry letter from the Prorector of Moscow State University, S A Khristianovich, who wrote on 28 December 1949 "You will agree that we cannot entrust the education of the young scientists to a person who is demonstrably in conflict with the whole of our nation", Kapitza was relieved of his duties at the University. following papers, which were accompanied by demonstrations:

1. "Wave motion of thin viscous-liquid layers", on 27 March 1950.‡

2. "Dynamic stability of a pendulum with a vibrating suspension", on 22 November 1950.

P L Kapitza

[‡]This was the first public appearance of P L Kapitza at a scientific session of the USSR Academy of Sciences since the day when he was removed as Director of the Institute of Physical Problems in August 1946. Up till then he in fact boycotted the Academy sessions. At least this was how it was regarded by some of his colleagues. "The whole country, all of us, all your friends and people close to you need you (wrote Petr Leonidovich's close friend I V Obreimov on 9 July 1948). Each person is valuable. And here Kapitza stands in majestic solitude, because he has been offended" The letter from Obreimov ended with the following words: "... The day when I will see you again at the Division, when I will read about your plans in the plans of the Academy, and when I will hear your lecture will be the happiest day. And if this letter will encourage you in this direction, I will be pleased that I have done one of the most important things in my life" [Petr Leonidovich Kapitza: Vospominaniya. Pis'ma. Dokumenty (Petr Leonidovich Kapitza: Reminiscences, Letters, Documents) (Moscow: Nauka, 1994) pp. 428-429]. However, Obreimov had to wait two more years. Kapitza presented a paper at the Division of Physicomathematical Sciences soon after. At the end of 1949 he ignored all the celebration meetings on the occasion of Stalin's seventieth birthday. To the man whose birthday was celebrated he explained his 'behaviour' in the following way (in a letter dated 3 January 1950): "... I do not come to sessions because I am in a poor nervous state and I find it difficult to consort with people who are rather afraid of me and avoid me' (ibid., p. 436). And who was responsible for being "rather afraid and avoiding"?Stalin and Beria above all. Thus Kapitza without any shadow of fear reminded Stalin about this time and time again.

Year 1951

10 April 1952

In 1951 I published the following papers:

1. "Calculation of the sums of negative even powers of the roots of the Bessel functions" *Dokl. Akad. Nauk SS SR* 77 (4) (1951).

2. "Dynamic stability of a pendulum with a vibrating point of suspension" Zh. Eksp. Teor. Fiz. **21** (5) (1951).

3. "Pendulum with a vibrating suspension" Usp. Fiz. Nauk 44 (1) (May 1951).

4. "Heat conduction and diffusion in a liquid medium during periodic flow. I. Determination of the coefficient of wave transport in a tube, slit, and channel." *Zh*. *Eksp. Te or. Fiz.* **21** (9) (1951).

I also completed four years of work on electronics. The future development of this work was considered on 27 December 1951 by an Academic Commission set up by the Presidium of the Academy of Sciences.

P L Kapitza

Year 1952

13 March 1953

In the past year (1952) I was working on experimental and theoretical aspects of electronics related to the solution of problems in the field of high-power electronics. The results of these investigations were presented to a Commission of the USSR Academy of Sciences which was set up by the Presidium of the Academy under the chairmanship of Academician A I Berg.

The conclusions reached by this Commission can be found in the records of the Academy and can be used to judge the importance of the results obtained.

Academician P L Kapitza

Year 1953

Physics Laboratoryof the USSR Academy of SciencesNikolina Gora21 January 1954

In the last year, like in the previous years, my work has concentrated mainly on problems in high-power electronics.

