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Kapitza Centenary Symposium at the Cavendish Laboratory (Cambridge, 8 July 1994)

The centenary of Peter Kapitza's birth was celebrated in Cambridge on 8 July 1994 by a symposium in the Cavendish Laboratory in which his contributions to low-temperature physics and technology were reviewed, with emphasis on the developments which have grown out of his pioneering contributions. In the evening, Trinity College hosted a dinner in Hall and Sergei Kapitza replied to a toast by the Master (Professor Sir Michael Atiyah) to Kapitza's memory. A shortened version of Sergei Kaptiza's speech is appended below.

(1) **D Shoenberg** (Cavendish Laboratory, Cambridge) "Kapitza in Cambridge and Moscow"

(2) **A B Pippard** (Cavendish Laboratory, Cambridge) "Magnetoresistance — the Kapitza legacy"

(3) **G G Lonzarich** (Cavendish Laboratory, Cambridge) "Experiments in high magnetic fields and at high pressures"

(4) **W F Castle** (BOC Group plc) "Kapitza and cryogenics"

(5) **W F Vinen** (School of Physics and Space Research, University of Birmingham, UK) "Liquid helium-4"

(6) **H E Hall** (Schuster Laboratory, University of Manchester, UK) "Flow and textures in superfluid ^{3}He-A"

(7) **A M Guenault** (University of Lancaster, UK) "Kapitza and Lancaster"

(8) **S P Kapitza** (Institute for Physical Problems, Moscow) Speech at Trinity College dinner for the Kapitza Centenary

Summaries of papers presented at the symposium

PACS numbers: 01.60. + q

Kapitza in Cambridge and Moscow

D Shoenberg

Kapitza in Cambridge

Peter Kapitza's connection with Cambridge came about rather accidentally. He had suffered a great tragedy with the loss of most of is close family in the epidemics which were raging in Petrograd in the aftermath of the Revolution and the Civil War. He himself only barely recovered from a long bout of nephritis and he was completely devastated by the death of his family. For a long time he was unable to work, but luckily something then turned up which was to change the whole course of his life. A commission had been set up under Abram Ioffe for renewing scientific relations with the outside world and Ioffe was able to appoint Kapitza to accompany the commission on an extended trip to the West in 1921. Ioffe wanted Kapitza to spend some time in the famous physics laboratory at Leiden, but the Dutch were frightened of possible communist infection.

Fortunately England proved more accommodating and eventually Kapitza arrived in May 1921. Ioffe introduced him to Rutherford at the Cavendish Laboratory in Cambridge, then a kind of Mecca for physicists from all over the world, and Kapitza asked Rutherford if he would accept him as his student over the winter. At this point Rutherford became distinctly less friendly and said his laboratory was already full. Kapitza responded with a seemingly quite irrelevant question — what sort of accuracy did Rutherford aim at in his experiments? Rutherford was rather nonplussed, but told Kapitza that usually something like 3% error was permissible. Ah, said Kapitza — you have about 30 students, so you wouldn't notice one more if you took me, since I would be just about within your permissible error. Rutherford was impressed by the ingenuity and the sheer cheek of Kapitza's approach and agreed to take him.

Although at first Kapitza was greatly in awe of Rutherford — indeed he nicknamed him the Crocodile — he managed to establish a very cordial relation. Rutherford was impressed not only by Kapitza's skill and originality, but also by Kapitza's ability to talk to him man to man in a way that very few of his other younger colleagues dared to do; he probably also enjoyed Kapitza's undisguised admiration. Kapitza's first research was a study of how the energy of an α-particle falls off towards the end of its range and he not only devised an original and ingenious technique, but completed the experiment and published it in record time. Kapitza tried a mild leg-pull in presenting Rutherford with an inscribed reprint. When he had started in the Cavendish, Kapitza was rather annoyed to be warned by Rutherford that he would not tolerate any communist propaganda, so he wrote on the cover: "The auther presenting this paper with hise most kind regards, would be very hapy if this work will convince Prof. E Rutherford in two things. (1) That the α-particle hase no energy after the end of his range. (2) That the auther came to the Cavendish Laboratory for scientifical work and not for communistical propaganda." (Kapitza's curious spelling and English style have been left uncorrected.) Rutherford was angered by the cheek of the inscription and threw it back at Kapitza, who, however, had come prepared with a second reprint with a more conventional inscription, so that peace was soon restored.

To follow up this first research Kapitza developed an ingenious new method for producing powerful magnetic fields which could be used to bend the α-particle tracks. The

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novelty was to overcome the difficulty of Joule heating in the magnet coil by creating the field only momentarily and synchronously photographing the tracks in a Wilson cloud chamber. Fields of about 10 T were achieved by discharging a specially designed lead battery through the coil, and eventually measurements of the curvature of the tracks convincingly confirmed and extended the results of the earlier experiments. In the course of the work Kapitza realised that the powerful though short-lived magnetic fields might be exploited to open up completely new areas of physics. His first venture outside nuclear physics was a study of the Zeeman effect. This confirmed that the general idea of using impulsive fields was sound, but even though his fields were several times larger than had hitherto been used, the results did not show any appreciable deviations from what could be expected by extrapolation from lower fields.

To go further, Kapitza developed an entirely new technique with which he was able to achieve still higher impulsive fields. The idea was to use the kinetic energy of the rotor of a large dynamo and discharge most of this energy into a coil. Designing the dynamo, the elaborate switching arrangements and a specially strengthened magnet coil was a major engineering project which Kapitza carried through in collaboration with industry. It says much for his drive and enthusiasm that it took only little more than 2 years from the conception of the new idea to the successful testing of the whole equipment, and fields as high as 35 T were achieved. During the next few years a series of novel researches was carried out, predominantly on the increase of electrical resistance of metals in high magnetic fields but also on magnetic susceptibility and magnetostriction; the Zeeman effect was left on one side, though it was taken up again in Moscow some 10 years later. It soon became evident that the results on metals became more interesting as the temperature was lowered, though at that time liquid nitrogen was the only cooling agent available in Cambridge, so Kapitza turned his attention to the liquefacation of hydrogen and helium.

By the early 1930s Rutherford, who continued to be impressed by Kapitza's imaginative style, and was then very much at the head of the English scientific establishment, persuaded the Royal society to use part of a bequest left by the industrial chemist Ludwig Mond for building a special laboratory for Kapitza's work, where he could more comfortably extend his magnetic work to lower temperatures. This Royal Society Mond Laboratory, as it was called, was opened in 1933 with great eclat by Stanley Baldwin, who had been Prime Minister and was then Chancellor of the University. The opening was performed with a key in the shape of a crocodile and a fine bas-relief of a crocodile carved by Eric Gill was revealed just to the side of the front door; a bas-relief of Rutherford, also by Eric Gill, was carved just inside the building, to emphasise, as it were, who the crocodile really was.

Let me go back for a moment to the significance of the nickname.

I think Kapitza rather enjoyed being a bit mysterious about it. He once told the scientific writer Ritchie Calder that "In Russia the crocodile is the symbol of 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 entirely apocryphal explanation popular in the Cavendish, was that it was after the crocodile in "Peter Pan" which swallowed an alarm clock to give warning of its terrifying approach. Like Rutherford (who had a booming voice and a heavy tread) it could be heard before it was seen. Yet another explanation is that the nickname was based on Kornei Chukovskii's comic poem for children about a crocodile walking the streets of Petrograd. However, Anna Alekseevna quite recently insisted on debunking all these explanations — it was simply that at first Kapitza was quite terrified by Rutherford and the crocodile was the most terrifying beast he could think of. At the tea which followed the opening, there was an amusing incident. Kapitza was sitting with Baldwin and was overheard telling him something rather implausible. When Baldwin said something like "surely that can't be true", Kapitza grinned and replied "You can certainly believe me — I'm not a politician!"

