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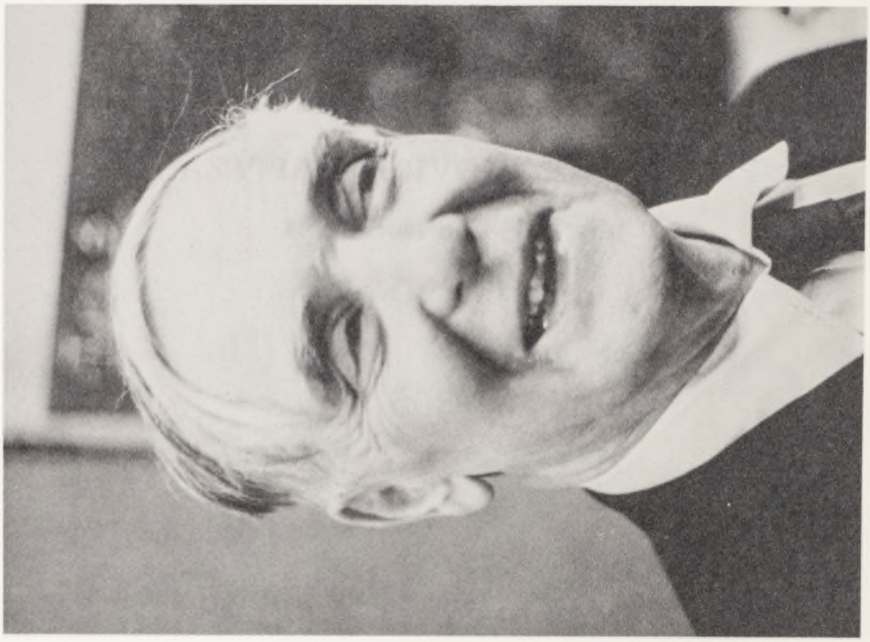

P. Kapitza

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Elected F.R.S. 1929

By D. Shoenberg, F.R.S.

## Russia, 1894–1921

Peter Kapitza* was a legendary figure both in Rutherford's Cambridge of the 1920s and 1930s and subsequently in Moscow to the end of his long life and the legends serve to illustrate his colourful personality. In his scientific work he showed great versatility and brought the skills of an engineer and mathematician to bear on important problems in physics and technology in an entirely original way. He also had broad cultural and social interests and his original ideas on scientific education and organization have had a profound influence, particularly on the development of Soviet physics.

He was born in Kronstadt, the island fortress on the Neva near St Petersburg,† into a family with strong intellectual traditions. His father, Colonel (later General) Leonid Petrovich Kapitza was a military engineer involved in modernizing the fortifications of Kronstadt. The Kapitzas had been landed gentry with Polish antecedents and the family was well represented in the professions. His mother, Olga Ieronimovna, to whom he was very close until her death in 1937, was a specialist in children's literature and folklore and an important figure in the literary world of St Petersburg. She organized a club for young writers, many of whom subsequently became famous. Her father, General Ieronim Ivanovich Stebnitski, also partly of Polish origin and a military engineer specializing in cartography, was a geographer of international repute, a corresponding member of the Imperial Academy of Sciences and an ardent traveller all over the world; it is perhaps from him that Peter Kapitza inherited his own love of travel. Much of Stebnitski's military service was in the Caucasus, where he personally mapped out the important mountain

* In England, the Russian Piotr was at first rendered as Pierre but he was usually known as Peter. The transliteration of his surname is the one he used himself, but the tz is often rendered as ts.

† After 1914 St Petersburg became Petrograd and after 1924 Leningrad.

peaks and made gravitational measurements; later he was head of the Military Topographical Department of the Imperial General Staff in St Petersburg. Unusually for his time, he arranged for his daughters to have a higher education, Olga in the humanities and Alexandra in mathematics and science.

Alexandra played an important part in Peter's upbringing for it was she who discovered, rather to the surprise of the family, that he had an unusually quick grasp of arithmetic. Though a lively boy, he had seemed a bit backward in some other respects. He never overcame a certain indistinctness and sloppiness in speech and never learned to spell properly in any language—a considerable handicap in his early career. He was admitted to the 'classical gymnasium', which specialized in the humanities, but had to leave this school in 1906 at the end of his first year because of inadequate progress and transferred to the more scientifically oriented 'realschule'. He enjoyed a kind of revenge 70 years later when, to mark his second award of the title Hero of Socialist Labour, his bust was erected in a public square of Kronstadt opposite the gymnasium from which he had been excluded! Although this exclusion evidently rankled, the 'realschule' was much more appropriate for developing his talents, and in 1912 he graduated with honours and entered the electrotechnical faculty of the St Petersburg Polytechnical Institute—for lack of Latin and Greek he was not eligible to enter the St Petersburg University, then regarded as the more prestigious institution.

His studies were interrupted by the 1914 war and for two years Kapitza was an ambulance driver at the Polish front. After demobilization he returned to the Polytechnical Institute where he graduated in 1918 and was appointed to a teaching post in the Institute, which he held until he moved to Cambridge in 1921. During these years the moving spirit in the physics world of Petrograd, and indeed of Russia, was A. F. Joffé, who was actively developing a physics school with an outlook more modern than had been traditional in Russia up to then, and with an emphasis on research and seminars to keep abreast of developments in the West. After 1918 the research effort was concentrated in the newly established Petrograd Physico-technical Institute, where Joffé succeeded in collecting a group of young and enthusiastic scientists around him who formed the nucleus of the enormous growth of Soviet physics in later years. The creation of this new institute for physics research must have seemed wildly visionary at that time since, after the chaos of the war and the upheaval of the Revolution, the economy of the country was in a disastrous state. There were great shortages of food and fuel and practically no scientific equipment, so that research could be done only on a 'do it yourself' basis. In spite of these difficulties, however, quite a lot got started.

Kapitza took up several projects of topical interest. One was the measurement of the angular momentum associated with magnetization of

matter, and his first scientific paper* was a critical review (1)† of the experiments by Einstein and de Haas, who had demonstrated the effect in 1915, and by Barnett who almost simultaneously demonstrated the inverse effect. At that time there appeared to be a contradiction between the two sets of experiments and Kapitza concluded that there must be some error in Barnett's experiment because it gave only half the classically expected ratio of angular momentum to magnetization. However, soon afterwards, Einstein and de Haas discovered an error in their experiment and confirmed Barnett's 'anomalous' result which was later explained in terms of electron spin. A second paper (2) describes an ingenious improvement in the technique of preparing Wollaston fibres; Kapitza also became adept at using Boys's bow and arrow method of producing very fine quartz fibres.

Another line of work was the study of atomic and molecular beams and together with his friend Semenov, Kapitza proposed a technique for measuring atomic magnetic moments by observing the spread of an atomic beam after passing through a strongly inhomogeneous magnetic field (6). Although their paper mentions that 'experiments in this direction have already begun' nothing further was published. In fact between the time of submission of the paper in December 1920 and its publication in 1922, Stern and Gerlach had not only proposed essentially the same idea, but carried it out in practice (Stern 1921, Gerlach & Stern 1921, 1922), and so provided the demonstration of spatial quantization for which many years later Stern was awarded a Nobel prize. A third field to which Kapitza contributed was that of X-rays and his proposal (3) to focus a broad beam of X-rays by a bent crystal seems to have anticipated what much later became an important practical technique. Johansson (1933) described the first practical realization of this idea, but does not appear to have been aware of Kapitza's paper.

Soon after Kapitza was demobilized, and following a romantic trip to Harbin in China, he married Nadezhda Kyrillovna, daughter of General Chernosvitov, and a son, Ieronim, was born in 1917. A second child was expected towards the end of 1919 but disaster struck the family. In the terrible conditions following the Revolution and the Civil War, epidemics were rife and Ieronim caught scarlet fever and died. Nadezhda was quite struck down by this blow and soon after she had given birth to a daughter (also called Nadezhda), both succumbed to the Spanish influenza that was then raging in the city. On top of this, Kapitza's father also died of 'flu at about the same time. Kapitza, who had himself had a bad bout of 'flu, was overwhelmed by these tragic losses and for a time was unable to work. His friends wondered what could be done to help

* This was in fact not his first publication. He had already published a popular article (G1) about the production of cod liver oil, based on his travels in the far North of Russia, and illustrated by his own photographs.

† Numbers given in this form refer to entries in the bibliography at the end of the text.

him forget his unhappy situation and something turned up that not only distracted Kapitza from his grief, but, as it turned out, completely changed the course of his life.

This was the setting up, at Joffé's prompting, of a 'Commission of the Russian Academy of Sciences for renewing scientific relations with other countries', which was liberally provided with foreign currency to buy equipment abroad (see Sominskii 1965, Krylov 1984). Besides Joffé, an important member of the commission was Admiral A. N. Krylov, the well known naval engineer and applied mathematician, who was later to become Kapitza's second father-in-law. Both had a high opinion of Kapitza's scientific gifts and wanted to help in his difficult personal situation; appointing him to join the commission seemed an ideal solution. Travel abroad at that time was complicated because most of the outside world had no diplomatic relations with Soviet Russia. Joffé set out in February 1921, and as soon as he got to Berlin began trying to get a visa for Kapitza, while he and other members of the commission were occupied with buying equipment. Germany, France and Holland did not want to risk admitting someone who might prove to be a young communist agitator, but England was more accommodating and eventually in May, Joffé succeeded in getting Kapitza an English visa.

At last, late in May, Kapitza arrived in England by ship and early in June he and Joffé started a round of scientific visits, which culminated in a visit to Rutherford in Cambridge on 12 July. They were received very cordially by Rutherford but when Kapitza asked if he might work in the Cavendish for a few months Rutherford was rather discouraging, saying the Laboratory was already crowded and it would be difficult to accommodate one more. Rutherford was rather taken aback when Kapitza replied by asking what accuracy he aimed at in his experiments. The answer to this seemingly irrelevant question was 2 or 3% and Kapitza then pointed out that as there were about 30 researchers in the Cavendish, one more would hardly be noticed because he would come within the experimental error! This ingenious approach persuaded Rutherford to admit Kapitza after all. As a postscript it may be mentioned that a year later Kapitza asked Rutherford why he had agreed to take him on and Rutherford laughed and said 'I can't think why, but I'm very glad that I did'.

## Cambridge, 1921–34

Kapitza joined the Cavendish in July 1921 and although the original plan was for him to stay only over the winter he remained for 13 years. His introduction to Cambridge life and his impressions of Rutherford and the Cavendish are vividly described in the letters he wrote at frequent intervals to his mother in Petrograd (quoted in Danin 1966, Parry 1968

and Rubinin 1985).* The usual initiation of new research students was a month or two of practical work in the Cavendish attic on various relevant techniques under Chadwick's supervision and a few days after his arrival Kapitza wrote (24 July) 'For the time being . . . I am working only at my practical course; as to the future, I don't know. . . . Time will show. . . .' In fact his skill and assiduity were such that Chadwick was satisfied after only two weeks and by early August Kapitza had at Rutherford's suggestion chosen to study how the energy of an α-particle falls off towards the end of its range. Previously this had been measured by observing the deflection of the α-particle in a magnetic field, but the limitation of this method was that particles of low energy could not be detected. Kapitza's method was to measure the energy in a collimated beam of α-particles by the heating it produced in a plate attached to a Boys radiomicrometer. This project was brought to a successful conclusion with amazing rapidity. Within nine months of the conception of the idea he was already drafting a paper for publication and by mid-June the paper was sent off. On 19 June 1922 he wrote to his mother:

> 'Today the Crocodile summoned me twice about my manuscript. . . . It will be published in the Proceedings of the Royal Society, which is the greatest honour a piece of research can achieve here. . . . Only now have I really entered the Crocodile's school . . . which is certainly the most advanced school in the world and Rutherford is the greatest physicist and organiser. It is only now that I have felt my strength. Success gives me wings and I am carried away by my work.'

'Crocodile' was a nickname Kapitza invented for Rutherford early on and many years later he told Ritchie Calder (1951, p. 66):

> 'In Russia the crocodile is the symbol for the father of the family and is also regarded with awe and admiration because it has a stiff neck and cannot turn back. It just goes straight forward with gaping jaws—like science, like Rutherford.'

A more fanciful but probably entirely apocryphal version, popular in the Cavendish, was that it was after the crocodile in Peter Pan that swallowed an alarm clock to give advance warning of its somewhat frightening appearance. This version finds some slight support in an early letter (12 October 1921):

> 'Rutherford greets me increasingly pleasantly and asks how things are going when he sees me. But I am a little afraid of him. I work almost next door to his office. This is bad since I have to be very

* A more complete edition of these and other letters is being prepared by Mrs A. A. Kapitza. The quotations in this memoir are my own rather free translations from the Russian.

careful with my smoking: its a misfortune if he should catch you with a pipe in your mouth. But thank God he has a heavy tread and I can recognize his footsteps . . .'

The published paper (7) shows that Kapitza's enthusiasm about his experiment was indeed justified. Boys (1923) said of his radiomicrometer that 'its extreme delicacy of construction requires more than ordinary skill on the part of the user' and Kapitza not only demonstrated this skill and exploited his experience of fine quartz fibres but incorporated many ingenious features to avoid various stray effects that in his preliminary experiments completely swamped the genuine small heating effect of the α-particles. He wrote to his mother (4 December 1921) that '. . . [his device] is so sensitive that it can detect the flame of a candle 2 versts [roughly 2 km] away and respond to a temperature rise of a millionth of a degree! . . .'. His results convincingly showed that the energy fell steeply to a negligible value towards the end of the α-particle range.

This publication illustrates several characteristic features of much of Kapitza's later work: his ability to work out in practice rather complicated schemes designed to give extreme sensitivity, his thorough grasp of potential pitfalls, his speed of working and his thoroughness in interpretation. Another interesting feature is Kapitza's fondness for regarding each project as part of a larger group of investigations, but then getting excited by a new idea and forgetting about continuing the original plan. Thus the paper is described as 'Part I' and the abstract mentions that further work will be described in another paper. However no Part II ever appeared, probably because Kapitza had become completely engrossed by a new idea that came up at that time.

This idea was a possible new method of studying how the α-particle velocity varied along its track by measuring the track curvature in a magnetic field. Existing magnets were not capable of producing large enough steady fields to curve the tracks sufficiently, and Kapitza's novel idea was to use fields that were much larger but lasted for only a very short time. This marked a turning point in Kapitza's career. Once the large impulsive magnetic fields had been achieved he saw whole new vistas opening up for exploiting the technique, and this led him into pioneering work in solid state and low temperature physics. It was also the beginning of the transition of the Cavendish from the string and sealing wax tradition to the age of large machine physics.

One of the basic problems of producing an impulsive field is how to provide a large store of energy that can be converted into electrical power and rapidly discharged through a coil. An obvious candidate for such an energy store was an electrical condenser of large capacity charged to a high voltage, but such a condenser would have been expensive and Kapitza chose instead a chemical store, essentially a large accumulator of special design, which he could build himself.

He was encouraged by Rutherford's reaction to his idea; as he wrote to his mother (15 June 1922):

> 'The Crocodile is taken with my idea and thinks it will succeed. He has a devilish feeling for experiment and if he thinks that something will come out of it, that is a very good omen. His attitude towards me gets better and better . . .'

He also mentioned that a 'young physicist' (P. M. S. Blackett, 3 years his junior) was going to work with him. Blackett introduced him to the subtleties of the Wilson cloud chamber, which was used for revealing the α-tracks, but the collaboration did not continue beyond the initial stages. A few days later Kapitza wrote of another significant sign of Rutherford's rising esteem: 'The Crocodile's permission for Laurmann to come is the best demonstration of his kind attitude to me.' Emil Yanovich Laurmann (for some biographical details see Shoenberg 1954) was an Estonian who had collaborated with Kapitza in Petrograd and was remarkably skilled in all sorts of techniques such as electrical engineering, photography and, most importantly, the handling of delicate equipment. He was a little deaf and visitors to the laboratory were often startled by Kapitza's loud cries of 'Baron' when he needed Laurmann's assistance. Baron was a nickname based on a half joking tradition in Russia that the Baltic provinces were entirely populated by nobility.