Over two years have passed since my letter of October[†] 1951 to the President of the Academy of Sciences Academician A N Nesmeyanov, reporting that I obtained a number of significant results in theoretical and experimental [investigations of] processes in physical electronics, which open up new avenues for the performance of a number of important tasks in high-power electronics. Therefore, I called the new topic 'high-power electronics'. The President of the Academy established in December 1951 a Commission to study my work. This Commission was chaired by Academician A I Berg and it included Academicians Vvedenskii, Fock, Leontovich, Lebedev and corresponding Members

Despite the favourable conclusions of the Commission, no real measures have yet been taken to improve the conditions of my work. After repeated requests and approaches the Presidium of the Academy of Sciences agreed a resolution on 28 August "On measures to help Academician P L Kapitza in his work". The main and perhaps the only result of this resolution, which is of decisive importance for immediate development of my work which I asked for particularly, has been the expansion of my laboratory at Nikolina Gora and the improvement of the conditions there. For this purpose the Presidium of the Academy of Sciences decided to expand the existing laboratory by constructing a small stone building, bring water from the RANIS§ village, provide central heating, connect a telephone, carry out maintenance repairs, etc. It must be mentioned that now the laboratory has been converted from the log cabin of a caretaker at the dacha and has stove heating, so that the temperature is uneven. I have been asking the Academy for several years to provide central heating. According to the Academy resolution, all these small works should have been finished by the domestic staff of the Academy by 1 January 1954. However, not only have they not been finished, but none of them has yet been started.

All this time from 28 August 1953 up to today I am steadily and continuously addressing the Presidium by letter or by word of mouth with the reminder that the resolution should be carried out, but without any results. It is obvious that under such conditions it is not possible to organise extensive work on high-power electronics.

In this report year I have also put forward a new important suggestion on high-power electronics, but this (like others) has not yet been realised. I called this suggestion the method of cumulative acceleration of charged elementary particles (letter to the President of the Academy on 12 January 1953). The method is as follows: if there is already a beam of fast-moving particles, a fairly simple electromagnetic interaction can transfer energy from some particles to others and thus, by reducing the number of particles in a beam, increase the velocity of the remaining ones. If some unforeseen technical difficulties are not encountered in the experimental realisation of this method, it would open up the opportunity of a very simple and uncumbersome way of reaching record particle velocities, which would be of major importance in a number of important tasks facing modern physics.

[†]This is an error: it should read *December*. On 20 October 1951 Kapitza wrote to A N Nesmeyanov the "Second note" on the progress of work relating to "Generation of high-power directed energy beams". The first note about this work was sent to S I Vavilov on 5 May 1950.

[‡]This was published in *Petr Leonidovich Kapitza: Vospominaniya. Pis'ma. Dokumenty* (Petr Leonidovich Kapitza: Reminiscences, Letters, Documents), pp 477–478.

[§]This is a resort village of scientists and artists at Nikolina Gora.

[¶] The idea of cumulative acceleration is a variant of what have subsequently been called collective acceleration methods (S Alwyn, V I Veksler, G I Budker). The interest in these methods has remained, but their practical applications depend largely on the generation of highintensity particle beams and even more on ensuring the stability of the process; see cf *Collective Methods of Acceleration* (Eds N Rostoker, M Reiser) (New York: Harwood Academic, 1979). (*Note by S P Kapitza.*)



At Nikolina Gora in the fifties.

On 15 May 1953 the President of the Academy of Sciences asked for a number of scientists from the Commission for a meeting and additionally Academicians Topchiev and Lavrent'ev were present. Although my proposal was not discussed in detail, all those present supported the need to supply me with means to realise this proposal. However, up to now the necessary conditions have not been provided. The large solenoid needed in this investigation has not been constructed and even if it had been constructed, there would have been nowhere to put it. The technical design and calculations of the solenoid were prepared by me on 1 May 1953.

This absence of help by the Academy of Sciences does not agree at all with our aim to develop leading science and has already led to the following sad occurrence. In my note on "High-power electronics", which I sent forward early in 1952 to the Commission in question, I pointed out in Chapter II on pp. 32-35 and on p. 37 that one of the possible ways of generating high-intensity radiation of wavelengths from 3 to 0.1 cm is what I call the 'electronic siren method'. A beam of very fast electrons moving at a velocity close to that of light is transmitted along a zigzagshaped magnetic field and it is then found that electrons can emit waves with these wavelengths. I called it 'electronic siren method' by analogy with the acoustic siren. In both cases the motion of a vibrating medium occurs at a velocity close to the velocity of radiation. Therefore, it is possible to generate highintensity radiation without cavities. It is now over two years since I have made the suggestion, which like other suggestions on high-power electronics, has not been put into practice.