Even before the Mond Laboratory was ready, Kapitza in collaboration with Cockcroft had built a hydrogen liquefier of novel design and his first big effort in the new laboratory was the completion of a helium liquefier. As always, he disliked following a trodden path, and developed a completely original method of cooling by adiabatic expansion, using the gas itself as a lubricant in the narrow gap between piston and cylinder. He had hoped that he would be the first to liquefy helium in England, but he was upstaged by Lindemann in Oxford, who brought Mendelssohn over from Germany in 1933 with one of Simons miniature liquefiers at the same time as the Mond Laboratory was opened. However, Kapitza's machine had the great advantage that the liquid helium could be transferred into a Dewar vessel, so when it first worked successfully in April 1934, he could claim that he had made the first liquid helium in England which could be looked at (the Oxford liquid helium was hidden in an all-metal vessel). His liquefier established Cambridge as one of the few cryogenic centres in the world, but even more significantly, it was the forerunner of the machine developed by S C Collins in the USA for factory production. The commercial availability of this Collins machine revolutionised low-temperature physics by making liquid helium easily available all over the world.

The new laboratory became a kind of Mecca for physicists visiting Cambridge and Kapitza loved showing off the points of interest in his characteristic style. To illustrate the novel feature of the very short-lived high magnetic field he liked to say "one hundredth of a second may seem a short time to you, but its quite long enough if you know what to do with it". His other little joke was to boast that he was the highest paid physicist in the world since he received a professor's pay for only a few seconds work in a year. Every time the big dynamo was short circuited though the magnet coil something like 20% of the kinetic energy of the rotor was suddenly lost and this produced a miniature earthquake. To avoid disturbing the delicate recording instruments, the coil and instruments were mounted about 20 metres away from the dynamo so that the seismic wave through the ground reached them only after the experiment was over. He liked to explain how this determined the elegant design of the new laboratory, centred on the 20-metre-long magnet hall with research rooms opening out from it. This was indeed an attractive feature and even after the dynamo had gone, the magnet hall proved a valuable sociological feature in providing an informal meeting place and lessening the isolation of the research rooms. Another feature Kapitza liked to show off (especially to nervous visitors) was the liquefier room with its very light roof, designed to blow off if there was a hydrogen explosion — fortunately this never happened!

Everything seemed set fair for exciting new results to start coming out of the combination of high fields and low temperatures in the new laboratory, but Kapitza's Cambridge period came to an abrupt end in 1934. Quite unexpectedly he was not allowed to return to England after a routine visit to Russia. Eventually a new Institute, the Institute for Physical Problems, was built for him in Moscow, and much of his special equipment was transferred to the Institute from Cambridge. The story of how all this came about is complicated and goes beyond the scope of this article, as does Kapitza's subsequent career in Moscow. Suffice it to say that although some of the themes of the Mond Laboratory were taken up and successfully developed by Kapitza's younger colleagues in Moscow, he himself turned his attention to other problems. The most spectacularly successful was his study of liquid helium, which — he demonstrated — became a superfluid below the lambda point transition. For this work he was many years later awarded a Nobel Prize. He also continued his interest in gas liquefaction to develop a turbine method of liquefying air on an industrial scale. This became important during the war in providing cheap oxygen for the steel industry.

During Kapitza's 13 years in Cambridge he became somewhat of a legend, both for what he achieved and for his eccentricities. Following his early scientific successes, he had moved rapidly up the academic ladder. He was elected a Fellow of Trinity College in 1925 and a Fellow of the Royal Society in 1929 — a rare distinction for a foreigner, especially for one who became a Corresposponding Member of the Soviet Academy of Sciences in the same year; in 1930 he was also appointed a Royal Society Professor. In many ways he was almost the prototype of the absent-minded professor who would forget such mundane matters as what he should be wearing, or that there were guests for dinner at home. When his mind was occupied with his work it was very difficult to get him to answer a question. He would reply ''What you say?'' and if you repeated the question he would repeat his "what you say?" until eventually he either walked off or you gave up. But when he was in the mood he could be a wonderful conversationalist — never lost for something interesting to say on any subject and with a great sense of fun and warm cordiality. He loved an argument, and was an excellent raconteur with an enormous repertoire of stories and anecdotes. Here is one example in which he makes fun of theoreticians: Two theoreticians were stirring their tea when one said to the other "I wonder what makes the tea taste sweet. Is it the sugar or the stirring?'' They argued about it for a long time but couldn't find a convincing answer, so they decided to consult Landau. He thought a little and said he thought he could see the answer, but there was one difficulty that needed further thought and he asked them to come back the next day. When they saw him again he said "Now everything is clear. It is obviously the stirring that makes the tea sweet. What held me up at first was that I couldn't see the reason for putting sugar in, but now I have realised that if you didn't put the sugar in, the tea wouldn't need stirring.'' 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 jokes was so infectious that those around him found themselves joining in, even if they had not altogether understood the joke.

He had quite a reputation for his love of cars and his reckless driving. When a nervous passenger would draw his attention to the high reading on the speedometer, which of course was in miles per hour, Kapitza would reassure him that his was a special speedometer which read in km per hour. One of the best stories is of his giving a ride to his clergyman friend F A Simpson (a historian at Trinity College). When they were coming to a dangerous corner Kapitza turned round to Simpson who was sitting behind him and said ''Pray God Simpson, Pray God.'' It seems the prayer was effective! He was indeed rather fond of teasing clergymen. Another occasion was when a clergyman was a guest at dinner in Trinity College and asked who was the distinguished-looking man sitting a little further down the table. It was in fact the famous astronomer Eddington and Kapitza explained: ''He knows far more about the heavens than you do.''

Kapitza left his mark in Cambridge in several ways. He was one of the first to start the transformation of the Cavendish from sealing wax and string into the machine age. He was the originator of solid-state and low-temperature physics in Cambridge and his emphasis on the importance of high-purity samples and single crystals was a valuable legacy to those who followed in his footsteps. And last, but not least, he started the tradition of a lively informal seminar, the Kapitza Club as it came to be called, which injected something of the Russian temperament into the more phlegmatic English. In Cambridge, his work has been continued by many generations of low-temperature and solid-state physicists in his original Mond Laboratory until 1972 and more recently, when the Cavendish Laboratory moved away from the centre of Cambridge, in the Low Temperature Group of the Cavendish.

Kapitza in Moscow

Kapitza's Cambridge period ended in the summer of 1934 when, during a visit to the Soviet Union to see his mother and to attend a scientific conference, he was suddenly told he would not be allowed back to Cambridge. The reasons for his detention and his negotiations with Soviet high-ups were too complicated; probably one factor was that he had sometimes been rather boastful of his successes in England and gave the impression that his work could be of immense technological importance if only he were given the right support. The authorities, possibly Stalin himself, took him at his word and told him he must in future work for them — although in fact none of his work was secret and it was available to everyone. For a while he sulked, protesting that he would abandon physics for biology if forced to stay, but in those days it was dangerous to sulk against a decision from on high and eventually Kapitza agreed to become Director of a prestigious new Institute for Physical Problems, to which much of his Cambridge equipment was transferred.

The Institute was ready for work by the end of 1936 and though very small compared with the typical huge Soviet Institutes, it was run by Kapitza in a very imaginative and effective way and soon established an international reputation. It had attractive living quarters for its staff and quite a palatial home for the Director, paradoxically known as the 'Cottage', which the Russians thought was the appropriate English for any detached house. He managed to avoid a good deal of the red tape and elaborate planning procedures usual in Soviet institutes and was fond of saying that the customary detailed planning was like a doctor prescribing medicine for an illness his patient would have in a year's time.

The Institute rapidly proved its worth and within a year or so Kapitza had broken completely new ground in discovering the striking property of superfluidity in liquid helium. For this discovery and his other contributions to low-temperature physics he was eventually — but only 40 years later — awarded a Nobel Prize. He did not himself continue his Cambridge work on magnetism and metals though it was vigorously taken up by some of his young research associates in Moscow whose work also enhanced the Institutes reputation. Another line which Kapitza himself developed very successfully was more technological in nature — a new and more efficient method of liquefying air in order to produce cheap oxygen for industrial processes, particularly in the manufacture of steel.