Although the new project was technically much more complicated and on a larger scale than anything Kapitza had tackled before, Kapitza's drive, backed by Rutherford's support and Laurmann's assistance produced results very quickly. After some early failures he wrote on 17 August, only 2 months after starting,

> 'The preliminary experiments were completely successful. I am told that the Crocodile speaks of nothing else just now. I have been given a large room in addition to the one I already work in, and for the full scale experiment I have got permission to spend a fairly substantial sum of money.' [This was £150!]

and on 2 September:

> 'My experiments are assuming a very broad scope . . . I shall remember my last conversation with Rutherford as long as I live. After a whole lot of compliments he said 'I should be very happy if I could have the possibility of creating a special laboratory for you in which you could work with your own students.' . . . Am I really such an able person? I feel uneasy and alarmed. Will I be able to cope?'

And then at last, on 29 November, presumably after the construction of the Wilson chamber as well as the high field equipment:

> '. . . For me today is to some extent a historic day . . . In front of me is a photograph with nothing but three curved lines on it. But

these three lines show the tracks of α-particles in a magnetic field of terrific strength. These three lines have cost Professor Rutherford 150 pounds sterling and have cost me and Emil Yanovich $3\frac{1}{2}$ months of extremely hard work. But here they are and everyone in the University is talking about them. Strange: only three curved lines! The Crocodile is very pleased with these three curved lines. . . . Many people have come to look at these three curved lines and admired them. Now we must go further and there is much work ahead.'

By March 1923, again only nine months after starting, a paper had been submitted that was published soon after (10). This was followed a year later, when the research had been completed, by two further papers (11, 12), the first giving details of the impulsive field method and the second describing the special design of the Wilson chamber, the elaborate switching and timing devices involved and finally a detailed account of how the curvature of the α-particle tracks varied with position along the track. Effectively the curvature measured the ratio of velocity to charge and the variation of the average charge with position could be deduced from other experiments and some appeal to theory. Thus finally a graph was obtained of velocity against range, just as in the heating experiment described earlier, and much to Kapitza's satisfaction the two agreed quite reasonably.

While all this work was progressing, Kapitza's position in Cambridge was rapidly being consolidated. In January 1923 he was officially admitted as a research student for the Ph.D. degree, with backdating to October 1921, and he obtained a year's remission in view of his Russian work. This meant he could complete his Ph.D. in the summer of 1923. He was also admitted as a member of Trinity College and soon after his Ph.D. he was awarded a Clerk Maxwell Scholarship. He continued to write ecstatically to his mother about Rutherford:

18 March 1923—'. . . The only thing that eases my work is the Crocodile's care, which can only be compared with the care of one's own father.'

15 June 1923—'. . . Yesterday . . . I met the Crocodile just as I came back from being made a Doctor of Philosophy. I asked him: "Don't you think, Professor Rutherford, that I look wiser?" "Why should you look wiser?" he replied, intrigued by my somewhat unusual question. "I have just been made a doctor" I answered. He immediately congratulated me and said, "Yes, you do look appreciably wiser and moreover you've had your hair cut" and roared with laughter. Taking such liberties with the Crocodile is generally very risky, since most often he simply sends you to the devil and I seem to be the only one in the laboratory who dares to try. . . . Such behaviour towards him from a junior is very unusual. Something like six times I

have had compliments from him such as 'fool', 'ass' etc. Most of the laboratory is astonished how such jokes are possible. . . .'

Another example of the kind of joke Kapitza had in mind was recounted many years later (G55). Kapitza had been rather shocked when at the start of his Cambridge career, Rutherford warned him that he would not tolerate his making Communist propaganda in the laboratory. When a year later Kapitza's first major paper appeared he tried a mild leg-pull:

> 'I presented Rutherford with a reprint and I made an inscription on it that this work was proof that I had come to his laboratory to do scientific work and not Communist propaganda. He got extremely angry with this inscription, swore and gave me the reprint back.* I had foreseen this and I had another reprint in reserve with an extremely appropriate inscription with which I immediately presented him. . . . Rutherford had a characteristically hot temper but cooled down just as quickly'.

The paper on α-tracks in strong magnetic fields was Kapitza's last research in Rutherford's personal area of interest. From then on he applied the impulsive field technique to other problems and extended it to produce still higher fields. The first application was a study of the Zeeman effect in fields up to 130 kG† (13, 14) in collaboration with H. W. B. Skinner. The hope was that something new might turn up at such high fields—typically 3 or 4 times higher than had ever been used before. However, in spite of the ingenuity of the technical arrangements (see P1, P2, P3), particularly as regards producing an intense light source at exactly the right moment, nothing very new did turn up. It was, of course, a great feat to make everything work properly at the same time but the results at the high fields proved to be mainly no more than reasonable extrapolations of what had been observed before at lower fields.

At about this time Kapitza started an even more ambitious project for the production of strong impulsive fields. The accumulator method was in practice limited to fields of little more than 100 kG in a reasonable experimental volume and, moreover, the accumulator required difficult repairs after comparatively few discharges. The idea of a new method was conceived by Kapitza in 1924 in the course of discussions with a Russian electrical engineer, M. P. Kostenko, who calculated that it should be practicable to design a large dynamo that on short circuit through a suitable coil could generate very high power for a very short time and so produce a field of order $10^6$ G. The stored energy would be the kinetic energy of rotation of the dynamo rather than the chemical energy of the accumulator.

* The original inscribed reprint is in the Cockcroft archive at Churchill College, Cambridge.
† $1\,G = 10^{-4}\,T$.

Once again Kapitza was enthusiastically supported by Rutherford. But the expense involved was of a bigger order of magnitude than could be provided from Cavendish funds, and Rutherford had to turn to the Department of Scientific and Industrial Research (D.S.I.R.) to realize this first step into the 'machine age'. A grant of £8000 was made, spread over 4 years, and yet again in extremely short time, the centre piece of the scheme, the big dynamo, had been designed, built and tested at Metropolitan-Vickers in Manchester. On 7 July 1925 Kapitza was able to write to Kostenko:

> '. . . The machine has been quite a success. . . . It has been tested and is already installed in Cambridge. All this has taken a lot of work and energy, but as you see in the 1 year and 4 months since we first conceived the idea of this machine it has been built and installed. That's not bad . . .'.

The dynamo was patented (P4) in the names of Kapitza and Kostenko, but some difficult problems had to be tackled before it could be used to generate large fields. These took years to solve, and a detailed account of the whole scheme appeared in 1927 (16). The most challenging problems were the switch that had to close and open exactly synchronously with the dynamo cycle, during which it had to carry as much as 30 kA and the coil that had to be made strong enough to withstand the enormous magnetic stresses. The detailed design of the coil was worked out by Cockcroft (1928) and the problem is vividly illustrated in a letter Kapitza wrote to Rutherford on 12 December 1925 when Rutherford was on his way home from a trip to the Antipodes.

> 'I am writing to you in Cairo to tell you that we have already obtained fields of more than 270,000 G. . . . We couldn't go further because the coil burst with a deafening bang which I am sure would have given you much pleasure. The coil had not been externally reinforced and this we now intend to do. . . . I am very happy that on the whole all went well and now you can be assured that 98% of the money has not been wasted . . . . The accident was the most interesting part of the experiment and finally strengthens our belief in success, since we now know exactly what happens when the coil bursts. We also now know what an arc of 13,000 A looks like.'

Although the high field machine worked successfully and enabled Kapitza to open up several new and fruitful fields of research over the next few years, it is puzzling that he should have chosen the dynamo method rather than what with hindsight would seem to have been simpler and cheaper—namely storing the energy in a large capacitor. In his earlier paper on the accumulator method (11) he mentions this as a possibility but dismisses it without any very convincing argument. It is true that large capacitors at that time may not have been as reliable or as cheap as

they have become since, but a capacitor of order 10 mF that could be charged to 2.5 kV would probably have been cheaper and simpler than the dynamo. Discharge of such a capacitor through a coil comparable to that used by Kapitza could have produced fields comparable to those achieved with the dynamo. Indeed T. F. Wall (1924), an engineer at

Kapitza by B. Kustodiev (portrait owned by the Fitzwilliam Museum and at present on loan to Darwin College, Cambridge).

Sheffield University, did propose this method and in 1926 described experiments giving fields as high as 200 kG (but for rather shorter times than Kapitza's fields), using a capacitor of about 1.4 mF charged to 2 kV. However, Wall's coils burst in more extreme conditions and he does not appear to have overcome the difficulty nor continued the experiments.

At the Solvay Congress in 1930 (28) Kapitza referred to Wall's limited success as evidence for the unsuitability of the condenser method, but this is hardly convincing and Kapitza's real reason for not trying the

capacitor method may have been his dislike of following a trodden path. In fact, with the development of more reliable capacitors and electronic methods of switching and recording, the condenser method came into its own soon after the war (for a review see Cotti 1960). The dynamo system was reproduced and successfully tested in 1939 by Y. Tanabe at Tohoku University in Sendai, but it was destroyed during an air raid on 9 July 1945 (see Tanabe 1949).

Kapitza loved showing off the special features of his installation. Even though the machinery was elaborately mounted to avoid transmitting vibrations, the considerable mechanical shock associated with a loss of 20% of the kinetic energy of the dynamo might easily have upset the delicate recording instruments. To avoid any disturbance, the coil and the recording instruments were placed about 20 metres away from the machine so that the seismic wave through the ground arrived only after the experiment was over, and the necessary space was provided by an outbuilding of the Chemistry Department. Later, when the work was transferred to the Royal Society Mond Laboratory, the long 'magnet hall' became the central feature of the building, with research rooms on either side of it. This turned out to be a very successful design even after the high field machinery had gone, because the long hall provided convenient opportunities for researchers to meet and exchange ideas.

To bring home the novelty of an experiment lasting such a short time Kapitza was fond of telling two little jokes. One was that $\frac{1}{100}$ s was a very long time if you know what to do with it, and the other was that he must certainly be the highest paid scientist in the world since he received a full academic salary for work lasting in total only a few seconds in a year. On one occasion—a rare one—Kapitza was upstaged in showing off his high field equipment. The occasion was a visit from R. W. Wood, the American physicist, himself famous for his ingenuity and his scepticism of what others could show him. Kapitza prepared a striking demonstration of the great strength of his fields by shooting a glass rod immersed in liquid oxygen (strongly paramagnetic) up to the ceiling, where it shattered, when the impulsive field was applied. Wood was accompanied by Rutherford and other Cavendish figures and they looked expectantly to see what impression had been made on him. He, however, did not seem particularly impressed. Instead, he walked up to the coil, took out the small vessel of liquid oxygen and calmly drank Kapitza's health! The spectators, not familiar with this rather difficult trick, which depends on keeping the liquid in the spheroidal state, were rather relieved to see Wood spit out the liquid again after a few seconds.

Kapitza's first major research with the new equipment was an extensive study of how the electrical resistance of metals increases with magnetic field (17–21). This was chosen partly because it was the simplest kind of measurement to make in a pulsed field and partly because it had been relatively little explored before. A general feature of the results was that

the resistance, after starting off quadratically in field, eventually changed asymptotically to a linear variation, often known as Kapitza's law of magnetoresistance. He was able to fit his results to a theory based essentially on the *ad hoc* assumption that ideally the basic law was linear but that this ideal behaviour was disturbed by the presence of randomly directed internal magnetic fields superimposed on the applied field.

Although this bold attempt to give a phenomenological theory of magnetoresistance at a time when the electronic theory of metals was still in its infancy was useful in providing a guide to further experiments, it has not stood the test of time. Later experiments, particularly at lower temperatures and with single crystals, coupled with a better understanding of the theory, showed that Kapitza's linear law was not really basic but a consequence partly of the relatively high temperature of his measurements (only down to liquid nitrogen temperature) and partly of his use of polycrystalline samples in most of his experiments (for a recent discussion see Pippard 1979). Kapitza himself realized that experiments at lower temperatures would probably be essential for a better understanding of metals and this was one of the main motivations for his work on hydrogen and helium liquefaction soon after.

Two more researches at high fields, both again of an exploratory nature, were then carried out. These were studies of the magnetization of a variety of substances (30) and of magnetostriction, mainly of single crystal bismuth (33–35). At the heart of both studies was an ingenious instrument (29) that could be used either as a balance to measure the force on a sample in an inhomogeneous magnetic field, and hence the magnetization, or as an extensometer to measure the change of length of a rod fixed at its other end. The idea was to apply the force or the change of length to a diaphragm sealing the bottom of a small vessel containing oil and itself completely immersed in a bath of the same oil. Displacement of the diaphragm squirted oil through a small hole between the inner vessel and the outer bath and so tilted a light mirror mounted on a pivot near the hole. By suitable design the deflection of a light beam reflected by the mirror could be made proportional to the force or to the extension.

The magnetization measurements did not reveal anything very novel, for it turned out once again that the laws established at lower fields could safely be extrapolated into the new range of higher fields. The observation of magnetostriction in bismuth was, however, the first such observation on a diamagnetic and Kapitza also developed the theory for specifying how the anisotropy of the effect was determined by crystal symmetry. Kapitza's interest in the somewhat anomalous properties of bismuth started a tradition that continued long after he had left Cambridge (see Shoenberg 1978), and his emphasis on the importance of chemical purity and single crystals was also a valuable legacy to those who followed in his footsteps.

In the course of all this work, there were several important

developments in Kapitza's life and career. In January 1925 he was appointed to an official University position, that of Assistant Director of Magnetic Research. In October 1925 he was elected to a Research Fellowship at Trinity College and he became a popular member of the High Table, mixing easily with young and old of all specialties. He greatly valued his association with the College where he lived until he was married and where he continued to dine frequently during the rest of his time in Cambridge. He was particularly pleased when, many years later (in 1966), he was elected to an Honorary Fellowship.

In the spring of 1927 he went to Paris and on 27 April wrote to Rutherford (his spelling has not been corrected):

> 'I am going to be married. . . . What do you think about it??!! I feer you are rather angrey. This is why I propose to have no honeymoon and bring my wife in a few day's time after my wedding to Cambridge . . . I hope you understand that I am a victime of my own 300,000 gausse and I have to confece that the dose which I received is rather a strong one. . . . You see that even in more important questions I have a quick dessision and gread speed of action.'

The lady in question was Anna Alekseyevna, daughter of the Admiral Krylov mentioned earlier. Although Anna's father stayed in Russia after the Revolution her mother emigrated to Paris and it was there that Anna completed her education as an archaeologist; she was also an accomplished artist.

Rutherford's reply (29th April) to Kapitza's announcement also deserves quotation:

> 'I received your letter at breakfast this morning and I read it with much interest and amusement. . . . My wife and I unite in sending our warmest congratulations. . . . They say it is a bad wife that does not help a little, so I shall expect your work to make even faster progress. . . . I am not surprised at the news as I had heard rumours of your magnetic susceptibility under intense attractive fields.'

The marriage was a very happy one and Anna was not only a charming hostess to their many friends, but a great support to Peter in the difficult times he had to go through on several occasions later. Two sons were born in Cambridge: Sergei in 1928, a distinguished physicist and a very successful presenter of popular science on Soviet television, and Andrei in 1931, a well known Antarctic explorer and geographer.

In 1929 Kapitza was elected to Fellowship of the Royal Society at the first election after he had been proposed. This in itself was a rare distinction but even rarer was the election of a foreigner—for Kapitza had always retained his Soviet citizenship. In fact the Statutes at that time did not exclude the election of a foreigner provided that he was an 'inhabitant of His Majesty's dominions', but such elections were indeed rare, the

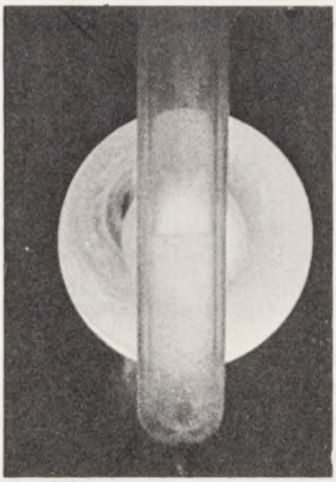
previous one having been in 1914! But to cap everything, his scientific distinction was almost simultaneously recognized in his own country by election to Corresponding Membership of the Soviet Academy of Sciences.

Early in 1930 Kapitza discussed with Rutherford the possibility of setting up a new laboratory that could house not only the high field equipment, but also provide cryogenic facilities to extend his researches to much lower temperatures. Rutherford backed this idea enthusiastically and persuaded the Royal Society to give £15 000 for building such a laboratory. The money was found from a bequest to the Royal Society by Ludwig Mond and the new laboratory was called the Royal Society Mond Laboratory. At the same time Kapitza was appointed to a Royal Society Messel Professorship. All the complicated negotiations between the Royal Society, the University and D.S.I.R. were completed remarkably quickly and smoothly—partly because of the dominant position of Rutherford in all three bodies and partly through the drive of Kapitza and Cockcroft—and by 1931 a handsome modern building designed by the architect H. C. Hughes was going up in the courtyard of the Cavendish.