During this time the scientists in the USA also discovered this method of generation of radiation and rapidly realised it experimentally, as described in a paper by H Motz, W Thorn, and R N Whitehurst "Experiments on radiation by fast electron beams" in *J. Appl. Phys.* **24** (7) (July 1953). Although the Americans have built such an electronic siren, they have not yet thought out how to generate (in this way) highly directed intense beams of highpower radiation, but since science develops in one common stream, they will undoubtedly sooner or later find how to do this as well. Science develops in one stream and it cannot be kept secret. It moves forward faster and more boldly. Therefore, in those years when we were the first to discover the principle of radiation emitted by an electronic siren, we religiously remained idle.

It is pointless to say that our science should be at the forefront if new efforts are helped in such a flabby way, as often happens here and has happened in the case of my own work on high-power electronics.

However, I am not afraid to recognise that, in all fundamentally new undertakings where boldness on a grand scale is required, we are so far lagging far behind the USA and several other countries. Because of secrecy, our scientists do not even have the satisfaction of establishing the intellectual priority in finding new ways in science.

In spite of the absence of help in the development of my work, in the last year I managed to do the following:

(1) develop a theory of a method for a cumulative accelerator (mentioned above);

(2) develop successfully and improve an instrument I proposed and called the 'planotron' (its working prototype is nearly finished);

(3) found a way of investigating the density of the electron gas and thus checked the laws of motion of such a gas which I established earlier; the results of this work will be included in my book *Elektronika Bol'shikh Mo shchnostei* (High-Power Electronics);†

(4) on the basis of the results of these theoretical investigations, I began to develop experimentally a high-power ionisation vacuum pump;

(5) in cooperation with Academician VAFock, I developed a new mathematical method for solving the problem of radiation emitted by a cylinder of finite length (a paper is being prepared for publication);‡

(6) I finished and prepared for publication a paper on the theory of lubrication for the rolling of a sphere and a cylinder;§

(7) put into practice a number of recommendations of the Presidium to develop and compile plans for dealing with particularly important problems, intended for the Council of Ministers; wrote a paper on Ernest Rutherford for *Bol'shaya Sovetskaya Entsiklopediya*.¶

Academician P L Kapitza

Year 1955†

20 January 1956

In 1955 I published two papers:

(a) "On the nature of ball lightning" Dokl. Akad. Nauk SSSR 101 (2);

(b) "Hydrodynamic theory of lubrication during rolling" (presented at a session of the Division of Physicomathematical Sciences on 31 January 1955) Zh. Tek h. Fiz. **25** (4).

On 24 December 1955 I presented a paper "On the nature of ball lightning" at a jubilee session of the Moscow Society of Natural Scientists.

On 28 January 1955 I was appointed Director of the Institute of Physical Problems and during the year I led the scientific activity of the Institute and its reorganisation needed because of new tasks that the Institute has to carry out.

As leader in the field of 'Low-temperature physics' I chaired a meeting on this subject, which took place in June 1955 in Kiev.

In summer of 1955 I was appointed Editor of Zhurnal Eksperimental'noi i Teoreticheskoi Fiziki and in the autumn I actually took over this job.

† Published by the Academy of Sciences of the USSR, Moscow, in 1962.
‡ "Static boundary-value problems for a hollow cylinder of finite length" *Zh. Tekh. Fiz.* 29 1177 (1959); "Symmetric vibrations of an ideal conducting hollow cylinder of finite length" *Zh. Tekh. Fiz.* 29 1188 (1959); both these papers were coauthored by V A Fock and L A Vainshtein.

§ "Hydrodynamic theory of lubrication in rolling" *Zh. Tekh. Fiz.* **25** 747 (1955). In *History of Tribology* (London: Longman, 1979). Dowson called this work classical.

¶ Bol'shaya Sovetskaya Entsiklopediya (Great Soviet Encyclopaedia) 2nd ed. **36** 266 – 267 (1955); author not given.

[†]The report for 1954 has not been found in P L Kapitza's archive or in the archives of the Russian Academy of Sciences. It had evidently not been presented.

As a member of the Bureau of the Division of Physicomathematical Sciences [of the USSR Academy of Sciences], I regularly attended meetings of this Bureau and participated in its work.

Academician P L Kapitza