During the war this oxygen work became his chief preoccupation and it was recognised by many orders and decorations in the middle 1940s. But another catastrophe was at hand! At that time Beria, the notorious head of the KGB, was also effectively in charge of high-level technological developments, which included both Kapitza's oxygen work and the urgent development of an atomic bomb. Kapitza was a member of the special committee on the atom bomb and found Beria's style of directing the project utterly distasteful and inappropriate. Eventually he complained to Stalin that "Beria was like the conductor of an orchestra who has the baton in his hand but has lost the score" and resigned from the committee. Beria was furious and got back at him by obstructing his oxygen work. No doubt Kapitza had trodden on many establishment toes in pushing through his new methods and it was not difficult for Beria to fill crucial committees with Kapitza's opponents and get them to claim that Kapitza's oxygen work was worthless.

It is amazing that Beria did not succeed in simply liquidating Kapitza, but apparently Stalin, who loved playing cat and mouse games, told Beria that he could dismiss Kapitza but he mustn't touch him. Kapitza wrote frequent letters to Stalin ever since his detention in the Soviet Union in which he boldly criticised the way science and education were administered and even more boldly intervened on behalf of scientists who had been unjustly repressed during the purges. The best known intervention was on behalf of Landau, Kapitza's 'house' theoretician, whom Kapitza managed to pull out of Beria's clutches after he had spent a year in prison. It seems that Stalin — a bit like Rutherford — liked Kapitza's cheek and boldness and perhaps regarded him as a kind of court jester. He replied to Kapitza's many letters only twice, but there is evidence that he enjoyed getting them, and it probably amused him to taunt Beria with Kapitza's criticisms.

In the summer of 1946 the blow fell. Kapitza was dismissed not only from his oxygen work but also from the Directorship of his Institute. He still had his salary as an Academician and was allowed to live peacefully in his dacha at Nikolina Gora. He was at first completely bowled over by this new catastrophe, but soon rallied. He once wrote to Stalin while still in disgrace, adapting a remark of Tolstoy, that he was "Not a scientist who does scientific work, but one who is unable not to do scientific work". With the help of his teenage sons (who are now distinguished scientists themselves and whom we are glad to see here today) he set up a miniature workshop and laboratory in the outbuildings of the dacha. Inevitably it got called the 'Izba Fizicheskikh Problem'. There he again succeeded in opening up a new line of work — the development of new methods of producing high-power microwaves. The new machine was christened a 'nigotron' (after Nikolina Gora). He was able to persuade the government that this work had immense military potential — in some sort of star-war scenario — and he gradually got much improved technical support.

After Stalin's death and Beria's fall and execution, Kapitza was reinstated in his old Institute and rapidly expanded it to exploit his nigotron in a new direction. This was the idea of using the high-power microwaves to heat a plasma to such high temperatures that nuclear fusion would occur. This project occupied him for the rest of his long life. Although he continued to believe it could be an economical way of achieving nuclear fusion — the cheap power of the future — he could not compete with the big battalions who were, and still are, developing more elaborate schemes, involving huge machines and huge teams of scientists. Kapitza's work provided important contributions to plasma physics, but the expert opinion is that his idea was not likely to lead to the Holy Grail of cheap power through nuclear fusion. However, this Holy Grail has not yet been reached by the big battalions either, though it seems they may not be far off.

On the occasion of his centenary we gratefully salute his memory.

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Magnetoresistance — the Kapitza legacy

A B Pippard

Kapitza's studies of magnetoresistance in metals, described in two long papers, came late in his work on high magnetic fields and tackled a problem that had hardly been investigated before, largely because no one previously possessed the appropriate combination of strong fields and low temperatures. Kapitza, indeed, was severely hampered by not being able to cool his samples below liquid air temperature, so that only bismuth, of all the available metals, showed a sizeable magnetoresistance — an increase by a factor of about 50 at 300 kG (30 T). This was in 1928, two years before the discovery of the Schubnikov – de Haas oscillations, but these are so weak at liquid air temperatures that Kapitza could hardly observed them.

The observed variation of resistance in bismuth seemed to kapitza to begin as H^2 and then settle into a linear rise with increasing H. All his subsequent interpretations of the effect in other metals were conditioned by this discovery, so that the linear form of magnetoresistance came to be known as Kapitza's law. It has not stood the test of time, but there are enough examples of a linear magnetoresistance to justify this as the central feature of the present discussion.

First, we must ask if Kapitza deluded himself about the behaviour of bismuth. Certainly his published curves look convincing enough, and there all too few later measurements to serve as a test. The simplest theoretical expectation is that the resistivity will increase quadratically with H, but this is not confirmed. Gerritsen et al., at liquid hydrogen temperatures, found nonlinear behaviour which was certainly not as strong as quadratic, though one sample at 14 K followed Kapitza's law very well. Later, Alers and Webber worked at liquid helium temperatures where the resistance shows Schubnikov – de Haas oscillations superposed on a generally linear variation. There is therefore no reason to cast doubt on Kapitza's measurements, though it must be admitted that there in no adequate theory of the behaviour.

When we turn to other materials we see that Kapitza measured magnetoresistance in a transverse field for polycrystalline wires of every metal he could find and handle. At liquid air temperatures the effect was weak, for example a 45% increase at 300 kG for copper in liquid nitrogen. It must be remembered that a free-electron metal should show no change of resistance in a magnetic field, and that the effect in copper is attributed to contacts of the Fermi surface with the zone boundary (this is an idea that could not have been entertained by Kapitza, working at a time when Bloch's quantum theory of electrons in metals was new and appreciated by very few). It is of interest that the quadratic law in copper gives way to linear when $\omega_c\tau$ is still only about 1/3 — the field of 100 kG is only able to push an electron about 1/20 of the distance around the Fermi surface between collisions. This is enough to suggest that there are rather sharp changes of direction in at least some parts of the Fermi surface; indeed electrons which are pushed through a neck contact suffer rapid changes of direction to which the magnetoresistance can be attributed. A great many metals have rather sharp corners to their Fermi surfaces, so that it is not surprising that the low-field quadratic behaviour breaks down while $\omega_c\tau$ is still small.

One is, however, surprised at how readily Kapitza could fit his data to his (quadratic + linear) model. To some extent this is because the range of variation was so limited — and one must compliment him on being able to get reliable results in an impulsive field which could all too easily induce overwhelming voltages in his leads. But later workers, using lower temperatures and consequently much larger $\omega_c\tau$, have not usually confirmed his interpretation. The high-field behaviour follows the theory of Lifshitz et al. fairly well: the resistivity should either increase quadratically or saturate. The conditions for either type of behaviour are somewhat complicated, and will not concern us. What is of concern is that this theory finds no place for linear variation of resistivity, such as is commonly observed. Let us note a few cases:

(1) Aluminium. The resistivity makes a determined effort to saturate, but there remains in high fields a slow drift upwards, which is more marked at 20 K than at 4 K.

(2) Copper (and probably gold). Polycrystalline wires, after a quadratic region, settle down to a regime in which resistivity is almost proportional to field strength.

(3) Potassium. This is an almost ideal metal, one would guess, with nearly spherical Fermi surface, which should have no magnetoresistance, but in fact it shows a slow linear increase which is irreproducible from one sample to another. Later measurements, by more sophisticated means, throw the whole matter into confusion.

Let us discuss these three cases in a little detail (the quoted reference goes into more depth). A general point is worth making as a start. Gas bubbles or other voids deflect the current flow more strongly in high fields, and can lead to a linear increase in resistivity. But it is unlikely that most samples have enough voids to account for the observations.

(1) The stronger linear form in aluminium at 20 K, compared to that at 4 K, suggests that phonon scattering plays a part. Since 20 K is much less than the Debye temperature, most phonon scattering will be through a small angle, and many scatterings are needed to take an electron round a spherical Fermi surface and destroy the memory of its direction of motion. But if there are regions of the Fermi surface in which the electron crosses a zone boundary between different types of orbit, scattering in and out of these regions is very effective in changing direction. Calculation show that magnetoresistance from this cause begins early and persists until the field is very strong, increasing roughly linearly in between.