The first step in the advance towards lower temperatures was a hydrogen liquefier that was completed by Kapitza in collaboration with Cockcroft shortly before the Mond Laboratory was ready (36). A difficulty in the liquefaction of hydrogen is that extremely pure hydrogen must be used to avoid condensed impurities blocking up the narrow-bore regenerator tubes. This difficulty was ingeniously circumvented by using only a small volume of extremely pure hydrogen in a closed circuit, which cooled ordinary commercial hydrogen and allowed the impurities to settle at the bottom of the heat exchanger without any blockage. A special feature of the liquefier room was a very light roof designed to blow off and relieve the pressure in the event of an explosion. Kapitza was fond of warning nervous visitors of this possibility, though in the event it never occurred.

In constructing a helium liquefier, Kapitza as usual chose a completely original method (39, 40, P10). Instead of relying on cooling with liquid hydrogen to get the helium below its Joule–Thomson inversion temperature, the main cooling was achieved by adiabatic expansion in a piston and cylinder engine. This of course was a well known principle but up to then the problem of how to lubricate the piston had never been solved. Kapitza achieved this by the ingenious expedient of using the helium gas itself as the lubricant. The machine was completed in 1934 and proved important not only in providing liquid helium for over 10 years of Cambridge research, but in supplying the basic idea for a factory built helium liquefier designed by S. C. Collins (1947) at the Massachusetts Institute of Technology (M.I.T.). The commercial availability of these Collins machines revolutionized low temperature physics by making

liquid helium easily accessible all over the world, rather than at only a few specialized centres. Kapitza had hoped that Cambridge would have the first liquid helium in England, but he was upstaged by F. A. Lindemann at Oxford, who brought Mendelssohn over from Germany with one of Simon's miniature liquefiers and triumphantly announced (Lindemann & Keeley 1933) the first liquid helium in England a year ahead of Kapitza. However, Kapitza could claim that his was the first liquid helium that could be looked at (the Oxford liquid was hidden in a metal vessel).



The first liquid helium in the Royal Society Mond Laboratory, 21 April 1934.

The opening of the Mond Laboratory by Stanley Baldwin, then Chancellor of the University, was a great occasion with a luncheon at Corpus Christi College hosted by the Vice-Chancellor, followed by speeches in the Arts School, and finally the opening itself with a gilded crocodile-shaped key and a visitation of the laboratory. Baldwin's speech accepting the Royal Society's gift to the University caused some amused puzzlement, for much of it was almost identical with what Rutherford had said earlier in the proceedings. It seems that Rutherford had forgotten he had already used his remarks in briefing Baldwin! During the laboratory visit Kapitza provided a nice example of his style of irreverent teasing. He was overheard replying to a query from Baldwin: 'Yes indeed. You can believe me, I'm not a politician'. In the course of the opening, a lifesize carving of a crocodile on the wall just outside the main entrance and a bas-relief of Rutherford himself, inside the foyer, both by Eric Gill, were revealed for the first time; the bas-relief gave rise to some controversy about the shape of Rutherford's nose (see Oliphant 1972).

Something should now be said about other aspects of his work and leisure in Cambridge. The Kapitza Club (see p. 346) represents his major contribution, albeit informal, to teaching activities. His other teaching

took the form of supervising research students and some lecturing. The latter consisted of eight weekly lectures on 'Recent researches in magnetism' and I have lively memories of attending this course in 1932. I found it fascinating not only for its content, but also for the intriguing presentation. His strong Russian accent and rather high-pitched voice, his peculiar English constructions and his habit of writing on the blackboard something quite different from what he had said, were entertaining, but sometimes made it difficult to follow. Once I came up after the lecture and asked him to clarify a contradiction in my notes. His reply was 'If I make everything so clear that there are no contradictions, there is nothing left for you to think about.' He also gave many semi-popular lectures and once described his technique: 'I try to pitch it so that 95% understand 5% and that 5% fully understand 95%. I always tell a joke in the first 5 minutes and if they laugh I know they are understanding my English well enough.'

Kapitza supervised only a few research students. The earliest were J. D. Cockcroft, subsequently famous for his nuclear physics work, and W. L. Webster, who did some pioneer work on ferromagnetism. The only others before the move to the Mond were D. S. Kothari, who later made distinguished contributions to astrophysics in India, and A. G. Hill who joined Kothari briefly. At that time the usual practice in the Cavendish was for the Cavendish Professor to be the official supervisor of all the research students, though the detailed supervision was often done by Rutherford's lieutenants. However, after the move to the Mond, Kapitza was named as the official supervisor of two more research students: myself, in 1932, and C. J. Milner, later Professor of Applied Physics at the University of New South Wales, in 1933.

Both Milner and I recall Kapitza's stimulating supervision and his original way of helping to overcome our experimental difficulties. I was put on to the measurement of the transverse magnetostriction of bismuth crystals in the field of an ordinary electromagnet. The expected change of length was very small, only of order $10^{-7}$ cm, and when I was feeling a bit desperate because the apparatus was still 100 times too insensitive, Kapitza would point out half a dozen simple improvements, each of which should give a factor 2 improvement and then throw in perhaps two rather unorthodox suggestions, each of which should give a factor 3. So the problem seemed to be solved, with even a bit in hand, since $2^6 \times 3^2$ is nearly 600. But when he had gone and I started trying his ideas, the factors of 2 proved to be only perhaps 1.2 and the factors of 3 perhaps 1.5 so that the gain was only perhaps 6 rather than 600. However, I was that much nearer my goal and ready for the next batch of suggestions. Milner was also impressed by the fertility of Kapitza's inventiveness, though he gradually learnt that many of Kapitza's ingenious suggestions carried some fatal flaw. But even if only perhaps one in five proved sound, this one might well hit the jackpot and overcome the difficulty.

Kapitza had many interests and skills outside his scientific work. One of his early enthusiasms was the internal combustion engine. Soon after his arrival in Cambridge he bought a motor-cycle and on 16 August 1921 he wrote to his mother:

> 'Everything went very well although we weren't going slowly, but then we had a spill. There are six bruises and scratches on my body but the worst is my face. If you could only see it! . . . When I came into the lab. I created quite a sensation. Now I shall be much more careful.'

Before long he graduated to a car (first a Lagonda, then a Triumph and later a Vauxhall, chosen because full working drawings were available) and acquired a reputation for rather reckless driving. When he was going particularly fast he would reassure a nervous passenger by saying that his high speedometer reading was in k.p.h. rather than m.p.h. On one occasion he solved the problem of overtaking a slow car observing the speed limit in Richmond Park by going off the road on to the grass and passing rapidly on the wrong side. Perhaps the best story is of his Trinity friend, the Reverend F. A. Simpson, sitting behind him on a country drive. As they approached a dangerous corner Kapitza pressed the accelerator and turned round to say 'Pray God Simpson, pray God'. It seems the prayers were effective. He had, indeed, rather a weakness for teasing clergymen; another example was when a clergyman, a guest dining in Hall, asked Kapitza who Eddington was and was told 'He knows far more about the Heavens than you do'.

As already mentioned, he was skilled at constructing and handling delicate equipment and he loved any activity involving fine mechanisms. One of his hobbies, dating from his schooldays, was repairing watches and clocks and making the replacement parts himself. Another hobby involving manual skill was conjuring. Sergei Kapitza recalls how fascinated he was as a small boy seeing his father appear to swallow table knives and bite bits off china plates. E. T. S. Walton remembers a train journey when Kapitza showed off the three-card trick ('find the lady') as convincingly as any professional trickster. In his memoirs (1968) the famous, but rather staid, applied mathematician Timoshenko recalls Kapitza's glee in making various eminent scientists look a little silly by involving them as stooges in his demonstrations. A rather different type of skill at which Kapitza excelled was chess and Smyslov, the Russian one-time world champion, said that he could not take a game with Kapitza lightly.

Finally, some comments on Kapitza's personality and style as remembered by those who knew him in his Cambridge days. At times he could be almost a text-book example of the absent-minded professor. If a question was addressed to him at such a time his typical reaction would be 'What you say?' and only after several repetitions of the question and

his 'What you say?' would he deal with the question—though sometimes he might simply walk off leaving the question unanswered. Occasionally this may have been a ploy to give himself time to think, but usually it was genuine absent-mindedness, or rather an indication that his mind was fully occupied by something else. Indeed when he was trying out a new idea in the laboratory he would become so completely absorbed as to lose all sense of time and sometimes Anna would have to come in to remind him that they were expecting guests to dinner. He paid little attention to his personal appearance and a Cambridge legend is that he was once refused admission to a formal gathering at the Senate House because he was wearing a blazer and carpet slippers rather than the obligatory dark suit and black shoes. But more often he was cheerful and outgoing, a great charmer and very good company. He loved an argument, he found something interesting to say on almost any subject and he was an excellent raconteur with an enormous repertoire of anecdotes, often of the shaggy dog variety, but usually with a Russian twist. Occasionally the point of the story would be obscure to someone not familiar with Russian traditions or because of Kapitza's peculiar English, but his laughter over his own joke was so infectious that those around him found themselves joining in, even if they hadn't altogether understood the joke.

It was perhaps the combination of his outgoing personality, his genuine interest in people and his curiosity about what made them tick that enabled him to overcome shyness and reserve and make friends of people like the rather reserved Chadwick, the taciturn Cockcroft, the usually silent Dirac and even the austere A. E. Housman of Trinity. These qualities also enabled him to gain the friendship and support of Rutherford, though here, as discussed by Wilson (1983), other factors were also relevant. Rutherford seems to have been captivated by Kapitza's boldness, both his boldness of scientific vision and his boldness in treating Rutherford with much less reverence and respect than the great man was accustomed to from his juniors, whilst at the same time openly showing his admiration of Rutherford's genius. A subject of joking in the Cavendish was Rutherford's fondness for using blunt stub ends of pencil, and on Rutherford's 60th birthday Kapitza presented him with a silver propelling pencil accompanied by a facetious congratulatory letter. Rutherford seems to have enjoyed this kind of lighthearted teasing, though Kapitza's gift did not wean him from his stub ends. When Kapitza had settled in Moscow some years later, Rutherford wrote how much he missed their evening walks after Sunday dinner in Trinity.

Two other aspects of Kapitza's personality will be evident from much of what has been said: his fondness of boasting and exaggeration and his enormous self-confidence. However, usually these were displayed in a somewhat tongue-in-cheek manner which endeared rather than repelled. For instance N. Kurti, recalls that when Kapitza visited Berlin in 1930 and F. E. Simon (who liked precision) asked for his exact English

address, he said with a chuckle 'Kapitza, England' will do. He loved above all to achieve something that was unique or at least exceptional. Thus he was proud of his unique distinction of being both an F.R.S. and a Soviet Academician and of his extremely rare status of remaining a Soviet citizen yet holding an important post in England. His self-confidence was based on realistic assessment of the problem in hand, and usually this self-confidence proved justified by the successful outcome of whatever he was trying to achieve. Rutherford was certainly impressed to see that Kapitza's confidence in his ingenious approach to his $\alpha$-ray problems was justified by the successful results he obtained so quickly. This made Rutherford all the more ready to accept Kapitza's persuasive arguments about the potential importance of high magnetic fields and low temperatures and to give his full backing in developing a new branch of physics outside his own field.

## The Kapitza Club, 1922–66

When Kapitza came to Cambridge in 1921 he missed the lively discussions of the Joffé seminars in Petrograd and he soon started an informal discussion group of his own, providing an injection of Russian temperament into his phlegmatic English colleagues. The first meeting was on 17 October 1922 with Kapitza giving a talk on magnetism. Although the emphasis was always on informality, Kapitza did insist on certain rules. No one could remain a member after missing more than a small number of consecutive meetings, and members were regarded as permanent only after they had themselves given a talk.

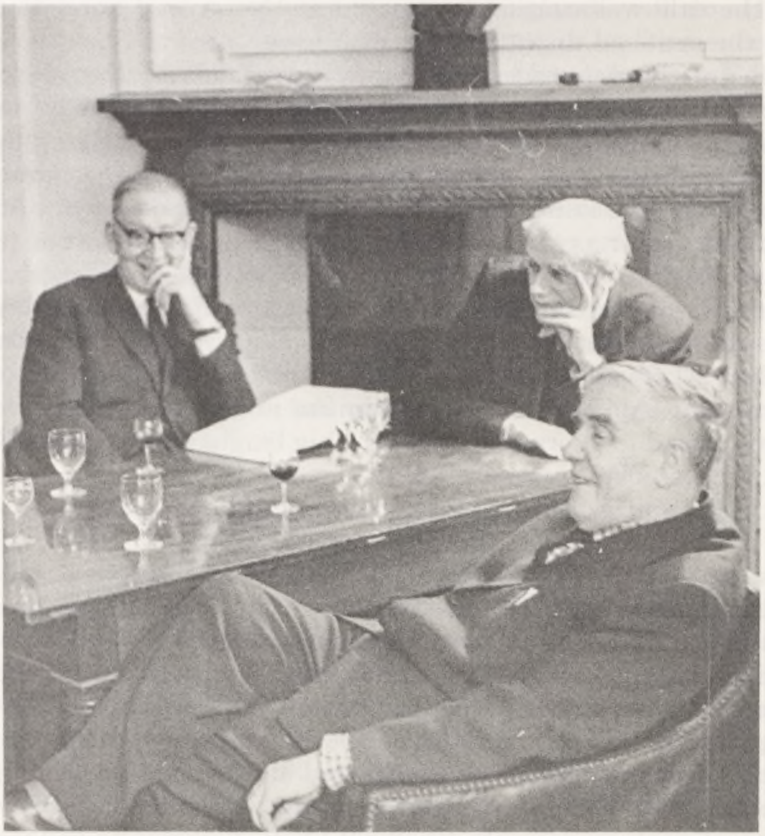
This weekly seminar soon came to be called the 'Kapitza Club', but Kapitza himself used to refer to it modestly as 'the club', though he was well aware of the name others used, and of the significance of the club in stimulating Cambridge physics. In a letter to Anna from Moscow in 1935 he wrote: '. . . Even in Cambridge I left a mark. Take the club which it is the custom to connect with my name, of which like old Pickwick, I was a permanent president. I think it will stay a long time . . .' Soon after its inception, a minute book was started and the tradition was established that the speaker should add his signature to the title of his talk. The original minute book is in Moscow, but a photocopy together with the second (final) volume, is in the Cockcroft archive at Churchill College. The record provided by these minutes is impressive. Among the speakers were not only the members, but also many leading physicists outside Cambridge: Bohr, Ehrenfest, Franck, Heisenberg and Langevin, to mention only a few. Most of the key advances of the time were eagerly discussed at the club and sometimes ahead of publication. Thus Landau spoke of the magnetism of metallic electrons in 1930, and in 1932 Chadwick, Lea and Feather reported the discovery of the neutron

(though the title was cautiously entered as 'Neutrons?') and Cockcroft reported the artificial disintegration of lithium.

Kapitza greatly encouraged interruptions and to break the ice he would himself often ask seemingly naïve questions. Sometimes such questions would provide a useful hint to the speaker that he was talking over the heads of his audience and nearly always they would start an argument and so clarify an obscure issue. R. W. Ditchburn recalls how on one occasion an American visitor presented a theory based on the theorem that the currents in a network adjust themselves to make the energy production a maximum. Kapitza interrupted to ask 'Don't you mean a minimum?' and others supported him. To decide the issue Kapitza took down from his bookshelves Maxwell's famous treatise, which he regarded almost as a Bible, and said 'Ah yes it says a minimum in Book II, Chapter 7' and someone chipped in 'Verse 12' causing a big laugh, (but for once Kapitza couldn't see the joke).