(2) Single crystals of copper may show nonsaturating quadratic magnetoresistance for some direction of $\boldsymbol{H}$, and saturating resistance for others, and the change-over is frequent as the crystal orientation changes. Ziman observed that if one averaged the conductivity rather than the resistivity one might get a linear variation as a result, but it was Stachowiak who explained why such an average was reasonable. In a strong field the metal conducts extremely well parallel to $\boldsymbol{H}$, compared to its conduction perpendicular to $\boldsymbol{H}$. If, then, the electric field is constant, it does not matter that currents in neighbouring crystallites do not match; any discontinuity as currents flow from one crystallite to another leads to the unwanted current flowing away parallel to $\boldsymbol{H}$. He developed this argument more stringently, and inspired by this I made a detailed computation which did indeed show the resistance increasing almost proportionately to $\boldsymbol{H}$. This is one of the rare cases where Kapitza's law holds rather well.

(3) Matchstick samples of potassium are hard to handle, and Datars and Lass independently developed contactless measurements in which massive samples (preferably spheres) are turned about an axis normal to $\boldsymbol{H}$. The induced currents lead to a torque which can be measured and related to the conductivity. Lass found that the torque remained almost constant as a single-crystal sphere was rotated, while Holroyd and Datars obtained a very marked oscillatory torque, varying smoothly with angle. Overhauser, who had convinced himself that charge-density waves should exist in potassium, saw the latter experiments as confirmation, but the difference between this and Lass' smooth result was worrying. It seemed to some that potassium crystals protected by an oil layer showed the oscillations, while clean crystals did not, and it might be that the metal is encouraged to undergo a phase-change by the stresses resulting from differential contraction of oil and metal. This favoured Overhauser's explanation, but Wilson and de Podesta suggested that it was not a charge-density wave but a change of crystal symmetry that was involved. Overhauser's ingenious interpretations in support of his model were only partially convincing, and the question remains open.

Later measurements by Coulter and Datars greatly increased the oddities. At field strengths approaching 8 T the smooth oscillations of Holroyd and Datars became extremely violent with many almost random variations in single revolution. Overhauser's interpretation in terms of many domains with different orientations of the charge-density were challenged by Elliott et al., who noted that Coulter and Datars used a different arrangement from Holroyd and Datars. Instead of rotating the crystal on a vertical axis in a horizontal field, the use of stronger fields compelled them to have $\boldsymbol{H}$ vertical and to rotate the sample, mounted on pivots, about a horizontal axis. They were satisfied that the friction in the pivots was small, but Elliott et al. pointed out that the very powerful Hall effect in high fields created a couple that could not be measured in the experiment, but which tried to twist the sample off its bearings. This couple was as much as 100 times greater than the measured couple and they suggested that the resulting friction would lead to the observed erratic behaviour. Their paper has not, it seems, drawn any response from Overhauser or from Datars and his group, perhaps because the discovery of high-T_c. superconductivity has changed their principal interest. So the behaviour of potassium remains a problem for which there is no agreed solution. Personally I see no reason to change the view, unsympathetic to Overhauser, that I expressed before these latest ideas appeared.

In conclusion, one may say that the hare started by Kapitza has metamorphosed several times, but remains alive and running. The linear magnetoresistance is still only imperfectly understood.

Reference

A B Pippard *Magnetoresistance in Metals* (Cambridge: Cambridge University Press, 1989)

PACS numbers: 71.28.+d; 75.30.−m; 74.70.Tx

Experiments in high magnetic fields and at high pressures

G G Lonzarich

The work of Kapitza and his colleague Landau led to the clarification of the concept of elementary excitations, a notion which has played a central role in the description of condensed matter at low temperatures. The low-lying excited states of interacting fermions, for example, are thought to be in a one-to-one correspondence with quanta which can be described as fermi quasiparticles and holes created out of a 'vacuum' but with finite momenta on or near the Fermi surface. In a crucial difference from the earliest single-particle theories these quasiparticles are not entirely decoupled, but exhibit residual interactions whose existence appears to be essential for a consistent description. When this coupling is short range and repulsive it may be ineffective in producing transitions between states of different momentum on the Fermi surface near absolute zero. In this case, the quasiparticle excitations are long lived and have the well-defined energy–momentum relation of normal modes. In leading order at low temperature the entropy is therefore that of a Fermi gas, but, in contrast to a model of truly uncoupled fermions, the thermal energy is not simply the sum of the individual quasiparticle energies [1]. The loss of additivity, which may be viewed as arising from nondiffractive scattering processes (e.g. those involving possible exchange, but no other momentum transfer), is an essential feature of the theory which has provided a detailed description of the normal states of liquid ^{3}He and of electrons in metals.

The assumption that the scattering is nondiffractive can clearly break down when the residual interaction is attractive. In this case, quasiparticles may form bound states and condense in superconducting or magnetically ordered phases. These is, however, another and more subtle way in which the normal description can fail. The suppression of real scattering processes by the effect of the Pauli principle is based on the assumption that the interaction is both repulsive and nonsingular for transitions on the Fermi surface. The latter assumption in fact breaks down for the Lorentz magnetic force between moving charges which, in contrast to the direct Coulomb potential, remains long range even in a plasma. Similar but more important components of the residual interaction may also arise from the exchange, not of transverse photons as for the Lorentz force, but of almost critical fluctuations of an order parameter near a continuous phase transition, e.g. at the threshold of magnetic or superconducting states. Systems discussed briefly below in which singular interactions may be important include the heavy fermion compounds based on 4*f* and 5*f* elements, nearly magnetic d-transition metals, and the copper oxide superconductors.

The properties of the low-lying excitations of charged Fermi systems may be probed by a technique based on the Landau quantisation of orbital motion of charge carriers, pioneered by the inheritors of Kapitza's Mond Laboratory, D Shoenberg and A B Pippard [2, 3]. The oscillatory variation of magnetic and electronic properties in a magnetic field has provided invaluable information not only on the nature of simple metals in which the quasiparticles are virtually indistinguishable from bare electrons, but also in more extreme cases where the quasiparticles are subtle composite entities with masses more than two orders of magnitude greater than that of ordinary electrons and with residual interactions of novel forms.

The study of these oscillatory effects, associated with massive quasiparticles on the largest Fermi surface sheets, requires the preparation of complex materials with carrier mean free paths in the range 10^3 to 10^4 Å and the use of low-noise techniques, intense magnetic fields, and ultralow temperatures. A new experimental system set up to study these materials is based on a superconducting magnet assembly, set up in collaboration with Oxford Instruments Ltd with the support of the SERC, that generates a uniform field of up to 18.4 T which can be modulated at a rate of up to 1 T s^{-1} at the sample. The assembly also provides a field of up to 6.5 T above the main magnet for adiabatic demagnetisation of copper down to 1 mK, and an extended low field region for sensitive electronics and for the mixing chamber of a top-loading dilution refrigerator – demagnetisation stage [4]. The low ambient noise levels of the assembly permit the use of SQUIDs and other high-sensitivity low-temperature detection systems. The most versatile of these, developed by S R Julian, is based around home-made toroidal transformers and may be used to detect signals below 10^{-12} V under normal operating conditions with the sample at full field. A SQUID detector, set up by I R Walker for studies in high magnetic fields, increases this sensitivity to 10^{-13} V or 10^{-14} V. The fridge is also equipped with clamp cells for measurements at hydrostatic pressures of up to 25 kbar, and a bellows-driven diamond-anvil system, developed by R K Haselwimmer and S V Brown, which provides variable pressures to 150 kbar at low temperatures.

Measurements of the oscillatory resistivity and magnetic susceptibility at very low temperatures and in high magnetic fields have enabled us to build a detailed model of the normal heavy fermion state in uranium and cerium compounds such as UPt_3 and $CeRu_2Si_2$ [5]. The key finding, based on the observation of the large and dominant sheets of the quasiparticle Fermi surface, is that the elementary excitations can be classified as charged fermions whose properties provide a full account of the low-temperature heat capacity in the normal state. The masses of these particles are more than two orders of magnitude above that of free electrons and are the largest thus far observed directly in any system.

These results rule out a proposal that the excitations consist of a collection of both charged and neutral fermions. The nearly localised f moments on the uranium and cerium atoms, expected in this picture to generate the neutral quanta, do not, in fact, act as separate entities but cooperate in a subtle manner with the conduction electrons to give rise to a single class of composite charged excitations. The Fermi volume of these compound quasiparticles is that expected in a naive picture in which the virtually localised f moments are treated as fully itinerant. The f electrons (at least in the above systems in their normal states) are thus engaged in an intricate dual role in which they create both well-formed local moments and a Fermi surface in a coherent quantum state.

On the verge of an electronic instability at low temperature, a description in terms of well-defined fermion excitations (with temperature independent properties) may break down. An example of this phenomenon is found near the critical pressure p_c which divides the magnetically aligned from the nonmagnetic state of the cubic d-metal MnSi. As p_c is approached, the resistivity which varies as the square of the temperature in the conventional Fermi liquid regime (when electron–phonon scattering is unimportant), tends to a behaviour which is found to be consistent with that expected for a marginal Fermi liquid [6]. This novel state is characterised by a $T \ln(T^*/T)$ form rather than the conventional linear variation of the heat capacity at a low temperature T. In a quasiparticle language, the 'fermions' would be ascribed strongly temperature dependent properties and, for example, 'masses' which are unbounded and hence undefined at the Fermi level as $T \to 0$. This breakdown of the usual Landau model may be traced, as discussed earlier, to the existence of a long-range part of the effective quasiparticle interaction in the neighbourhood of a phase transition at low temperature. That an analogous breakdown, but at much lower temperatures, is expected to arise in principle from the effect of the long-range Lorentz force, was first noted by Holstein in his theoretical analysis of the Shoenberg magnetic interaction phenomena and (indirectly) of the Pippard anomalous skin effect [7].

A related problem is the transition from the superconducting to the normal phase as a function of pressure, composition, or magnetic field. Measurements of the latter, or of the temperature dependence of the transition field, have related a behaviour in low-dimensional systems which is in striking contrast with that expected in the traditional BCS model. The upper critical field in the cuprate superconductor $Tl_2Ba_2CuO_6$, for example, is found to rise steeply with positive curvature as the temperature is reduced, with no sign of saturation down to temperatures in the low millikelvin range [8]. The saturation expected in the standard model below a cross-over at intermediate temperatures (not far below T_c, where the Ginzburg–Landau correlation length falls below the intrinsic coherence length) is not observed. The correct interpretation of the temperature dependence of the transition field, and even of its precise connection to what is normally defined as the upper critical field, remains an unsolved problem.

The above and other related studies probe the limits of the notion of elementary excitations, which has been a corner stone of the theory of condensed matter for so long. The concept appears to remain useful even in cases where the quasiparticles are entities far removed from single electrons. But it may break down in the presence of long-range effective interactions in the above materials and, in principle, even in ordinary metals in the pure state at very low temperature. The description of such systems in simple terms would seem to require the introduction of a new theoretical framework.

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Kapitza and cryogenics

W F Castle

Introduction

The achievements of Kapitza in cryogenic technology and specifically helium liquefaction, expansion turbines and air liquefaction are reviewed, as are the developments since his day. It is hardly possible in this short paper to do justice to Kapitza's work in this field, but the importance of his initial work and the achievements he made, that have led to further development by others to the state of the art, are highlighted.

1. Helium liquefaction

Kapitza's main interest in low-temperature physics was generated by the need for refrigeration at low temperatures and particularly for his research in Cambridge in strong magnetic fields [1, 2]. He determined to use a more efficient way of liquefying helium than by using valve expansion alone; adiabatic expansion, i.e. doing external work, did provide such a process. He would have preferred to use an expansion turbine, but the size contemplated for helium liquefaction did not provide a practical solution, so a piston expansion machine was developed. At low temperatures, a difficulty arises in finding a lubricant for the cylinder which retains lubricating properties at very low temperatures and he developed a gas-lubricated reciprocating expansion engine. The expansion engine that he developed was carefully detailed with the concentricity of the piston and the cylinder being made to high accuracy and close tolerances to avoid significant gas losses [3, 4]. The helium liquefier used liquid nitrogen precooling and the expander produced further cooling. Kapitza's helium liquefier had a capacity of 5 litres h^{-1}. Liquefiers of similar type, known as Collins liquefiers, for use in small-scale laboratory use of liquid helium were subsequently produced on a factory scale in the USA. In a later development Kapitza produced a helium liquefier (18 litres h^{-1}) which used two expansion engines in series and avoided the need for liquid air/liquid nitrogen precooling [5].

More recently, helium refrigerators have been developed for larger applications. One such liquefier for the National Institute for Research in Nuclear Science at Harwell was produced by BOC with a capacity of 80 W at 4 K and with a liquid production potential. This 1964 refrigerator had two expansion turbines in series. Subsequently, much larger developments have taken place and, since 1980, there exist or are projected 9 or 10 refrigerators having capacities between 750 and 10 000 W refrigeration capacity. These refrigerators can be expected to employ between 3 and 7 expansion turbine steps — with 5 steps about optimum. Today, helium liquefiers with capacities between and 5 and 600 litres h^{-1} or 1500 W refrigeration are available. Two 12 kW helium plants have been built for CERN for LEP-200 by Sulzer (now Linde Kryotechnik) in Geneva. Similarly, liquid helium plants producing 2600 litres h^{-1} have been built for bulk helium liquefaction.

2. Expansion turbines

Kapitza, in considering air liquefaction, decided to modify existing practices for air liquefiers utilising low rather than the high pressures used. He showed that an expansion turbine could be used for this purpose and indicated that a significant increase in efficiency could be made. He adopted a design of radial inflow turbine and produced a turbine which showed an efficiency of 83% compared with that achieved by others of about 50% [6].

This turbine, produced in 1939, preceded the development of high-efficiency turbines at a later date by others, such as Elliot – Sharples, a few years later. In 1964/65, Jekat at Worthington produced an expansion turbine of about 92% efficiency. Current expansion turbines achieve efficiencies in this same range and with good reliability.

A modern development in expansion turbines is the use of magnetic bearings to minimise losses and hence maximise power recovery. Present-day turbines utilise fully adjustable nozzles designed to improve gas flow and maximise efficiency. They can also use a high-efficiency compressor brake that can directly utilise the energy recovery in the turbine by boosting the head pressure in the liquefier (for example) and hence contributing further to improved efficiency. In manufacture, the wheels are machined from solid material — allowing high rotational speeds without mechanical failure. Clearances are closely monitored and very small — to minimise internal losses. Expansion turbines used for hydrocarbon processes, for example, achieve very high capacities up to 5 MW power recovery for hydrocarbon processing plant. Kapitza's expansion turbine rotated at about 40 000 rpm with a maximum of 60 000 rpm, as do modern expansion turbines. An earlier development in the 1960s and 1970s by Sixsmith at Oxford and in NBS Colorado, USA and further developed by BOC, had a diameter of 9.5 mm and rotated at speeds in excess of 350 000 rpm. This was gas-lubricated with an efficiency about 50%, but could be used with advantage for applications such as the NIRNS refrigerator mentioned above. The expansion turbine has been further developed since then initially by Sulzer and now by Linde Kryotechnik.

3. Air and atmospheric gas liquefaction

For the plant to liquefy air, Kapitza designed an expansion turbine as described above to use a low-pressure rather than a high-pressure cycle and this plant operated with a maximum pressure of between 5 and 7 bar. This led the way in the development of large-tonnage oxygen plants operating at low pressures. Liquefiers, usually for nitrogen rather than air, utilise rotary machines, turbo compressors, and expansion turbines. Because of the size of liquefiers now contemplated [1000 metric tonnes per day (MTPD) liquid capacity is not uncommon] these principles can be applied with some success to higher pressures than those adopted by Kapitza. The higher mass flows that are feasible at higher pressures can reduce the overall size of the machinery and equipment without detracting from the performance of the plant; incidental system pressure drops, etc, also are lessened by operating at higher pressures. Current tendency is to use several expansion turbines, usually 2 or 3. The use of freon precooling at the higher temperatures is avoided because this requires expensive capital equipment and latterly the ozone-depleting effect of freons has caused them to be phased out. Kapitza's air liquefier operated with a specific power consumption of 1.2 kWh kg^{-1} of liquid air. Figures for comparable nitrogen liquefiers have improved from 1975 for example, when specific power consumption was typically 1.0 kWh kg^{-1}. Currently, with overall developments and improved efficiencies a typical figure would be 0.4 kWh kg^{-1}.