Some of the spirit of the discussion and argument comes over in brief remarks in the minute book. For instance in 1923 G. N. Lewis talked on 'What is the origin of radioactive substances?' and Kapitza recorded a characteristic remark: 'It is not advisable to look on the phenomena on the stars if you do not understand the phenomena on the earth'. In the same year Skinner spoke about the newly discovered Compton effect and the comment 'Compton is wrong' appears over the initials of Kapitza, Skinner, Hartree and Lennard-Jones. But a few months later 'Compton right we hope' is initialled by Stoner and Blackett, while Kapitza and others still backed the wrong horse and wrote 'We hope wrong'. Not long after a sensational report in the Sheffield Daily Telegraph (26 May 1924) of a death-ray machine invented by T. F. Wall (perhaps an offshoot from his condenser discharge experiments?), the minute book records 'Will Dr. Wall disintigrate the Univerce? P.K.'

The 377th meeting of the club on 21 August 1934 was the last with Kapitza in charge; when he was unable to return from the Soviet Union, Cockcroft took over and after the war I kept the club going for another 12 years. However as physics became more compartmentalized it was only rarely that physicists from one speciality could be persuaded to come and learn what was going on in a different field and eventually I felt that the club had had its day. The last regular meeting, the 675th, was on 4 March 1958 with a paper by Sir Ronald Fisher on 'Probability and scientific inference'. This, however, was not quite the last meeting. In 1966 Kapitza was at last able to visit Cambridge again and a special meeting was arranged on 10 May with quite a few present who had been members when Kapitza was in charge more than 30 years earlier. The scientific highlight of the meeting was a review by Kapitza and Dirac of a project (38) they had mooted in 1933 of scattering electrons by standing light waves. In 1933 light sources were too weak and electronic detectors too insensitive to produce any detectable Bragg reflection, but with the



Cockcroft, Dirac and Kapitza at the final meeting in 1966 of the Kapitza Club in Gonville and Caius College.

technical advances since then the project no longer seemed as 'way out' as it had seemed then.

It is convenient to conclude this section with a brief account of the seminars that Kapitza organized in his Moscow institute after 1936. These, like his Cambridge club, acquired a considerable reputation. As in Cambridge, they were at first intimate affairs with a dozen or so regulars from the Institute itself and occasionally a few guests. The audience sat in easy chairs in Kapitza's huge office and instead of the Cambridge coffee and biscuits, Russian tea and caviare sandwiches were provided. Kapitza presided in his idiosyncratic way and there was the same tradition of irreverent interruptions from him and the audience.* After the war and after the eight year interval of Kapitza's demotion, the seminars resumed, but gradually became much larger, with audiences of a hundred or so in a large lecture room, rather than the small group in Kapitza's office. Although little of the informality and intimacy survived the greatly increased numbers, the seminars still served a useful purpose in bringing

* For an amusing skit based on the early days of the seminar see Shalnikov (1975).

together physicists from all over Moscow, and it was regarded as a great privilege to be invited to give a seminar. Eventually, although regular dates were set aside, only a few seminars were actually given and during the last ten years or so they practically ceased.

## Return to the Soviet Union, 1934

As already mentioned, Kapitza was rather proud of his unusual status of directing a prestigious laboratory in Cambridge while remaining a Soviet citizen and being able to go in and out of the Soviet Union at will. After 1926 he visited the Soviet Union nearly every summer, and always had his permission to return underwritten by high-up figures in the Soviet political and scientific establishment. During these trips he visited his mother, gave lectures, consulted and usually managed to have a good holiday in the Caucasus or Crimea. As early as 1929 he had been invited by L. B. Kamenev (then an important political figure with special responsibility for science) to head the big Ukrainian Physico-Technical Institute being set up in Kharkov. Kapitza was able to convince Kamenev that he could more usefully develop his work in Cambridge for the time being, while acting as a consultant to the new Institute during short visits in the summer. Although his friends wondered whether his exclusive status could continue indefinitely, Kapitza laughingly shrugged off any warnings and in August 1934 made his usual summer trip, accompanied by his wife and travelling in their Vauxhall across Scandinavia. After attending a congress in Leningrad and lecturing in Kharkov, he and Anna were preparing to return to England from Leningrad early in October when the blow fell. Kapitza was told that his permission to return was no longer valid and that he would have to stay in the Soviet Union. After a few days Anna was allowed to return to Cambridge to look after the children and on 10 October she was able to tell Rutherford what had happened.

The story of the subsequent developments is one of almost Byzantine complexity and will only be outlined here; for a more detailed account see Wilson (1983, chapter 16). The reason for Kapitza's retention has been the subject of much speculation, but the basic reason was probably that suggested by Rutherford in a letter to Sir Frank Smith (then Secretary of the Department of Scientific and Industrial Research):

> 'I think I told you that Kapitza in one of his expansive moods in Russia told the Soviet engineers that he himself would be able to alter the whole face of electrical engineering in his lifetime. . . . This seems to me a very probable explanation of their action and is due to our friend's love of the limelight.'

Two other circumstances were also probably relevant. One was that George Gamow, a top young Soviet theoretician, had recently failed to

return from a visit to the West and Kapitza's retention may have been to some extent a tit-for-tat. The other was the great expansion of the Soviet economy in the early 5-year plans, of which a concomitant was the need for a rapid growth of science. In particular, Krzhizhanovski, an electrical engineer who held an influential position in the political establishment, wanted Kapitza to build an Institute in Moscow on the lines of the Mond, which would, as Krzhizhanovski thought, revolutionize the production of electricity.

Kapitza was utterly devastated by his inability to return to Cambridge and tried to disabuse the authorities of the exaggerated ideas they had formed about the immediate technical relevance of his work. For a time he sulked and thought seriously of taking up biophysics with Pavlov in Leningrad if he could not continue his work at Cambridge. But it was dangerous to sulk too long in those days and gradually Kapitza began to go along with the idea of working in Moscow, although he still hoped that a return to England might be possible.

Indeed, considerable efforts were being made by Rutherford on his behalf. He appealed to I. M. Maiski, the Soviet Ambassador, in very diplomatic terms, he got eminent scientists abroad, such as Langevin and Bohr, to make discreet representations to Soviet establishment figures, and representations were made to Stanley Baldwin, then Prime Minister. Nothing came of all these attempts, even though the whole affair had been kept private, so that no loss of face would have been involved if the Soviet authorities had made a concession. Eventually, however, and inevitably, the affair leaked into the Press with a big splash in the *News Chronicle* on 24 April 1935. On 1 May *The Times* published letters from both Rutherford and Gowland Hopkins (then President of the Royal Society), setting out the facts in a restrained fashion and appealing for Kapitza to be allowed to return to Cambridge to complete the work he had started. The effect of these letters was somewhat undermined by a jingoistic letter to *The Times* on 7 May from the chemist H. E. Armstrong (then the senior Fellow of the Royal Society), who claimed that England had no need of foreign scientists in general and of Kapitza in particular: 'Instead of leading a lotus life at Cambridge, he, too, may well be doing national work (in Russia) of a far higher importance than even that involved in magnetizing atoms to destruction'. Finally Maiski publicly defended the Soviet action in retaining Kapitza and concluded: 'Cambridge would no doubt like to have all the world's greatest scientists in its laboratories, in much the same way as the Soviet Union would like to have Lord Rutherford and others of your great physicists in her laboratories'.

By the end of 1934, though still unhappy with his situation and still often thinking of giving up physics, Kapitza began to cooperate in planning the new Institute of which, as he learnt from the newspapers, he had been appointed Director (the Institute was established on 23 December). Together with A. I. Shalnikov, a clever young experimenter

from Leningrad, he toured Moscow looking for suitable sites. They settled on an attractive place on the Lenin Hills and the new Institute for Physical Problems (I.F.P.) began to go up in May 1935. According to Khrushchev's memoirs (1974, p. 63)—not the most reliable of sources—this choice site had originally been reserved for the American Embassy, but Stalin had taken against Bullitt, the then Ambassador, and decreed that Kapitza should have it.

Kapitza continued to feel very frustrated and miserable all through the early months of 1935. He greatly missed his family and his work and he was treated with some reserve by scientific colleagues who were either too afraid or else too important to see him, so that he felt particularly lonely living alone in the Metropole Hotel. In his negotations with V. I. Mezhlauk (then an important political figure and Chairman of the State Planning Commission) he had great difficulty in getting across the idea that his work lay primarily in pure rather than applied physics and that he couldn't do anything useful unless he had facilities similar to those in Cambridge. At times he had fits of depression and he wrote to Anna in April: '. . . Here I sit all alone. What for? I do not understand. I want to scream and break furniture. I sometimes think I begin to go mad.'

However, gradually Kapitza's views began to prevail and even before the newspaper publicity, the Soviet authorities had started negotiations with Rutherford for transferring Kapitza's special equipment from the Mond to the new Institute. The newspaper publicity seems to have acted as a spur to settle things more quickly, though Mezhlauk was unpleasant to Kapitza about it. Kapitza wrote to Anna on 5 May:

> '. . . I said [to Mezhlauk] I obey in all I was ordered. But some of the orders sound as if Beethoven was told to write the 4th Symphony to order; of course Beethoven could conduct an orchestra to order, but would scarcely be willing to write a symphony to order, in any case a good one . . .'

Things began to get easier in other respects. The 'guardian angels went back to heaven' (he was no longer under surveillance), he was allowed to travel freely within the Soviet Union, he was assigned a good flat, frequent theatre tickets and a good car. The car evidently meant a lot to him:

> 'Yesterday the Institute received a very good car, big and comfortable and which I can use while I am Director. It is rather fun to have such a car. When I came to the door of the Metropole the porter helped me out . . .'

The negotiations went on with many ups and downs all through the summer of 1935; several times Kapitza threatened to resign and take up biophysics. At one stage there was an intervention by Molotov, who got Kapitza to write to Rutherford saying that he was a loyal Soviet citizen and that he was prepared to work in the laboratory being built for him in

Moscow. This letter finishes rather pathetically:

> 'I miss you, my laboratory and specially my work and it is not to be expected that I soon will be able to resume it, and all this makes me very unhappy. The stupidity of the created position is that it is based on complete misunderstanding as everyone concerned really acts with the best intentions.'

The most decisive stage of the negotiations came in August with visits to Moscow by Adrian and Dirac, when both had the opportunity of frank discussions with Kapitza that they were able to report to Rutherford. Eventually, by November a definite agreement had been reached between Cambridge University and the Soviet authorities, giving Kapitza most of what he wanted and on 5 November he formally resigned his Directorship of the Mond Laboratory. The agreement provided that in return for payment of £30 000 the University would transfer to Moscow the high field equipment and duplicates of such items (e.g. the liquefiers) as were needed in Cambridge to continue the Mond's activities. In addition the University was to give a year's leave of absence to Kapitza's key assistants, Laurmann and Pearson, so that they could help Kapitza set up all the equipment in the new Institute as quickly as possible.

In the mean time Anna had had the difficult task of winding up everything in Cambridge and by the end of 1935 she and the children had rejoined Kapitza in Moscow, where before long they moved into a comfortable and central apartment. Even though things now began to hum, with Cockcroft in Cambridge devotedly organizing the immense task of sending the equipment to Moscow—the big dynamo had already left by the end of December 1935—and Kapitza struggling to get everything ready to install the equipment as soon as it arrived, he still felt frustrated that he could not return to scientific work immediately.

His mood may be judged from one of his letters to Rutherford (March 1936) in the course of which he says:

> 'I feel myself very miserable here, better than last year, but not so happy as I was in Cambridge. Anna's return brought me much comfort and happiness and this is very important as I was here very lonely, quite alone. . . . Your letter reminded me of my happy years in Cambridge and then I felt you as you are, rough in words and manners and good in your heart such as I like you, and this makes me feel rather happier. The lost Paradise!'

This letter was in reply to one in which Rutherford, while sympathizing with Kapitza's difficulties, had expressed himself rather bluntly about a previous letter from Kapitza in which he had impatiently complained about various delays in the transfer arrangements and had made further requests that Rutherford thought excessive.

By the summer of 1936 the building of the Institute was far advanced,

most of the vital items of equipment had arrived and so had Laurmann and Pearson. They immediately got busy installing the machinery and familiarizing the Russian technicians with its operation and by the end of 1936 the I.F.P. was a going concern. Kapitza carried on his scientific work there to the end of his life apart from two interruptions: from 1941 to 1943, when the Institute was evacuated to Kazan during the war, and from 1946 to 1954 when he was temporarily dismissed from his Directorship.

## Moscow and Kazan, 1934–46

The Institute for Physical Problems (see references G7 and G28) was in part almost a Chinese copy of the Mond with a long magnet hall and adjacent research rooms, a liquefier room with a light roof and of course much of the original Mond equipment or duplicates, such as the distinctive-looking central switchboard and the distribution boards in each room. However, in other respects the accommodation was on a rather grander scale than the Mond. The Director's office was immense, there was a large meeting hall and considerable office space. Though the administration was inevitably larger than in the Mond, Kapitza succeeded in keeping it to a minimum by eliminating much of the elaborate paper work involved in planning and finance that was usual in most Soviet institutes. He was fond of saying that over-elaborate planning in research was rather like a doctor having to prescribe medicine for an illness his patient would have in a year's time. In a report to the Praesidium of the Academy of Sciences (G28) he said:

> 'The Finance people wanted to introduce the so-called thematic accounting whereby expenditure was reported in detail for each line of research. In my correspondence with the Commissariat I asked: "When looking at a Rembrandt picture are you really interested in the great artist's expenditure for brushes and canvas? Then why are you so interested in the cost of equipment and materials when you consider research?" A fruitful research is worth incomparably more than the expenditure involved in it. The cost of research in money terms is incommensurable with its cultural value. I asked: "How much funds in the Commissariat's opinion should Isaac Newton have been given for his work which culminated in the discovery of universal gravitation?" Yet the Commissariat remained adamant . . . and I think they would have gained the upper hand were it not for the intervention of the U.S.S.R. Council of People's Commissars. Finally our Institute was given a simplified financial system which saved the director from everyday troubles and endless "combinations" of work. Under the new system, for example, the institute employs a single bookkeeper, and when we are short-handed he finds time to help us in testing equipment, taking records and making measurements.'

The Institute was attractively sited with gardens and parks around it, and the living quarters looked out on a tennis court, which became a skating rink in winter. There was a magnificent house close by for the Kapitzas, which the Russians called the 'Cottage' (under the impression that this was the English word for any detached house) even though the house looked more like a small palace. Not only was the Institute well equipped, but it also provided a high standard of technical assistance. Indeed Kapitza had an extraordinary flair for picking good people to assist him. The glassblower, A. V. Petushkov, was a virtuoso who loved the challenge of a difficult problem. Similarly, the senior mechanics Minakov and Yakovlev, and Kapitza's personal assistant S. I. Filimonov, who later got a doctorate and became an important member of the scientific staff, were all masters of their craft. Another key figure was Kapitza's secretary, Oleg Pisarzhevski, who was able to shield Kapitza from many administrative chores. Later he left to become a scientific journalist and was replaced by Pavel Rubinin, a man of many parts, who again was well able to deal with many matters on his own initiative.

The main themes of the I.F.P. for its first 20 years or so were essentially the same as those of the Mond: magnetism and low temperature physics, though later the scope was extended to include plasma physics, which became Kapitza's chief personal interest. His first project (44) in Moscow was to extend his earlier Zeeman effect experiments to the threefold higher magnetic fields of the dynamo method. Once again the experiments confirmed the validity of the theory up to the highest fields and nothing very exciting turned up, but this work was a morale booster in demonstrating that the Cambridge equipment was able to work just as effectively in its new environment. It was also somewhat of a swan song because this was the last time the high field equipment was used. It remained, however, enshrined in its magnet hall and became essentially a museum piece to show to visitors.

In parallel with this high field work, rapid progress was being made in completing the cryogenic facilities, and Kapitza took up two new lines of research that became his main preoccupation for the next ten years. One of these, the study of the transport properties of helium II (liquid helium below its $\lambda$-point) led to a series of remarkable discoveries and the award of two Nobel prizes: one to Landau for his theory of quantum liquids, based largely on Kapitza's experiments and the other, very belatedly, to Kapitza himself. The second line of research was more technically oriented: the development of a new and more efficient method of liquefying air and hence, by fractional distillation, of manufacturing oxygen on an industrial scale.