Following his laboratory-scale work on air liquefaction after his return to Russia in 1934, Kapitza founded an air separation factory at Balashikha near Moscow to deal with wartime needs of oxygen for the steel industry. This establishment has since grown into an extremely large cryogenic engineering contracting organisation. Kapitza's contribution to the establishment included the development of the low-pressure cycle for air liquefaction mentioned above with a capacity of 20 kg h^{-1} in the 1930s. In the period 1939 to 1942 the organisation produced, under his supervision, the TC200 plant series, having a capacity of 200 kg h^{-1} of liquid oxygen.

1943 to 1944 brought the development of the TC2000 plant, 2000 kg h^{-1} liquid oxygen. In 1945, the cryogenic engineering works at Balashikha near Moscow was constructed and this establishment later became part of Cryogenmash. A maximum size of oxygen plant over the period before and since Kapitza's day shows steady increase in capacity from, for example, 180 MTPD in 1932 through 1000 MTPD in 1960 to 2750 MTPD since 1975. Up to now, Cryogenmash has produced more than 500 large air separation plants with a total production capacity of 40×10^6 m^3 oxygen per annum or approximately 160 000 MTPD oxygen capacity.

Cryogenmash themselves directly indicate that "the heritage of P L Kapitza, the pioneer of Cryogenmash, is so rich that it cannot be easily summarised'' and that "he was an outstanding scientist, honoured as a founder of Cryogenmash'' [7]. The achievement of P L Kapitza in cryogenics is extremely significant and has led to very important developments in both technology and the establishment of a large cryogenic engineering activity in Russia.

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Liquid helium-4

W F Vinen

The lecture provided a brief history of the discovery of superfluidity in liquid ^{3}He and of the development of our understanding of this phenomenon, and it was aimed at a general audience.

Helium was first liquefied and cooled below 2 K by Kamerlingh Onnes in 1908, but it was not until the about 1930 that it became clear that there are two phases of the liquid: 'helium I' above about 2 K; and 'helium II' at lower temperatures; the change from one phase to the other being accompanied by a sharp peak in the heat capacity (the 'λ-point'). It was also by then clear that the solid phase does not exist at any temperature unless the pressure exceeds about 25 atmospheres.

The first clear indication that helium II has anomalous properties was the discovery by Keesom and Keesom in 1936 that it appeared to have a very high thermal conductivity. Allen, Peierls, and Uddin showed that this high thermal conductivity is anomalous in its character, the effective conductivity depending on the temperature gradient, ∇T, and the channel dimensions, and tending to become essentially infinite as $\nabla T \to 0$.

It was at this point in 1938 that Kapitza made his first important contribution. He argued that the high apparent thermal conductivity was probably due to some form of convection, the process being very efficient because of a very low viscosity. The viscosity had already been measured, by observing the damping of an oscillating disc in the liquid; its value appeared to be rather low, but not anomalously so. Kapitza suggested that this was not inconsistent with a very low real viscosity because the Reynolds number relevant to the experiment might have been very high, so that the flow was turbulent. Kapitza therefore set about measuring the viscosity by observing flow through a very narrow channel formed between two optical flats: he found that up to a critical velocity of some tens of centimetres per second the flow was impeded by no observable friction. This was the first reported discovery of superfluidity, although its publication in *Nature* was accompanied by a note reporting the same discovery, made independently, by Allen and Misener. The next couple of years saw the discovery of various related phenomena: the thermomechanical (fountain) effect by Allen and Jones; film flow, by Rollin and Simon and by Daunt and Mendelssohn; and the mechanocaloric effect, by Daunt and Mendelssohn. Important, detailed, and influential experimental studies of the nature of the heat transfer process in helium II and of the mechanocaloric and thermomechanical effects were published by Kapitza himself in 1941.

These strange phenomena offered a strong challenge to the theorists, and it is interesting that superfluidity has attracted the attention of some of the worlds most distinguished theoretical physicists: the London brothers, Landau, Onsager, Feynman, and many others.

Part of the reason for this interest lay in the fact that helium is a quantum liquid and that superfluidity is a quantum phenomenon. The importance of quantum effects in liquid helium was probably first recognised by Simon in 1934, who pointed out that the failure to solidify at low pressures is due to the effect of a high zero-point energy in the atoms.

The third law of thermodynamics requires that liquid helium at zero temperature be ordered. The form of the heat capacity at the λ-point is the typical signature of an order – disorder transition, and it seemed reasonable to assume that superfluidity was the result of this ordering process. It was suggested by Fritz London in 1938 that the ordering involved Bose condensation, such as was known theoretically to occur in the ideal Bose gas and that superfluidity was the result of the presence of a Bose condensate. It was recognised in due course that this suggestion was indeed correct, although in a less straightforward way than had been envisaged by London.

The important idea that the complex and often apparently irrational behaviour of helium II can be understood phenomenologically in terms of a two-fluid model (an interpene-trating mixture of normal and superfluid components, able to have separate velocity fields) was first developed by London and Tisza, but their attempts to identify the superfluid component with the Bose condensate were ill-founded. The first satisfactory theoretical basis for the two-fluid model was provided in remarkable work by Landau, which ultimately earned him a Nobel Prize in 1963 (strangely, Kapitza's own Nobel Prize for the discovery of superfluidity was not awarded until 1978). Landau developed his description of liquid helium at a low temperature in terms of a ground state in which a gas of weakly interacting excitations form, the excitations having a special dispersion relation, with only phonons at low momenta and rotons at higher momenta. Direct experimental evidence for this spectrum came much later from inelastic neutron scattering experiments. Landau was able to show that this excitation picture provides at least a partial explanation of the success of the two-fluid model, and, very importantly, to show that the temperature dependence of the normal and superfluid densities can be predicted. The predictions were soon verified by the direct experimental determination of these densities by Andronikashvili. The two-fluid model also led to the predicted existence of second sound, the discovery and study of which by Peshkov added further support to Landau's theory. The high thermal conductivity is due to a counterflow of the two fluids, so that heat transfer is indeed due to a kind of convection process, as envisaged by Kapitza.

Landau's theory was very successful and it represented a brilliant contribution to our understanding of superfluidity. But many questions remained unanswered. Why does the excitation spectrum have the suggested form? What happens at higher temperatures, when the independent excitation picture must break down? A persistent superflow can surely be only metastable; what determines its ultimate lifetime? What is really the nature of the ordered state in helium II? Does it involve some form of Bose condensation? How exactly is this ordering related to superfluidity?

An essential understanding of the form of the excitation spectrum was provided by Feynman in the 1950s, and his and other arguments have since been developed to provide a rather full understanding. Evidence that a form of Bose condensation, in which only about 10% of the atoms occupy the condensate even at zero temperature, has come both from theory and from experiments on deep inelastic neutron scattering. A description of superfluidity has been developed in which superflow is related to gradients in the phase of the 'condensate wave function' (CWF), and it has become recognised that this function is the appropriate order parameter in the system. A persistent superflow in seen as a metastable state of local equilibrium in which the phase of the CWF is constrained (as a consequence of the macroscopic occupation of the condensate), just as a constrained order parameter in other systems can lead metastable states with broken symmetry. However, the existence of the condensate alone is insufficient to ensure superfluidity. The ideal Bose gas is not a superfluid. Particle interactions are also necessary. The link with the Landau theory and the form of the excitation spectrum has come from the realisation, first by Bogolubov, that a combination of the condensate and the particle interactions forces the low-lying excitations to be phonons, although, interestingly, the Feynman argument for the form of the excitation spectrum makes no explicit reference to the condensate.