Kapitza's interest in liquid helium was stimulated by the Keesoms' discovery (1936) of the extraordinarily high thermal conductivity of helium II and by the experiments of Allen, Peierls & Uddin (1937), which showed a nonlinear dependence of heat flow on temperature

gradient. This led him to the idea that the heat transport might be associated with convection, brought about by an extremely low viscosity, rather than conduction. To observe such a low viscosity, however, the liquid flow must be through very fine channels, otherwise it would be governed entirely by turbulence, and to achieve this Kapitza studied the radial flow under gravity of the liquid between two optical flats separated by a gap of order 1 μm or less. The result was that the viscosity was indeed many orders of magnitude less than had been inferred from previous experiments in which the flow must have been turbulent rather than laminar. Indeed Kapitza was able to set only an upper limit to any possible viscosity and suggested, by analogy with superconductivity, that below the λ-point liquid helium became a 'superfluid'.

By coincidence, similar results were being obtained in Cambridge by Allen & Misener at the same time, though entirely independently. Their flow was through fine capillaries rather than through a narrow gap and they too found only an upper limit (comparable to Kapitza's result) to any possible viscosity. The Cambridge group first learnt of the Moscow experiments from W. L. Webster, who had been visiting Kapitza, and showed Cockcroft the note Kapitza had sent to *Nature*. Cockcroft then wrote to Kapitza about the Cambridge work and sent a note of Allen and Misener's results to *Nature*, in which they commented that Kapitza's explanation of the high heat conductivity by convection was probably too simple. The two notes appeared together ((43) and Allen & Misener 1938), though the appended dates of submission (3 and 22 December 1937) imply a slight priority for Kapitza.

During the short remaining time before the interruption of the war, an intense effort both in Moscow and in the West went into trying to understand the behaviour of helium II more fundamentally. Important contributions were made by Allen & Jones, who discovered the thermo-mechanical ('fountain') effect; by H. London, who predicted the inverse mechanocaloric effect; by Daunt & Mendelssohn, who demonstrated this inverse effect; and by Tisza, who produced a phenomenological two-fluid theory, though without any sound fundamental basis. The decisive results were, however, obtained in a series of ingenious experiments by Kapitza (47, 48). It was this work that enabled Landau (1941) to develop a fundamental quantum theory of liquid helium that provided a rigorous basis for the two-fluid idea and also led to important new predictions.

The major result of the experiments was to show that heat flow was accompanied by mechanical flow. Thus when heat flowed from a closed bulb containing helium II through a capillary into a bath of helium II, a light vane suspended opposite the mouth of the capillary could be seen to deflect. A rather spectacular demonstration of the effect, involving a masterpiece of Petushkov's glassblowing art, was provided by a miniature reaction turbine consisting of six capillaries radiating out of a blackened closed bulb and bent near their ends to point round the circumference of

the circle. This whole 'spider', as Kapitza called it (in spite of its entomological inadequacy), was mounted on a needle pivot and when light shone on the bulb the spider began to spin rapidly, up to 120 rev./min, in reaction to the liquid flowing out of its legs.

This outflow of liquid accompanying heat flow immediately raised the question of how the amount of liquid in the closed bulb was conserved. At first Kapitza proposed that there was a compensating flow inwards restricted to a thin layer close to the capillary wall and having a different heat content from the bulk liquid because of proximity to the wall. However, a more convincing explanation was provided by Landau's theory, according to which the inflowing liquid was the superfluid component of the two-fluid ensemble. This component exerted no force on the capillary or the suspended vane and carried no entropy, while all the entropy of the liquid was concentrated in the outflowing normal component that deflected the vane or made the spider rotate. In his second study (48) Kapitza used a somewhat different experimental arrangement in which liquid could be made to flow into a bulb containing a heater and thermometer through a very narrow channel (a gap between flats, as in his superfluidity experiments). He found that for sufficiently low power in the heater there was no temperature difference between inside and outside and in this régime the power in the heater was directly proportional to the rate of mass flow into the bulb. The detailed analysis of these experiments provided a quantitative confirmation of the two-fluid theory according to which only the superfluid component, carrying no entropy, could flow into the bulb.

In the course of these studies Kapitza also discovered that if heat flows from a solid surface into helium II there is a temperature discontinuity at the surface. This has come to be known as the 'Kapitza boundary effect' and has been the subject of much study in postwar years. All this work was broken off in 1941 when the Soviet Union was invaded, and Kapitza himself did not continue it after the war. However, he did actively encourage further work and two of the most striking new predictions of Landau's theory were convincingly confirmed in his Institute. One was the existence of 'second sound', essentially a temperature rather than a pressure wave, as E. M. Lifshitz (1944) pointed out, which was demonstrated by Peshkov (1944). The other was an elegant and direct demonstration of the two-fluid theory by Andronikashvili (1946), who showed that the moment of inertia of a pile of closely packed disks was enhanced by only the normal component of the liquid between the discs. An entertaining account of all this work is given in Andronikashvili's (1980) memoirs.

Kapitza's other major research effort at this time was the development of a new method of liquefying air, with a view to simplifying and cheapening the large-scale production of oxygen for industrial use. The methods then in use achieved the final cooling by the Joule–Thomson

effect, which involves high pressures, so that the machinery was inevitably bulky and expensive. Kapitza (45, P13) proposed instead the use of an expansion turbine, requiring only about 5 atm, to cool the air all the way to its liquefaction temperature, and showed that with a novel radial–axial design a higher efficiency could be achieved than was possible with the conventional impulse type of turbine.

The working out of the whole scheme involved a profound understanding of both physical and engineering principles, and the successful construction of a laboratory-scale liquefier in less than 2 years was a considerable achievement. The machine delivered about 30 kg of liquid air per hour after a start-up time of only about 20 min and with an expenditure of 1.2 kW h $kg^{-1}$, a good performance even by today's standards. An important part of the design was how to overcome the instabilities associated with the very fast rotation of the turbine rotor. The analysis involved was presented in a separate paper (46) and the methods were patented (P12, P16). P. B. Moon (1978) in his Rutherford Lecture, mainly about other applications of high-speed rotors, comments enthusiastically on the elegance of Kapitza's analysis, which he found directly applicable to his own work.

Shortly after Russia entered the war, the I.F.P., together with many other Academy institutions, was evacuated to Kazan, where the equipment was set up in the University, and one of the main efforts was the rapid development of an industrial-scale plant for oxygen production based on the expansion turbine method. It was a remarkable achievement that in spite of the very difficult conditions (lack of fuel, extreme shortage of food, etc.) a large-scale pilot plant was successfully completed during the two years that the evacuation lasted. Kapitza was appointed head of a new government department dealing with oxygen production and after the return from evacuation in 1943 he was also in charge of a factory being built at Balashikha near Moscow. All this work resulted in a number of patents (P14–P18). His design of expansion turbine has proved to be the basis of much of the world's industrial production of oxygen and his pioneer work is still greatly respected by industrial engineers.

Kapitza received many official recognitions of his achievements: election as a full Academician in 1939, Stalin prizes in 1941 and 1943, Orders of Lenin in 1943 and 1944, and for his oxygen work the title of Hero of Socialist Labour together with a third Order of Lenin and the Hammer and Sickle gold medal in May 1945. But soon afterwards there were ominous signs that all was not well. To quote Andronikashvili (1980):

> 'All sorts of odd things began to happen in the Institute in the spring of 1946. Commissions kept arriving to investigate the activities of the Institute and then the status of the commissions got higher with the arrival of Ministers. They began to investigate

> Kapitza's oxygen project. They spoke badly of the factory at Balashikha—not productive enough, uneconomical . . . late in meeting its targets etc . . .'

All this culminated in the summer of 1946 in a curt announcement on the bulletin board that:

> 'P. L. Kapitza, having shown a cavalier attitude to both Soviet and foreign achievement in the technology of oxygen production and having failed to meet the scheduled dates for introducing new installations into the metallurgical industry, is relieved of his duties as Director of the Institute for Physical Problems.'

It is likely, however, that this sudden turnabout in Kapitza's fortunes occured for quite different reasons than those officially stated, though no doubt the fact that Kapitza had trodden on many toes in putting through his oxygen project also played its part (see G86). The real reason was probably Kapitza's refusal to work in the organization set up under Beria (then head of the Ministry of Internal Affairs, which controlled the K.G.B.*) to develop the Soviet atom bomb immediately after the American bombs had been exploded (see, for instance, York 1976). Kapitza is said to have written to Stalin that Beria was 'like the conductor of an orchestra with the baton in his hand but without a score'. Beria wanted to arrest Kapitza for such insubordination, but Stalin in his unpredictable way, perhaps because he admired Kapitza's courage, vetoed this proposal and said dismissal would be sufficient.

This was not Kapitza's first brush with the K.G.B., and perhaps the memory of that earlier occasion may have rankled and also played a part in his dismissal. During the great prewar purges Landau was arrested in 1938 and Kapitza showed great courage by intervening on his behalf. He wrote to Stalin saying that his work could hardly go on without Landau and later sent a reminder to Molotov. Eventually he was summoned by a high-ranking K.G.B. official who kept trying to offer Kapitza a fat dossier. Kapitza however succeeded in outfacing him, saying he understood nothing about legal technicalities, and insisting that though Landau may have had a sharp tongue he was certainly no spy. Landau was indeed soon released and many years later in a tribute for Kapitza's 70th birthday wrote (1964):

> 'These years [the late 1930s] are memorable to me for another but very sad reason. Because of a stupid denunciation I was arrested and accused of being a German spy. Now I can sometimes even find this funny, but then it was no joke. I spent a year in prison and it was clear that I couldn't last another 6 months—I was simply dying. Kapitza went to the Kremlin and announced that he would have to leave his

* The familiar name K.G.B. will be used throughout, though in fact it was called the N.K.V.D. in the 1930s and M.G.B. in the early 1950s.

Institute if I wasn't released. I was released. It is hardly necessary to say that such an action in those years required no little courage, great humanity and crystal-clear honesty.'

## Nikolina Gora, 1946–54

Although Kapitza was dismissed from his Institute he retained his position (and salary) as an Academician and chose to live at his dacha (country house) at Nikolina Gora, where he managed to carry on scientific work until he was reinstated eight years later, when it was acknowledged that the unfavourable report on his oxygen work was mistaken. Most of his effort went into building up a laboratory in various outhouses at the dacha where, aided by his sons (particularly Sergei) and Filimonov, he could continue experimental work. This laboratory was jokingly called the Izba for Physical Problems (izba means a peasant's hut) with the same initials as the I.F.P.

He also wrote theoretical papers on various topics, such as heat transfer in two-dimensional turbulent flow (50), wave flow in a thin layer of viscous fluid (51, 52), formation of sea waves by wind (54), and the hydrodynamic theory of lubrication in rolling (59). All this work was characterized by considerable ingenuity in simplifying the mathematical analysis without losing the essence of the problem; the lubrication work is described as 'classic' in a recent review (Dowson 1979). The theory of wave flow along liquid flowing over the outside surface of a cylindrical tube was verified experimentally in collaboration with Sergei (53), with carefully designed apparatus entirely built with the relatively primitive resources of the izba, and rather in the style of Kapitza's early work in the Cavendish.

But a good deal of other work was going on at Nikolina Gora that because of its potential applications was initially classified and only published in 1962 (66, 67). An early hint of this work was a paper on ball lightning (60) in which it was suggested that the ball of hot gas is essentially a resonator fed by high-frequency radio waves typically of wavelength 50 cm, which are somehow generated by the storm. Later Kapitza tried to verify this idea experimentally (73), but no conclusive evidence was obtained and his hypothesis has not found general acceptance. Another and less direct hint (55) was a novel theoretical demonstration of the stability of an inverted pendulum with the point of support vibrating much faster than the period of the pendulum; an elegant experimental demonstration of the effect was also described.

As was later made clear, both these studies were spin-offs from Kapitza's main thrust, the development of powerful high-frequency radio generators of the magnetron type. In a series of researches, essentially completed by 1952, he had worked out the detailed theory in an explicit form of a 'planatron' (essentially a linear magnetron, some-

times called a 'dicotron') and constructed and tested a working model. The merit of his approach was that it provided an objective method of design of powerful oscillators, in contrast to the trial and error methods that had mostly been used up to then. It was the formal analogy of the differential equations involved with those of the vibrating pendulum support problem that led him to tackle the latter by the same technique. The planatron gave several kilowatts output of 10 cm waves and a striking observation was that if the waves fell on a quartz sphere containing low-pressure helium, a vivid discharge took place, which after a few seconds raised the temperature enough to melt the quartz. It was this observation that led to the ball lightning hypothesis, but more significantly, it led to the idea that with a still more powerful microwave source a plasma might be heated sufficiently to produce a thermonuclear reaction. The next step was the development of a more powerful type of magnetron, called a 'nigotron' (after Nikolina Gora), but by the time this was done Kapitza was back in his old Institute. What had been achieved at Nikolina Gora was impressive considering the limited resources available and again illustrates Kapitza's drive and his intellectual powers in formulating and solving difficult problems. However, during the 10 years before publication much of this achievement was overtaken by developments in microwave generators elsewhere (for reviews see Okress 1961 and Harvey 1963) and its impact has not been great outside the Soviet Union.

## Moscow, 1954–84

Once back in Moscow Kapitza worked mainly on the nigotrons, though he still maintained his interest in liquefaction techniques (61, 64, 65, 85). The scale of the high power electronics work was greatly increased and a special wing was set aside for the 'Physical Laboratory' where the nigotrons could be developed and applied. The secrecy of the work was partly lifted in 1962 and more completely in 1969, when striking observations, made as early as 1958, of very hot plasma were described (77). The glowing region had a sharply defined shape that could be varied from spherical to stringlike and it could be kept well away from the walls of the vessel into which the microwaves were fed. Spectral studies suggested that electron temperatures of order $10^6$ K were reached, though the ion temperatures were much lower.

Kapitza was convinced that it should be possible to exploit such hot plasma strings to reach the conditions necessary for a sustained thermonuclear reaction and that his microwave heating approach offered a better chance of success than the Tokamak and laser heating methods so actively being pursued in the Soviet Union and in the West. He proposed a schematic design for a realistic reactor (78, P19) and continued hot plasma experiments (79–84) on an ever larger scale to the end of his life. Although he claimed to have achieved electron temperatures as high as

$5\times10^7$K (83, 84) his claim may have been based on faulty diagnostic methods, and some of the basic assumptions in his analysis of the problem have been questioned. The whole research must be regarded as a brave effort in Kapitza's typical style of going it alone and taking little notice of what others were doing around him, but he could hardly compete with the big battalions. Although he produced much interesting plasma physics, his efforts were largely eclipsed by the work of the powerful teams at the Kurchatov Institute and in the West using the Tokamak system. It should be noted, however, that there is an echo of Kapitza's ideas in the latest Tokamaks that make use of microwave heating of plasma confined in quasi-d.c. magnetic fields (see, for example, Alikaev *et al.* 1985).

In his style of running his Institute, Kapitza modelled himself closely on Rutherford and often explicitly said so. When giving advice about possible research projects he was fond of telling how, many years ago, Moseley had suggested several projects to Rutherford and Rutherford with unerring intuition had picked the winner. Like Rutherford he insisted that laboratory work should stop at 6 pm (later 7 pm) except by special permission, so that research workers would have time to reflect. Just as Rutherford had once insisted that Kapitza should take a holiday after a period of hectic work, so Kapitza too insisted on occasion that his subordinates must take a break whether it suited them or not. Oliphant (1972) recalls an occasion when Rutherford was so impatient to see a photographic oscillograph record that he ruined it and stained his clothes by awkward handling before the film had been properly processed; so too was Kapitza famous for spoiling films and wetting his clothes by undue impatience. Appalling handwriting was another common feature, though this was perhaps coincidental. Again like Rutherford, he made a point of keeping in touch with what everyone was doing, and made shrewd comments even if the topic was remote from his personal research. Often he would achieve this contact through a rule he made that any new apparatus had to be approved by him before it was made by the workshop and this gave him the opportunity of reviewing progress from time to time. Also it was usual for any completed work to be presented at his seminar before it could be sent for publication.

Kapitza's fondness for nicknames such as 'Crocodile' and 'Baron' has already been mentioned—another was 'Rat' for Anna because of her burrowing in archaeological archives (his calling out 'Rat' to her at the theatre caused some alarm to neighbouring ladies on one occasion!). It was perhaps poetic justice that he and his Institute should also be widely known by nicknames. The Institute and the seminar were usually called the Kapichnik, which sounds a bit comic in Russian, though otherwise harmless, but his own nickname was less flattering. It came about when a visitor asked someone in the Institute 'What sort of a chap is your boss? Is he a man or a beast?' The answer was a bit hesitant: 'It's difficult to say—a

bit of each perhaps' and the visitor immediately said 'Oh, I see, a centaur' and 'centaur' stuck for many years, though later it was gradually replaced by the more affectionate 'grandfather'.

There was indeed some truth behind the 'centaur' characterization. He could on occasion fly into a rage and be very abusive (once again like Rutherford), he could be autocratic in dealing with subordinates, and if, unreasonably, he took against someone, it was difficult to make him change his mind. On the other side of the coin, however, he could be extremely generous in helping where help was needed—indeed not only generous, but courageous. His intervention to save Landau is only one example. Other examples were his public denunciation of anti-semitism (G18), his speaking out for the dissident geneticist, Zh. Medvedev, who had been put in a psychiatric institution, his sponsoring talks on genetics at his seminar by opponents of Lysenko, at a time when Lysenko was still in favour, and opposing a move to expel Sakharov from the Academy by the reminder that even the Nazis had never expelled Einstein from the Prussian Academy. Moreover, everyone in the Institute greatly appreciated the remarkable working conditions he had created, probably unique in the Soviet Union, and the protection they enjoyed there in various ways. And of course, above all, when he was in the mood, it was a delight to experience his sense of fun and warm cordiality, which he would extend to everyone irrespective of rank.

During the last half of his life, although Kapitza was very actively pursuing his laboratory work he managed to find time for many other activities. He thought deeply about the organization of science and its relation to technology and in various speeches and articles he was often sharply critical of the establishment. An example is his report (G28) on the organization of his own Institute, where he emphasizes that pure scientists should not be made to develop the technical applications of their work:

> '. . . if Semenov [his close friend] were to attempt to build an internal combustion engine . . . the result would only be a waste of precious time and energy best spent in pure science, where he is nothing less than a virtuoso. . . . If the singer cannot possibly accompany his own songs, why force him to do it? . . .'

However, Kapitza points out that he himself is an exception to the rule, being both scientist and engineer!

Closely related was Kapitza's active interest in education (G59, G64, G66, G80). He took a leading part in organizing a new and somewhat elitist kind of university institution, the Moscow Physico-Technical Institute (M.F.T.I.) where students came into active contact with research during their last undergraduate years. This enabled the research institute to get to know the students well and so to be able to pick the best of them for further research in a rational way. The M.F.T.I. was set up in

1946 just before Kapitza's demotion and he lectured there for a year or two afterwards; after his reinstatement he continued to be involved in the organization of M.F.T.I. but did not give many lectures himself. He took an active part in examining, and devised (partly during his time at Nikolina Gora) many problems of a kind requiring intuition and understanding rather than mere book learning (B5). He was concerned with maintaining a high standard of scientific publication and from 1956 was Editor-in-chief of the *Journal of Experimental and Theoretical Physics*. He also took an active interest in global problems such as ecology and energy (G69, G76–G79) and participated in the Pugwash movement.

He was much in demand for keynote addresses at conferences and could on occasion make somewhat sharp remarks, such as in commenting on what he thought was rather an inadequate level of presentation at a low-temperature meeting: 'It is important not only that the dish should be nutritious, but that it should be well served'. He could always rise to the occasion and say something original when he had to respond after a ceremonial award or give an address commemorating some great figure in science. In such addresses and articles (B8) he showed a talent for picking out what it was that characterized the life and work of his subjects and made them great. Particularly interesting are his recollections of Rutherford (G10, G55). In 1971 he organized a Rutherford Centenary Colloquium in Moscow (B7) to which he invited the few British physicists still alive with personal memories of the great man. One of these, T. E. Allibone, in a letter to *The Times* (3 November 1971) complained that he had been unable to persuade the British Post Office to issue a Rutherford commemorative stamp, and said that he had asked Kapitza how he managed to persuade the Soviet postal authorities to issue one. Kapitza pointed to a second telephone on his desk and said it was connected


The Rutherford stamp.

directly to the Kremlin. He had picked it up and said 'Mr Brezhnev, I want a stamp to commemorate the centenary of the birth of Lord Rutherford' and Brezhnev had said 'O.K.'. Allibone regretted that he had no second telephone on his own desk!

For many years Kapitza was denied the possibility of travelling beyond the Eastern Bloc countries, even though he was frequently invited. However, in 1965 the ice was broken when he was allowed to travel to Copenhagen to receive the Niels Bohr Gold Medal of the Danish Engineering Society. A year later he was awarded the Rutherford Medal of the Institute of Physics and returned to England after a lapse of 32 years. He made Cambridge his headquarters and was delighted to find so many of his old friends still there, most of them now establishment figures. He particularly appreciated the hospitality of Churchill College, of which his old friend Cockcroft was then Master, and while in Cambridge he arranged for his former house at 173 Huntingdon Road to be used by the College as a hostel (now known as Kapitza House), particularly for Soviet academic visitors. In subsequent years he was able to indulge his love of travel to the full and visited Canada, U.S.A., India, Switzerland and many other countries to receive honorary degrees, honorary membership of academies and medals (for a full list see Kedrov 1984). He came back to England on two more occasions: in 1973 to receive the Simon Memorial Prize of the Institute of Physics and in 1976 to give the Bernal Lecture (G78) at the Royal Society. On the second occasion, although saddened to find so many of his old friends no longer alive, he enjoyed the opportunity of making new friends during his stay at Churchill College, which had made him an Honorary Fellow in 1974.

The culminating event was the award of the Nobel Prize in 1978, for his work in low-temperature physics. In his Nobel lecture (83), Kapitza rather wittily made it clear that he thought the award was a bit belated:

> '. . . I left this field some 30 years ago, although at the Institute under my direction low temperature research is still being done. Personally I am now studying plasma phenomena at those very high temperatures that are necessary for a thermonuclear reactor. . . . I think that as a subject for the lecture it is of more interest than my past low temperature work. For it is said "les éxtrèmes se touchent"'.

He then went on to review his plasma work. The visit to Stockholm has been amusingly described by Rubinin (1984).

The Kapitzas also led an active social life in Moscow, enjoying the company not only of their family (five grandchildren and seven great-grandchildren as well as the two sons and numerous nephews, nieces and cousins) but of a wide circle of friends among the 'intelligentsia': artists, sculptors, writers, musicians, actors, film directors and so on. On his 80th

birthday almost everyone in the Soviet cultural world was at the huge party given at Nikolina Gora. Kapitza supported many non-conformist artists by arranging exhibitions of their work at the Institute, particularly during the 1960s. These exhibitions were the talk of Moscow and helped the artists sell some of their work. Kapitza himself bought a few, though his own collection was mostly of Kustodiev, Fonvisin and Falk, whose paintings had been frowned on in the Stalin era but had, by the 1960s, partly returned to favour. Kustodiev's fine portrait (see p. 337) of Kapitza as a young man is at present on loan to Darwin College, Cambridge from the Fitzwilliam Museum. The Kapitzas' social life extended also to visitors from abroad, particularly those with Cambridge connections. Many such visitors recall the warm hospitality they received in Moscow or Nikolina Gora and Kapitza's lively conversation.

It is astonishing that he managed to combine such a busy social and intellectual life outside the laboratory with his intensive scientific work. This intense activity continued almost to the end of his life, though latterly he was becoming rather frail. At the end of March 1984 he had a severe stroke and died on 8 April after a few days in hospital. The announcement of his death in *Pravda* was signed by all the Politbureau as well as by the top scientific establishment, and many tributes have appeared in Soviet journals (particularly in *Priroda* of June 1984) as well as abroad. Great preparations had been in hand for his 90th birthday, but sadly it was instead a memorial meeting at the Institute, a moving occasion with many personal recollections by close colleagues and friends.

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One of the photographs in the frontispiece was taken in 1937, the other was taken by the author in 1966 at Cambridge.

## Bibliography*

### *Scientific Papers*


(1) 1916 Electron inertia in molecular Ampere currents. *J. Russ. Phys. Chem. Soc. Phys. Sect.* **48**, 297 (R).

(2) The preparation of Wollaston fibres. *J. Russ. Phys. Chem. Soc. Phys. Sect.* **48**, 324 (R).

(3) 1919 A method of reflection from crystals. *Bull. Roentgenol. Radiol.* **1**, 33 (R).

(4) The Koch recording microphotometer. *Bull. Roentgenol. Radiol.* **1**, 54 (R).

(5) 1921 The dependence of the emission edge of the continuous X-ray spectrum on the azimuth of the emission and the influence of the anticathode metal. *Usp. fiz. Nauk* **2**, 322 (R).

(6) 1922 (With N. N. Semenov) On the possibility of an experimental determination of the magnetic moment of an atom. *J. Russ. Phys. Chem. Soc. Phys. Sect.* **54**, 159.

(7) The loss of energy of an α-ray beam in its passage through matter. I. Passage through air and $CO_2$. *Proc. R. Soc. Lond.* A **102**, 48.

(8) Note on the curved tracks of β-particles. *Proc. Camb. phil. Soc.* **21**, 129.

(9) 1923 On the theory of δ-radiation. *Phil. Mag.* **45**, 989.

(10) Some observations on α-particle tracks in a magnetic field. *Proc. Camb. phil. Soc.* **21**, 511.

(11) 1924 A method of producing strong magnetic fields. *Proc. R. Soc. Lond.* A **105**, 691.

(12) α-ray tracks in a strong magnetic field. *Proc. R. Soc. Lond.* A **106**, 602.

(13) (With H. W. B. Skinner) The Zeeman effect in strong magnetic fields. *Nature, Lond.* **114**, 273.

(14) 1925 (With H. W. B. Skinner) The Zeeman effect in strong magnetic fields. *Proc. R. Soc. Lond.* A **109**, 224.

(15) 1926 Over-tension in a condenser battery during a sudden discharge. *Proc. Camb. phil. Soc.* **23**, 144.

(16) 1927 Further developments of the method of obtaining strong magnetic fields. *Proc. R. Soc. Lond.* A **115**, 658.

(17) 1928 The study of the specific resistance of bismuth crystals and its change in strong magnetic fields and some allied problems. I. The growth of crystal rods with a definite orientation of the crystal planes and the specific resistance of bismuth crystals. *Proc. R. Soc. Lond.* A **119**, 358.

(18) The study of the specific resistance of bismuth crystals and its change in strong magnetic fields and some allied problems. II. The method and apparatus for observing the change of resistance of bismuth in strong magnetic fields. *Proc. R. Soc. Lond.* A **119**, 387.

(19) The study of the specific resistance of bismuth crystals and its change in strong magnetic fields and some allied problems. III. The change of resistance of bismuth and the time lag in magnetic fields. *Proc. R. Soc. Lond.* A **119**, 401.

(20) 1929 The change of electrical conductivity in strong magnetic fields. I. Experimental results. *Proc. R. Soc. Lond.* A **123**, 292.


* All the scientific papers and many of the 'general' papers, are reprinted (in English translation where necessary), in *Collected Papers of P. L. Kapitza* (see B2 below). In this bibliography the titles of papers in Russian (indicated by (R)) and other languages are given in English translation. *HPE* denotes the series *High Power Electronics* (R) and *HPME* the English translation, *High Power Microwave Electronics* (see B1 below); *ETP* denotes the English translation of *Experiment, Theory and Practice* (see B8 below).

(21) 1929 The change of electrical conductivity in strong magnetic fields. II. The analysis and the interpretation of the experimental results. *Proc. R. Soc. Lond.* A **123**, 342.

(22) (With R. H. Fowler) Magnetostriction and the phenomena of the Curie point. *Proc. R. Soc. Lond.* A **124**, 1.

(23) Metallic conductivity and its change in a magnetic field. *Metallwirtschaft.* **19**, 443 (German).

(24) A property of superconducting metals. *Nature, Lond.* **123**, 870.

(25) Magnetostriction of diamagnetic substances in strong magnetic fields. *Nature, Lond.* **124**, 53.

(26) 1930 The change of resistance of gold crystals at very low temperatures in a magnetic field and supra-conductivity, *Proc. R. Soc. Lond.* A **126**, 683; *Phys. Z.* **31**, 713 (German).

(27) Methods of experimenting in strong magnetic fields. *Proc. phys. Soc. Lond.* **42**, 425.

(28) Experimental studies in strong magnetic fields. In *Proceedings of the 6th Solvay Congress, Le Magnetisme*, p. 458. Paris: Gauthier-Villars. *Usp. fiz. Nauk* **11**, 533 (1931) (R).

(29) 1931 The study of the magnetic properties of matter in strong magnetic fields. I. The balance and its properties. *Proc. R. Soc. Lond.* A **131**, 224.

(30) The study of the magnetic properties in strong magnetic fields II. The measurement of magnetization. *Proc. R. Soc. Lond.* A **131**, 243.

(31) (With W. L. Webster) A method of measuring magnetic susceptibilities. *Proc. R. Soc. Lond.* A **132**, 442.

(32) Reply to some remarks by O. Stierstadt about an error of principle in my measurements of the change in resistivity in strong magnetic fields. *Z. Phys.* **69**, 421 (German).

(33) 1932 The study of the magnetic properties in strong magnetic fields. III. Magnetostriction. *Proc. R. Soc. Lond.* A **135**, 537.

(34) The study of the magnetic properties in strong magnetic fields. IV. The method of measuring magnetostriction in strong magnetic fields. *Proc. R. Soc. Lond.* A **135**, 556.

(35) The study of the magnetic properties in strong magnetic fields. V. Experiments on magnetostriction in diamagnetic and paramagnetic substances. *Proc. R. Soc. Lond.* A **135**, 568.

(36) (With J. D. Cockcroft) Hydrogen liquefaction plant at the Royal Society Mond Laboratory. *Nature, Lond.* **129**, 224.

(37) 1933 The change of resistance of metals in magnetic fields. *Leipziger Vorträge*, p. 1. Leipzig: S. Hirzel.

(38) (With P. A. M. Dirac) The reflection of electrons from standing light waves. *Proc. Camb. phil. Soc.* **29**, 297.

(39) 1934 Liquefaction of helium by an adiabatic method without precooling with liquid hydrogen. *Nature, Lond.* **133**, 708.

(40) The liquefaction of helium by an adiabatic method. *Proc. R. Soc. Lond.* A **147**, 189; *Usp. fiz. Nauk* **16**, 145 (1936) (R).

(41) 1937 (With C. J. Milner) A modified potentiometer for measuring very small resistance. *J. scient. Instrum.* **14**, 165.

(42) (With C. J. Milner) Note on the use of liquid nitrogen in magnetic experiments. *J. scient. Instrum.* **14**, 201.

(43) 1938 Viscosity of liquid helium below the λ-point. *Nature, Lond.* **141**, 74; *Dokl. Akad. Nauk* **18**, 21 (R).

(44) (With P. G. Strelkov & E. Laurmann) The Zeeman and Paschen–Back effects in strong magnetic fields. *Proc. R. Soc. Lond.* A **167**, 1; *Zh. eksp. teor. Fiz.* **8**, 276 (R).

(45) 1939 Expansion turbine producing low temperatures applied to air liquefaction. *J. Phys. USSR*, **1**, 7; *Zh. tekh. Fiz.* **9**, 99 (R).

(46) Influence of friction forces on the stability of high-speed rotors. *J. Phys. USSR*, **1**, 29; *Zh. tekh. Fiz.* **9**, 124 (R).

(47) 1941 The study of heat transfer in helium II. *J. Phys. USSR* **4**, 181; *Zh. eksp. teor. Fiz.* **11**, 1 (R).

(48) Heat transfer and superfluidity of helium II. *J. Phys. USSR* **5**, 59; *Zh. eksp. teor. Fiz.* **11**, 581 (R); *Phys. Rev.* **60**, 354 (abridged version).

(49) 1944 On Professor S. Ya. Gersh's paper 'Low and high pressures in deep-cooling systems'. *Vest. Mashinost.* No. 7/8, p. 43 (R).

(50) 1947 Theoretical and empirical expressions for the heat transfer in a two-dimensional turbulent flow. *Dokl. Akad. Nauk* **55**, 595 (R); *C. r. Acad. Sci. URSS* **55**, 591.

(51) 1948 Wave flow of thin layers of a viscous fluid. I. Free flow. *Zh. eksp. teor. Fiz.* **18**, 3 (R).

(52) Wave flow of thin layers of a viscous fluid. II. Fluid flow in the presence of continuous gas flow and heat transfer. *Zh. eksp. teor. Fiz.* **18**, 19 (R)

(53) 1949 (With S. P. Kapitza) Wave flow of thin layers of a viscous fluid. III. Experimental study of flow in the wave regime. *Zh. eksp. teor. Fiz.* **19**, 105 (R).