The fact that superflow is associated with gradients in the phase of the CWF, together with the requirement that the CWF be single-valued, leads to the quantisation of superfluid circulation in units of h/m_4, where m_4 is the mass of a helium atom. This is a macroscopic quantum effect, in which the Bose condensation effectively forces all atoms to have the same quantised angular momentum. Similar arguments lead to the requirement that the superfluid component can rotate only through the presence of free quantised vortex lines. These ideas were developed theoretically by Onsager and independently by Feynman, and it pleasant to remember that the early experimental verification was carried out in the Mond Laboratory in Cambridge, which Kapitza had established. With the discovery of quantised vortex lines all the essential elements of an understanding of the fluid mechanics of superfluid ^{3}He became available, although much work is still in progress to understand all the details.

Quantised vortex lines are also relevant to the metastability of superflow. In principle, a persistent superflow can always decay by the passage across the flow of one or more vortex lines (leading to what Phil Anderson has described as a slip in the phase of the order parameter). However, as is easily shown, this process in opposed by an energy barrier, so that decay of superflow can occur only if this barrier is surmounted, either thermally or by quantum tunnelling. Both processes have been the subject of theoretical study, and both processes have been observed experimentally under specially chosen conditions. Much of this work has been carried out quite recently.

There was insufficient time in the lecture to do more than mention much important work that has been carried out over the past thirty or forty years on liquid ^{3}He: the detailed study of the excitations in superfluid helium and of their interactions; the study of the superfluid phase transition, which has played an important role in the development of our understanding of phase transitions of this type; and the behaviour of superfluid helium in restricted geometries, such as thin films. Examples of interesting work that is currently in progress are provided by the study of small clusters of helium atoms (how large does such a cluster have to be before it shows evidence of superfluid behaviour?) and by the study of the production of vortex line on pressure quenching through the λ-line (which may provide an analogue for the production of cosmic strings in the early Universe). Although superfluid ^{3}He is now well-understood from many points of view, it is still the subject of interesting and fundamentally important study.

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Flow and textures in superfluid ^{3}He-A

H E Hall

The fact that superflow in the A-phase of ^{3}He is coupled to liquid crystal textures is one of the qualitatively new phenomena made possible by the richer order parameter structure of superfluid ^{3}He; I am sure that it would have appealed to Kapitza's constant interest in novel and surprising effects.

The subject started with a talk by David Mermin called 'Games to play with ^{3}He-A' [1], in which he showed that a simply connected vessel of ^{3}He-A must have at least one textural singularity on the surface, around which there were two quanta of circulation. Mermin called such a singularity a boojum, for reasons that will become apparent.

The orbital part of the A-phase order parameter is a vector triad, and both flow and textures are represented by rotations of this triad; the coupling arises because threedimensional finite rotations to not commute. The liquid crystal anisotropy direction is $\hat{l}$, the direction of the orbital angular momentum of the Cooper pairs, so that rotations of $\hat{l}$ produce a liquid crystal texture. But rotations around $\hat{l}$ produce a change of phase, so that gradients of such rotations correspond to superflow. It is the strong boundary condition that $\hat{l}$ is normal to a wall that makes surface singularities a topological necessity. The essential relationship between flow and textures is embodied most neatly in the modified circulation theorem for ^{3}He-A:

$$\oint_C S\,\mathrm{d}r = \frac{\hbar}{2m}\left[2\pi n + A(D)\right] .$$

In this equation the nonquantised term $A(D)$ is obtained by the following construction. For each point on the curve C along which the circulation integral is carried out, mark the local $\hat{l}$ direction as a point on the surface of a unit sphere. This maps the closed curve C onto a closed curve D on the surface of the unit sphere; $A(D)$ is the area enclosed by D, to the left in the direction corresponding to the integration along C. $A(D)$ can thus vary smoothly between 0 and 4π. A temporal change of texture that accomplishes this will change the circulation along a fixed path by two units; Mermin showed that suitable motion of a boojum would cause a supercurrent to "softly and suddenly vanish away" — hence the name. Alternatively, in a suitable static texture the circulation can change smoothly from one path to another; this gives the possibility of nonsingular vortex structures.

The $\hat{l}$ texture can be manipulated by magnetic fields via its dipolar coupling to the $\hat{d}$ vector, which is perpendicular to the spin of the Cooper pairs. Wheatkey's group found [2] that after a suitable sequence of field changes the A-phase could be put into a state where the strength of a received sound signal oscillated continuously, without any sign of decay, indicating a continuous periodic motion of the anisotropy axis $\hat{l}$, as shown in Fig. 1. The variation of oscillation period as the sample was heated and cooled provided evidence that the motion was not dissipationless, but rather was driven by superflow associated with thermal conduction by counterflow: the inevitable heat leak was driving the motion. The effect can be described semiquantitatively by numerical integration of the orbital equation of motion for a model texture. The essential effect is that the superflow itself causes the texture to precess in such a sense as to cause dissipation. If the flow is not maintained by an external source, the dissipation causes the flow to decay; we have the destruction of a persistent current by precession of a boojum envisaged by Mermin.

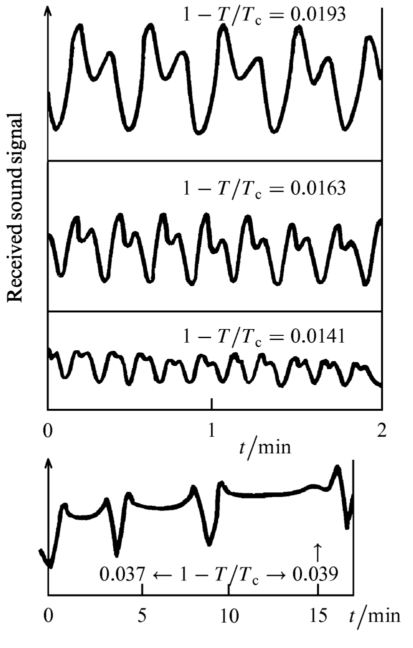

Figure 1. Ultrasonic signals showing textural oscillations in ^{3}He-A [2].

Nonlinear dissipation of flow associated with textural motion was investigated further in early torsional oscillator experiments at Manchester [3]. One of the most striking effects seen was a pronounced history dependence: in given circumstances the dissipation was largest if the experiment had last been cooled through T_c while oscillating with large amplitude. A fairly successful model of averaged dynamics was constructed in which the history dependent effect were modelled by a variable density of surface singularities.

The complementary aspect of coupling between flow and textures, static textures with vorticity, has been investigated more recently in a long series of experiments on rotating ^{3}He at Helsinki. The simplest nonsingular vortex texture is the Anderson–Toulouse two-quantum vortex, in which the $\hat{l}$ direction varies from up on axis through radial to down at large radii; such vortices can clearly be arranged to form a lattice. Most experiments, however, have involved NMR as a probe, and hence a magnetic field, commonly parallel to the rotation axis. Magnetic energies then set $\hat{l}$ largely perpendicular to the axis, the rotational symmetry is broken, and the soft core of the vortex splits into two parts, for example a circular and a hyperbolic texture, each with one quantum of circulation. The various possible vortex structures have characteristic NMR modes associated with them, so a major tool in studying them has been by identifying NMR frequency shifts. Evidence for association with vortices is provided by a signal strength proportional to angular velocity Ω.