(54) On the problem of the formation of sea waves by the wind. *Dokl. Akad. Nauk* **64**, 513 (R).

(55) 1951 Dynamical stability of a pendulum with a vibrating point of support. *Zh. eksp. teor. Fiz.* **21**, 588 (R).

(56) Pendulum with a vibrating suspension. *Usp. fiz. Nauk* **44**, 7 (R).

(57) Evaluations of the sum of negative even powers of roots of Bessel functions. *Dokl. Akad. Nauk* **77**, 561 (R).

(58) Heat conductivity and diffusion in a liquid medium under periodic flow conditions. *Zh. eksp. teor. Fiz.* **21**, 964 (R).

(59) 1955 The hydrodynamic theory of lubrication in the presence of rolling. *Zh. tekh. Fiz.* **25**, 747 (R).

(60) On the nature of ball-lighting. *Dokl. Akad. Nauk* **101**, 245 (R); *Phys. Bl.* **14**, 11 (1958) (German).

(61) 1959 Design of a helium-liquefying cycle with expansion engines connected in cascade. *Zh. tekh. Fiz.* **29**, 427 (R); *Sov. Phys. tech. Phys.* **4**, 377.

(62) (With V. A. Fok & L. A. Vainshtein) Static boundary problem for a hollow cylinder of finite length. *Zh. tekh. Fiz.* **29**, 1177 (R); *Sov. Phys. tech. Phys.* **4**, 1077 (1960).

(63) (With V. A. Fok & L. A. Vainshtein) Symmetric electric oscillations of a perfectly conducting hollow cylinder of finite length. *Zh. tekh. Fiz.* **29**, 1188 (R); *Sov. Phys. tech. Phys.* **4**, 1088 (1960).

(64) 1961 (With I. B. Danilov) Expansion engine for liquefaction of helium. *Zh. tekh. Fiz.* **31**, 486 (R); *Sov. Phys. tech. Phys.* **6**, 349.

(65) 1962 (With I. B. Danilov) Cascade compression helium liquefier with no external refrigeration. *Zh. tekh. Fiz.* **32**, 457 (R); *Sov. Phys. tech. Phys.* **7**, 333.

(66) High power electronics. *HPE* **1**, 9 (R); *Usp. fiz. Nauk* **78**, 181 (R); *Soviet Phys. Usp.* **5**, 777 (1963); *HPME* **1**, 3 (1964); abridged version in *Priroda* No. 8, p. 21 (1964) (R).

(67) Characteristic oscillations of gridded resonators. *HPE* **1**, 159 (R); *HPME* **1**, 117 (1964).

(68) 1964 (With S. I. Filimonov & S. P. Kapitza) The theory of electronic processes in a continuous generator of the magnetron type. *HPE* **3**, 7 (R).

(69) 1965 Transducers of H-waves into E-waves. *HPE* **4**, 7 (R).

(70) (With L. A. Prozorova) Experimental study of a transducer. *HPE* **4**, 53 (R).

(71) Absolute measurements of a high-frequency field in a resonator. *HPE* **4**, 206 (R).

(72) 1968 (With L. A. Prozorova) New $H_{01}$-wave transducers. *HPE* **5**, 209 (R).

(73) Ball lightning and radio emission from linear lightning. *Zh. tekh. Fiz.* **38**, 1829 (R); *Sov. Phys. tech. Phys.* **13**, 1475 (1969).

(74) (With I. B. Danilov) Cascade helium liquefiers with piston type engines. In *Proc. 1st Cryogenic Eng. Conf. Tokyo and Kyoto, Japan, 1967*, p. 228. London: Heywood Temple Industrial Publication.

(75) (With S. I. Filimonov) Solenoid producing a magnetic field up to 30 kOe in a volume of 5 litres, using 500 kW. *Usp. fiz. Nauk* **95**, 35 (R); *Soviet Phys. Usp.* **11**, 299; *HPE* **6**, 147 (1969) (R).

(76) 1969 (With S. I. Filimonov & S. P. Kapitza) High continuous power double set nigotron. *HPE* **6**, 7 (R).

(77) Free plasma filament in high frequency field at highpressure. *Zh. eksp. teor. Fiz.* **57**, 1801 (R); *Soviet Phys. JETP* **30**, 973 (1970).

(78) 1970 A thermonuclear reactor with a plasma filament freely floating in a high frequency field. *Zh. eksp. teor. Fiz.* **58**, 377 (R); *Soviet Phys. JETP* **31**, 199.

(79) 1971 (With S. I. Filimonov) Apparatus for production of a free plasma filament. Determination of the current and resistance of the filament. *Zh. eksp. teor. Fiz.* **61**, 1016 (R); *Soviet Phys. JETP* **34**, 542 (1972).

(80) 1973 (With E. A. Tishchenko & V. G. Zatsepin) Active submillimeter diagnostics of the moving UHF discharge in deuterium at high pressure. In *Proc. 11th Int. Conf. Phenom. Ionized Gases* (ed. I. Stoll), p. 457. Prague: Czech. Acad. Sci.

(81) 1974 (With L. P. Pitaevskii) Plasma heating by magnetoacoustic oscillations. *Zh. eksp. teor. Fiz.* **67**, 1411 (R); *Soviet Phys. JETP* **40**, 701 (1975).

(82) 1975 Useful energy from thermonuclear reactors. *Pisma v Zh. eksp. teor. Fiz.* **22**, 20 (R); *Soviet Phys. JETP Letters* **22**, 9.

(83) 1979 Plasma and the controlled thermonuclear reactions, (Nobel Lecture). *Usp. fiz. Nauk* **129**, 569 (R); *Rev. mod. Phys.* **51**, 417; *Science, Wash.* **205**, 959; *Postepy Fiz.* **30**, 547 (Polish); *Cesk. Cas. Fyz.* **30**, 272 (1980) (Czech); *Wuli* **10**, 214 (Chinese); *ETP* p. 86; also in *Les Prix Nobel–1978* Stockholm and *Nobel Lectures*, Amsterdam: Elsevier.

(84) 1981 The study of thermonuclear problems conducted at the Institute for Physical Problems of the U.S.S.R. Academy of Sciences. In *Proc. 10th European Conf. Controlled Fusion and Plasma* **2**, 59.

(85) 1983 (With M. P. MALKOV and I. B. DANILOV) Apparatus for the liquefaction of nitrogen using the 'compound' system. *Khim. i Neftyanoe Mashinostr.* no. 5, p. 18 (R).

## *Popular, semipopular and miscellaneous general articles**

(G1) 1913 Cod-liver oil. *Argus* no. 10, p. 76 (R).

(G2) 1927 The future of magnetism. *Nature, Lond.* **119**, 809.

(G3) 1931 The production of and experiments in strong magnetic fields. *Trans. Oxf. Univ. jr scient. Club* (5), no. 4, p. 129; *ETP* p. 3.

(G4) 1934 A new method for the liquefaction of helium. *Sots. Rekonstr. Nauka* no. 8, p. 104 (R); *Pokroky Mat. Fyz. Astr.* **24**, 255 (1979). (Czech); *ETP* p. 8.

(G5) 1936 A great man (in memory of I. P. Pavlov). *Pravda* 27 February (R); *ETP* p. 32.

(G6) The constitution of the most powerful country. *Pravda* 15 July (R).

(G7) 1937 The construction and work of the Institute of Physical Problems of the U.S.S.R. Academy of Sciences. *Izv. Akad. Nauk SSSR Ser. fiz.* no. 3, p. 265 (R) (English version on p. 278); *ETP* p. 103.

(G8) Lord Rutherford. *Nature, Lond.* **140**, 1053.

(G9) In memory of Ernest Rutherford. *Izvestia* 21 October (R); In *Physics of the 20th century* p. 3 (1971). Moscow: Znanie (R); *ETP* p. 229.

(G10) Recollections of Professor E. Rutherford. *Usp. Khim.* **6**, 1605 (R); *Usp. fiz. Nauk* **19**, 1 (1938) (R); B3, p. 35 (R); *ETP* p. 231.

(G11) 1939 Liquid air. *Tekhnika Molod.* no. 3, p. 28 (R); *Plan. Khoz.* no. 2, p. 73 (R).

(G12) A new method of producing low temperatures for air liquefaction. *Sov. Nauka* no. 5, p. 150 (R).

(G13) 1940 Science for the people. *Komsomolskaya Pravda* 1 January (R); *ETP* p. 224.

(G14) On a stereoscopic cinema. *Mashinostroenie* 12 March (R).

(G15) Science fiction. *Detskaya Literatura* no. 4, p. 18.

(G16) 1941 The unity of science and technology. *Pravda* 17 June (R).

(G17) Stalin will lead us to a true and decisive victory. *Pravda* 23 June (R).

(G18) Speech at meeting of representatives of the Jewish people (24 August). In *Brother Jews of the whole world*, p. 22. Moscow: Gospolitizdat. (R).

(G19) Arm yourselves with knowledge. *Pionerskaya Pravda* 30 August (R).

(G20) Problems of liquid helium. *Sov. Nauka* no. 1, p. 33 (R); *Vest. Akad. Nauk SSSR* no. 2/3, p. 27 (R); *ETP* p. 12.

(G21) Rutherford. *Bolshaya Sovetskaya Entsiklopediya* (1st edition) **48**, 504 (R).

(G22) Rutherford. *Entsiklopedicheskii slovar' Granat* (7th edition) **36**, 852 (R).

(G23) Speech at scientists' anti-fascist meeting, Moscow 12 October. *Vest. Akad. Nauk SSSR* no. 9/10, p. 9 (R); *Chem. Prod.* **5**, 9.

(G24) 1942 Science and war. *Krasnaya Zvezda* 26 February (R); *Tekhnika Molod.* no. 7, p. 3 (R); *Science* **95**, 396.

(G25) On the 25th anniversary of the establishment of the Soviet state (unsigned editorial). *J. Phys. USSR* **6**, 233.

(G26) We fight for freedom. *Trud* 28 October (R).

(G27) 1943 Scholars of their country. *Vechernaya Moskva* 5 May (R).

* Some of these articles have appeared in several different journals and books in several languages and sometimes in different variants, with varying titles. Here, an English version is always cited where possible, but the list is not exhaustive as regards other versions. A few articles that have not been published except in *ETP* are omitted from this list, as are also numerous commemorative and obituary notices (mostly in *Usp. fiz. Nauk*) signed jointly by serveral authors including Kapitza.

(G28) 1943 Report on the organization of scientific work at the Institute for Physical Problems of the Academy of Sciences of the USSR. *Vest. Akad. Nauk SSSR* no. 6, p. 75 (R); *Pod znamenem marksizma* no. 7/8, p. 90 (R); *Vesmir* **37**, 3 (1958) (Czech); *ETP* p. 115.

(G29) Kiev. *Pravda* 11 November (R).

(G30) 1944 Problems of intensification of technological progress by oxygen. *Kislorod* no. 1, p. 1 (R).

(G31) On the superfluidity of liquid helium II. *Usp. fiz. Nauk* **26**, 133 (R); *Nauka Zhizn'* no. 3, p. 13 (R); In *Quantum liquids. Theory, experiment.* (1969) Moscow: Znanie, (R).

(G32) Superfluidity. *Bolshaya Sovetskaya Entsiklopediya* (1st edition) **50**, 423 (R).

(G33) Viscosity free flow of helium II. In *Proc. Conference on Viscosities of liquids and Colloid Solutions* **2**, 8 (1940). Moscow: Academy of Sciences USSR (R).

(G34) Isaac Newton. *Izv. Akad. Nauk SSSR ser. fiz.* **8**, 169 (R); *Priroda* no. 7, p. 3 (1977) (R).

(G35) 1945 (With A. Joffé and S. Vavilov) Professor L. I. Mandelstam. *Nature, Lond.* **156**, 105.

(G36) 1946 Letter to the Editor. *Radio* no.1, p. 2 (R).

(G37) 1956 250th anniversary of the birth of Benjamin Franklin. *Pravda* 17 January (R).

(G38) The scientific activity of Benjamin Franklin. *Usp. fiz. Nauk* **58**, 169 (R); B3, p. 21 (R); *Vest. Akad. Nauk SSSR* no. 2, p. 65 (R); *Analele Romino-soviet. Ser. Mat. Fiz.* **11**, 101 (1957) (Rumanian); *ETP* p. 300.

(G39) How is atomic war to be prevented? *Novoe Vremya* no. 39, p. 9 (R); *ETP* p. 337.

(G40) Sortilège du souvenir et de l'amitié. (Interview) *l'Humanité* 19 December (French).

(G41) 1957 Some aspects of the organization of scientific work. *Pravda* 4 May (R); *Scient. Wld, Lond.* no. 1 (5), p. 12 (1959).

(G42) The path of development of physics in U.S.S.R. in 40 years of Soviet power (unsigned editorial). *Zh. eksp. teor. Fiz.* **33**, 1081 (R); *Soviet Phys. JETP* **6**, 385 (1958).

(G43) 1959 (With L. A. Artsimovich and I. E. Tamm) Thoughtless scientific sensationalism. *Pravda* 22 November (R); *Soviet News* no. 4162, 27 November.

(G44) 1960 Introductory and closing remarks. In *Proc. All-Union Conf. Low Temp. Phys., Tbilisi 1958*, pp. 5 and 271. Academy of Sciences of Georgian SSR (R).

(G45) The future of science. *Scient. Wld, Lond.* no. 3/4, p. 4; *Nauka Zhizn'* no. 3, p. 18 (1962) (R); *Fiz. Szle* **25**, 341 (1975) (Hungarian); *Bull. atom. Scient.* April 1962, p. 3 (modified version); *ETP* p. 345.

(G46) 1961 Lomonosov and world science. *Izvestia* 18 November (R); B3, p. 3 (R); *Usp. fiz. Nauk* **87**, 155 (1965) (R); *Soviet Phys. Usp.* **8**, 720 (1966); *ETP* p. 279.

(G47) 1962 Theory, experiment, practice. *Ekonom. Gazeta* 26 March, p. 10 (R); *ETP* p. 155.

(G48) Interview with Kapitza. *Trybuna Ludu* 22 September (Polish).

(G49) 1963 In commemoration of Niels Bohr. *Priroda* no. 1, p. 67 (R).

(G50) 1965 The physicist and public figure Paul Langevin. B3, p. 54 (R); *l'Humanité* 19 December (1966) (French); *ETP* p. 315.

(G51) Effectiveness of scientific work. *Vest. Akad. Nauk SSSR* no. 2, p. 55 (R); *ETP* p. 161.

(G52) Introduction to F. Kedrov's book: *Ernest Rutherford* p. 3. Moscow: Atomizdat (R).

(G53) Man in the world of information (interview). *Komsomolskaya Pravda* 2 September (R); also in Parry (1968) p. 223.

(G54) 1966 Application of the achievements of science and technology. *Komsomolskaya Pravda* 20 January (R); *New Wld Rev.* April, p. 28; *ETP* p. 165.

(G55) Recollections of Lord Rutherford. *Proc. R. Soc. Lond.* A **294**, 123; *Nature, Lond.* **210**, 780; *Novy Mir* no. 8, p. 205 (R); *Fiz. Szle* no. 3, p. 65 (1967) (Hungarian); *Ceskoslovensky casopis profysiku* A **20**, 59 and 111 (Czech); *ETP* p. 251.

(G56) Aleksandr Alexandrovich Fridman. In *Selected works of A. A. Fridman*, p. 397. Moscow: Nauka (R); *ETP* p. 325.

(G57) 1967 Introductory remarks. In *Proc. 10th Int. Low Temp. Conf. Moscow, 1966* **1**, 4. Moscow: Viniti (R).

(G58) Invitation to an argument (interview). *Yunost* no. 1, p. 79 (R); *Polityka* 18 February (Polish); also in Parry (1968) p. 233.

(G59) Experiment is the basis of school physics teaching (interview). *Fizika shk.* no. 2, p. 3 (R); *Phys. Teach.* **6**, 82 (1968).

(G60) 1967 On the 50th anniversary of the Soviet State (unsigned editorial). *Zh. eksp. teor. Fiz.* **53**, 1178 (R).

(G61) 1969 Philosophy and the ideological struggle. *Vop. filos.* no. 5, p. 147 (R); *ETP* p. 342.

(G62) Lev Davydovitch Landau. *Biogr. Mem. Fell. R. Soc.* **15**, 141; *ETP* p. 327.

(G63) 1970 The centenary of the birth of V. I. Lenin (unsigned editorial). *Zh. eksp. teor. Fiz.* **58**, 1129 (R).

(G64) 1971 Science teaching and scientific method. *Scient. Wld* no. 1, p. 12; *Vop. filos.* no.7, p. 16 (R); *Phys. Teach.* **9**, 429; *Komsomolskaya Pravda* 31 July; *Fiz. Szle* **21**, 1 (Hungarian); *Fiziko-matematichesko spisanie* **14**, 347 (Bulgarian); see also discussion in *Vop. filos.* (1972) no. 9, p. 126 (R); *ETP* p. 204.

(G65) The role of an eminent scientist in the development of science. *Komsomolskaya Pravda* 28 August (R); *New Scient.* **51**, 639; *Tekhnika molod.* no. 1, p. 14, (1972) (R); *ETP* p. 271; also in (B7) p. 18 (R).

(G66) Remarks on the anniversary of the Physico-Technical Institute. In *The 50th Anniversary of the Physico-Technical Institute, Leningrad*, p. 28 (R); also in (B5) p. 42 (1972) (R); *ETP* p. 220.

(G67) Not a blind copy, but creativity (interview). *Komsomolskaya Pravda* 28 November (R).

(G68) 1972 The scientific activity of Rutherford. In *Ernest Rutherford selected scientific papers. Atomic structure and artificial disintegration of elements*, p. 495. Moscow: Nauka (R).

(G69) 1973 Three aspects of the global problem of relation between man and nature. *Vop. filos.* no. 2, p. 37 (R); *Pravda* 15 May; *Scient. Wld, Lond.* no. 2, p. 17; *Bull. Atom. Scient.* no. 1, p. 40 (1981); *ETP* p. 365.

(G70) Basic factors in the organization of science and how they are handled in the U.S.S.R. *Daedalus* **102**, 167; *ETP* p. 183.

(G71) The centenary of the *Journal of Experimental and theoretical Physics* and the role of journals in the development of science. *Vest. Akad. Nauk SSSR* no. 7, p. 13 (R); *Usp. fiz. Nauk* **111**, 535 (R); *Soviet Phys. Usp.* **16**, 928 (1974); *Fiziko-matematichesko spisanie* **19**, 102 (1976) (Bulgarian); *ETP* p. 173.

(G72) 1974 Modern developments in studies of magnetism. *Priroda*, no. 2, p. 50 (R); In *Proc. Int. Magnetism Conf. MKM73* **1**, 1. Moscow: Nauka (R); *ETP* p. 60.

(G73) Professor and student. *Khim. Zhizn'* no. 7, p. 21 (R); *ETP* p. 215.

(G74) Oxygen (lecture of 25 May 1944). *Nauka Zhizn'* no. 19, p. 25; *ETP* p. 35.

(G75) 1975 Reading M. M. Prishvin's diary. *Sever* no. 6, p. 69 (R); In *Prishvin and present times*, p. 147 (1978). Moscow: Sovremennik (R).

(G76) 1976 Our home, the planet Earth. In *Society and environment*. Moscow: Progress (R); English edition 1977, p. 9. [Variant of G69.]

(G77) Energy and physics. *Vest. Akad. Nauk SSSR* no. 1, p. 34 (R); *Priroda* no. 2, p. 70 (R); *Usp. fiz. Nauk* **118**, 307 (R); *Soviet Phys. Usp.* **19**, 169; abridged version in *New Scient.* **72**, 10 and 83; *ETP* p. 75.

(G78) 1977 Scientific and social approaches for the solution of global problems (Bernal Lecture). *Proc. R. Soc. Lond.* A **357**, 1; *Vop. filos.* no. 1, p. 46 (R); *ETP* p. 387.

(G79) Global problems and energy. *Usp. fiz. Nauk* **122**, 327 (R); *Soviet Phys. Usp.* **20**, 547; *ETP* p. 372.

(G80) The training of scientific and technical personnel at the Moscow Physico-technical Institute. *Vest. Akad. Nauk SSSR* no. 8, p. 8 (R).

(G81) 1979 The impact of modern scientific ideas on society. *Vop. filos.* no. 1, p. 61 (R); *ETP* p. 403.

(G82) 1980 Albert Einstein. *Vop. Filos.* no. 6, p. 29 (R); *Vest. Akad. Nauk SSSR* no. 7, p. 37 (R); *Sov. Stud. Phil.* no. 3, p. 3.

(G83) Quarter of a century of friendship and collaboration (talk on Moscow Radio). In *Collection in honour of Pavel Savich*, p. 21. Belgrade (Serbo-Croat).

(G84) 1983 Global problems. *Proc. 2nd Int. Seminar on Nuclear war, Erice, Sicily 1982* Servizio Documentazioni dei Laboratori Nazionali di Frascati dell'INFN, July, p. 183 (also to appear in (B2), vol. 4).

(G85) Shakespeare and Bacon. *Izobretatel' i Ratsionalisator* no. 2, p. 14 (R).

(G86) 1985 Twenty two reports on liquid air and oxygen (1939–41) (ed. P. E. Rubinin). *Khimiya Zhizn'* no. 3, pp. 15, 64; no. 4, p. 63; no. 5 to appear (R).

## *Books and pamphlets**

(B1) 1962 *High power electronics* vol. 1. Moscow: Academy of Sciences USSR (R) Translated as *High-Power Microwave Electronics* Oxford: Pergamon Press. (1964).†

(B2) 1964 *Collected papers of P. L. Kapitza* (ed. D. ter Haar), vol. 1, 1964; vol. 2, 1965; vol. 3, 1967; vol. 4, 1985. Oxford: Pergamon Press.

(B3) 1965 *Life devoted to science (Lomonosov, Franklin, Rutherford, Langevin)*. Moscow: Znanie. (R).

(B4) 1966 *Theory, experiment, practice*. Moscow: Znanie. (R).

(B5) *Physics problems*. Moscow: Znanie (R); some solutions in *Nauka Zhizn'* nos. 2–6 (1967) (R); expanded edition (R) 1972 (R); *Le livre du problème de physique*. Paris: Édition CEDIC. 1977 (French).

(B6) 1971 *The future of science*. Rome: Editori Riuniti (Italian).

(B7) 1973 *Rutherford—scholar and teacher*. (ed.) Moscow: Nauka (R).

(B8) 1974 *Experiment, theory, practice*. Moscow: Nauka (R); 2nd edition, 1977; 3rd edition 1981; Sofia: Nauki i Iskustvo 1977 (Bulgarian); Rome: Editori Riuniti 1979 (Italian); Novi Sad: Radivoj Cirpanov 1980 (Serbo-Croat); Bucharest: Editura Politika 1981 (Rumanian); Budapest: Gondolat 1982 (Hungarian); Prague: Mlada Fronta 1982 (Czech); Berlin: Akademie Verlag 1982 (German); English translation (ed. R. S. Cohen) with additions: Dordrecht, Boston and London: D. Reidel Publ. Co. 1980.

(B9) *Science, mankind, organization*. Tokyo: Misuzi Shobo (Japanese).

## *Patents*‡

| | | | |
|---|---|---|---|
| (P1) | 1925 | 226857 | (With H. F. Heath) Improvements in electric storage apparatus. |
| (P2) | | 226858 | (With H. F. Heath) Improvements relating to the production of high tension discharges. |
| (P3) | | 227149 | Improvements relating to flash illumination. |
| (P4) | 1926 | 254349 | (With M. Kostenko) Electric impulse generator. |
| (P5) | | 259272 | Improvements in electric current breakers. |
| (P6) | 1929 | 308329 | (With G. V. Lomonossoff) Improvements in brakes. |
| (P7) | | 310093 | (With G. V. Lomonossoff) Improvements relating to brakes for vehicles. |
| (P8) | | 312259 | (With G. V. Lomonossoff) Improvements in electromagnets. |
| (P9) | | 318474 | (With G. V. Lomonossoff) Improvements relating to clutches and variable-speed gearing for power transmission apparatus. |
| (P10) | 1935 | 433860 | Improvements relating to the production of low temperatures. |
| (P11) | 1939 | 511337 | Coupling means including a stabilizing device for securing high speed rotors to shafts. |
| (P12) | 1940 | 520762 | Improved device for stabilizing high speed turbine and like rotors. |
| (P13) | 1941 | 540555 | Improvements in expansion turbines for low temperature plants. |
| (P14) | 1948 | 610572 | Twin expansion engine for production of low temperatures. |
| (P15) | | 614006 | A method and means of producing liquid oxygen or liquid air rich in oxygen. |
| (P16) | 1949 | 615464 | Means for damping transverse oscillations at high shaft speeds. |
| (P17) | | 622469 | Means for rectification. |
| (P18) | | 625107 | Method and means for rectification and distillation of liquids with low temperature boiling points. |
| (P19) | 1971 | 1256686 | Improvements in and relating to thermo-nuclear plasma producing arrangements. |

* The books B3, B4, B6, B8 and B9 are mostly different selections of the popular, semi-popular and miscellaneous general articles listed above; the most extensive of these is B8.

† Subsequent volumes of *HPE* (**2**, 1963; **3**, 1964; **4**, 1965; **5**, 1968, **6**, 1969) were edited by P. L. Kapitza and L. A. Vainshtein. Only volumes 1 and 2 were translated into English (*HPME*).

‡ Only the British patents are listed (the year is that of final acceptance, often several years later than the date of application). Most of the inventions are also patented in the U.S.A. and some in France, Germany and the U.S.S.R. No foreign patents have been found for inventions other than those in the British list.

## *References to other authors of scientific papers*

Alikaev, V. V. *et al.* 1985. In *Nuclear fusion: supplement on plasma physics and controlled fusion research*, vol. 1, p. 419.
Allen, J. F., Peierls, R. & Uddin, M. Z. 1937 *Nature, Lond.* **140**, 62.
Allen, J. F. & Misener, A. D. 1938 *Nature, Lond.* **141**, 75.
Andronikashvili, E. L. 1946 *J. Phys. USSR* **10**, 201.
Boys, C. V. 1923 *Dictionary of Applied Physics*, vol. 3, p. 720. London: Macmillan.
Cockcroft, J. D. 1928 *Phil. Trans. R. Soc. Lond.* A **227**, 317.
Collins, S. C. 1947 *Rev. Sci. Instrum.* **18**, 157.
Cotti, P. 1960 *Z. angew. Math. Phys.* **11**, 17.
Dowson, D. 1979 *History of tribology*. London: Longman.
Gerlach, W. & Stern, O. 1921 *Z. Phys.* **8**, 110.
Gerlach, W. & Stern, O. 1922 *Z. Phys.* **9**, 347 and 353.
Harvey, A. F. 1963 *Microwave engineering*. London and New York: Academic Press.
Keesom, W. H. & Keesom, A. P. 1936 *Physica* **3**, 359.
Johansson, T. 1933 *Z. Phys.* **82**, 507.
Lifshitz, E. M. 1944 *J. Phys. USSR* **8**, 110.
Landau, L. D. 1941 *J. Phys. USSR* **5**, 71.
Lindemann, F. A. & Keeley, T. C. 1933 *Nature, Lond.* **131**, 191.
Moon, P. B. 1978 *Proc. R. Soc. Lond.* A **360**, 303.
Okress, E. (ed) 1961 *Crossed field microwave devices*. London and New York: Academic Press.
Peshkov, V. P. 1944 *J. Phys. USSR* **8**, 381.
Pippard, A. B. 1979 *Phil. Trans. R. Soc. Lond.* A **291**, 569.
Stern, O. 1921 *Z. Phys.* **7**, 249.
Tanabe, Y. 1949 *Sci. Rep. Res. Inst. Tohoku Univ.* A **1**, 267.
Wall, T. F. 1924 *Nature, Lond.* **113**, 568.
Wall, T. F. 1926 *J.I.E.E.* **64**, 745.

## *Books and articles about Kapitza**

Andrade, E. N. daC. 1974 *Rutherford and the nature of the atom*. New York: Doubleday.
Andronikashvili, E. 1980 *Recollections of liquid helium*. Tbilisi: Ganat Leba. (R)
Badash, L. 1985 *Kapitza, Rutherford and the Kremlin*. New Haven: Yale University Press.
Biew, A. M.† 1956 *Kapitza*. London: Frederic Muller.
Brown, H. 1968 Peter Kapitsa on life and science. *New York Times Book Review* 22 December.
Calder, R. 1951 *Profiles of science*. London: Allen & Unwin.
Danin, D. 1966 *Rutherford*. Moscow: Molodaya Gvardiya. (R)
Dirac, P. A. M. 1980 Dirac recalls Kapitza. *Physics Today* May, p. 17.
Dobrovolskii, E. N.† 1968 *A sketch of Kapitza*. Moscow: Sovetskaya Rossiya. (R)
Eve, A. S. 1939 *Rutherford*. Cambridge: Cambridge University Press.
Fen, E. 1976 *A Russian's England*. Warwick: Paul Gordon Books.
Gamov, G. 1970 *My world line*. New York: Viking Press.
Hartcup, G. & Allibone, T. E. 1984 *Cockcroft and the atom*. Bristol: Adam Hilger.
Hendry, J. (ed) 1984 *Cambridge physics of the thirties*. Bristol: Adam Hilger.
Jungk, R. 1958 *Brighter than a thousand stars*. London: Gollancz.
Kedrov, F. B.† 1979, 1984 *Kapitza, life and discoveries* 1st and 2nd editions. Moscow: Moskovskii rabochii. (R). English translation 1984 Moscow: Mir.
Khariton, Yu. B. 1984 He simply couldn't live without creating. *Priroda* no. 6, p. 28 (R).
Khrushchev, N. 1974 *Khrushchev remembers—the last testament*. London: Andre Deutsch.
Kresin, V. 1985 A unique figure in Soviet science. *Obozrenie* (Paris) no. 14, p. 29.
Krylov, A. N. 1984 *My recollections*. (8th edition) Leningrad. Sudostroenie: (R).
Landau, L. D. 1964 Born to be bold. *Komsomolskaya Pravda* 8 July (R).
Larsen, E. 1962 *The Cavendish Laboratory*. London: Ward.
Medvedev, Zh. A. 1979 *Soviet science*. Oxford: Oxford University Press.

* Those marked † are full length biographies, though Biew (1956) is almost pure science fiction. The other books and articles listed contain material, not always reliable, and sometimes no more than a mention, about Kapitza, but the list is only a selection.

Oliphant, M. L. 1972 *Rutherford—Recollections of the Cambridge days*. Amsterdam: Elsevier.
Parry, A. 1968 *Peter Kapitza on life and science*. New York: Macmillan.
Pochivalov, L. 1984 P. L. Kapitza: a memoir by his friend. *Literaturnaya Gazeta* 16 May (R); English translation in *Physics Can*. **40**, 101.
Rubinin, P. E. 1979 Kapitza's Nobel week. *Priroda* no. 6, p. 122 (R).
Rubinin, P. E. 1984 In the director's office. *Priroda* no. 6, p. 34 (R).
Rubinin, P. E. 1985 Extracts of letters from P. L. Kapitza to his mother. *Priroda* no. 1, p. 56, and in the press (R).
Scammell, M. 1985 *Solzhenitsyn*. London: Hutchinson.
Shalnikov, A. 1975 At the seminar. *Literaturnaya Gazeta* 15 October (R) (partially reproduced in Kedrov 1984).
Shoenberg, D. 1954 Mr E. Laurmann (obituary). *Nature, Lond*. **174**, 1129.
Shoenberg, D. 1978 Forty odd years in the cold. *Physics Bull*. **29**, 16.
Snow, C. P. 1967 *Variety of men*. London: Macmillan.
Snow, C. P. 1981 *The physicists*. London: Macmillan.
Sominskii, M. 1965 *A. F. Joffé*. Moscow: Nauka (R).
Sominskii, M. 1984 The Kapitza saga. *Krug* (Israel), no. 388, p. 38 and no. 389, p. 36 (R).
Spruch, G. M. 1979 Pyotr Kapitza, octogenerian dissident. *Physics Today* September, p. 34.
Timoshenko, S. P. 1968 *As I remember*. Princeton, New Jersey: D. van Nostrand.
Wilson, D. 1983 *Rutherford, simple genius*. London: Hodder & Stoughton.
York, H. F. 1976 *The advisors*. San Francisco: W. H. Freeman.