But even almost twenty years after its beginning, the subject is capable of springing surprises. Very recently [4] it has been found in Helsinki that after a rapid sequence of rotations first one way and then the other, a state can be produced with an unusual NMR frequency shift and a

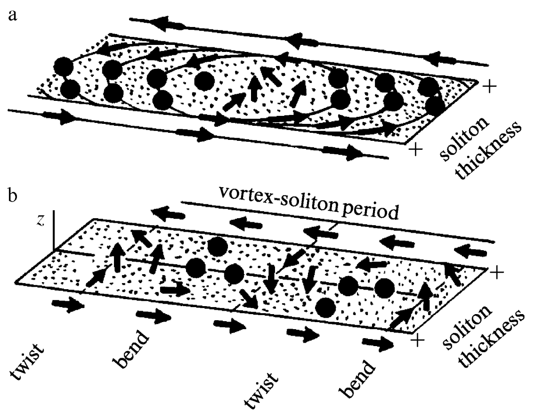

Figure 2. Decorating an $\hat{l}$–$\hat{d}$ domain wall with circular and hyperbolic vortices to make a vortex sheet [4].

signal strength proportional to $\Omega^{2/3}$. This is interpreted in terms of a particular type of vortex sheet. It is thought that the original preparative procedure somehow produces a domain wall, aligned parallel to the rotation axis, in which $\hat{l}$ switches from parallel to antiparallel to $\hat{d}$. Once such a wall has formed, it costs rather little energy to decorate it with an alternating sequence of one-quantum circular and hyperbolic vortices, turning it into a vortex sheet, the structure of which is illustrated in Fig. 2. The area of vortex sheet produced is determined by a balance between surface energy and counterflow energy, and it is this balance that gives rise to the characteristic $\Omega^{2/3}$ dependence found experimentally.

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Kapitza and Lancaster

A M Guenault

Of course the title is not serious. I doubt if Peter Kapitza ever visited Lancaster. But we hope that his spirit lives on there, in particular in relation to the work in the Lancaster ultralow-temperature physics group in recent years. The group has George Pickett, myself, Ian Miller, Ian Bradley, and Martin Ward as permanent members, shortly to be joined by Shaun Fisher.

Two of Kapitza's enthusiasms were (1) cooling methods and (2) superfluids. Let me briefly mention two items of interest about each.

About cooling methods, techniques have developed a lot since Kapitza's day, due in no small part to the availability of the isotope ^{3}He and to the invention of the dilution refrigerator, which makes the holding of stable temperatures of 2 to 5 mK a comparatively commonplace matter. Using this springboard, we now cool liquid ^{3}He deep into the superfluid phases to around 100 μK, by adiabatic demagnetisation of Cu nuclei. In cooling a metal lattice itself, we have recently achieved 7 μK [1]. This is the temperature recorded on a Pt-wire NMR thermometer, in contact with a copper plate which formed the final stage of a nested single demagnetisation cycle. The demagnetisation started from a magnetic field of 6 T, following a precooling to around 5 mK. The experiment was performed to check up on the physics of spin-lattice relaxation in copper; the temperature measured is the lattice temperature of the copper.

Kapitza's name is well known in low-temperature physics by its association with the 'Kapitza boundary resistance' between a solid and liquid helium. The use of sintered powder heat exchangers has long been a black art in refrigeration. We are just completing a series of measurements aimed at understanding the thermal contact between sintered silver pads and the saturated dilute phase of ^{3}He dissolved in ^{4}He in a dilution refrigerator. The pads are made from the usual submicron silver powder which is commercially available and widely used for heat exchanger manufacture. The temperature dependence of the effective boundary conductance in the dilute solution has long been thought to vary as T^{-2}, although there has been no theoretical explanation for this. Our latest measurements show a cross-over from a slow (roughly $T^{-1.5}$) dependence at high temperatures towards T^{-3}, as expected from any plausible phonon-based theory, at low temperatures. The crossover takes place in the 30 to 10 mK region, dependent on the thickness of the sinter. The explanation of the high-temperature behaviour is that the conductance is limited by conduction through the helium in the sinter pores. This result has much significance for the optimal design of heat exchanges in dilution refrigerators, showing that this sinters are better at high temperatures, but that at 5 mK one should use very thick (say 4 mm) sinter pads. It is also a daunting result for those of us attempting to search for ^{3}He superfluidity in the dilute solutions, since T^{-3} is worse news than T^{-2}.

Secondly, a few words about superfluid ^{3}He. Here the long-range Kapitza influence has been very active upon the Lancaster group, through the ideas and advice of scientists from the Kapitza Institute in Moscow. In the essentially isotopic B-phase of the superfluid, our experiments have been performed at around 0.1 times the transition temperature ($T_c \sim 1$ mK at zero bar pressure). At these low temperatures the few ^{3}He quasiparticles behave ballistically, with calculated mean free paths of kilometres. The key to understanding the properties of the superfluid in this limit has been to recognise the importance of Andreev reflection of the Fermi (quasiparticle, quasihole) excitations from superflow fields which are generated by any moving object. This has recently been demonstrated directly in an experiment employing retroreflection from a moving paddle of an excitation beam generated from and observed in a small 200 μK oven (see Ref. [2]).

Finally, the Moscow Kapitza influence has come through Borovik-Romanov and Bun'kov, to enlighten us about magnetic superfluidity and spin supercurrents in ^{3}He. In visits to Lancaster, Bun'kov has worked with us to discover a new type of long-lived state, in which persistent spin precession is observed up to 25 seconds after the exciting NMR pulse. An important new field of study is appearing here.

This talk is anecdotal and not comprehensive. What it does show is that Kapitza's hares are still running.

References

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Speech at Trinity College dinner for the Kapitza Centenary

S P Kapitza

It is indeed a great honour to be here tonight on the very day of the centenary of my father, Peter Kapitza. For me to speak here is even more remarkable. For although I was born in Cambridge, most of my life has been spent in Russia. In the past, when my father became so well known here in Cambridge, I was not able to understand much of what was going on. Of what really happened I have but scant memories, and perhaps the early fears of my childhood come to my mind first, as stark reminders of the past.

In the garage of our house at 173 Huntingdon Road, hung a fire extinguisher with a red dragon drawn on its black cylinder. I was so afraid of the dragon, that I never dared to go past it. I would wait and only go into the garage to get my bicycle when my father opened the main door to drive his car out. As part of the upbringing of a professor's son, I was sent to King's College Choir school. There again the solemn gothic hall scared me out of my wits. I also remember being taken to the Cavendish, where I saw the Cockcroft and Walton accelerator, the first of its kind. The huge machine with its weird high-voltage insulators towering up into the attic and a small hut covered with black cloth again scared me, and these images were strongly imprinted on my mind. Many years later I started to work with accelerators and maybe to defy these early memories, I developed the microtron — a small electron cyclotron that had no high- voltage insulators at all!

Recently in Moscow, and today in the new Cavendish, research was discussed, the origins of which could be traced to my father's work. Tonight I shall not speak of these discussions but rather try to take a broader, if personal, view of what happened, recall episodes from those fateful decades, and try to discern the future at a time of great changes. With the closing of this century and the approach of the threshold of a new millennium, I am sure that many will use this occasion and exercise their imagination as to what can happen. But apart from the mystique of the year 2000, I am sure that we are now crossing a boundary, a discontinuity in all human history. This will affect both science and society, and probably nowhere is this as obvious as in Russia. I have always felt that in an exciting, and even dangerous way my country has demonstrated and even amplified the critical phenomena of the world. Whether these experiences can be a lesson in history for others is yet to be seen, or perhaps the main lesson of history is that no such lesson can be learned?

Science in the Soviet Union had as much support as it could summon and scientists enjoyed their science, influence and even power. Now all that has gone, not because the day of reckoning has come, but mainly because the main social contract between science and society is now null and void. My father often repeated the words of Rutherford that you cannot serve God and Mammon at the same time. In those happy days subservience to Mars was yet unknown. What we see now in Russia is lack of support for 'big science'. The big science of huge accelerators, reactors, space stations and oceanographic ships, is now all stranded. Big science also has difficulties elsewhere — only recently the US Congress has cut the funds for the SSC. Is this the end of the boom-and-bust cycle of big science? Here in Cambridge, Kapitza was one of those who began that cycle and put engineering into the service of fundamental research. The Cockcroft – Walton accelerator that so scared me as a child and the invention of the cyclotron by Lawrence which followed were the early precursors of the SSC.

But Cambridge and the Cavendish are special. As soon as these big machines had been pioneered here, they were taken and expanded elsewhere. Later radioastronomy led to its own large telescopes and even molecular biology now has its big project — the human genome. Probably there is a lesson to be learned here. Although Kapitza in Moscow did profitably exploit science for technology and use modern technology for science, he did not build the big machines that were developed in other places largely to serve the grandeur of the state. The association with the military became fateful for my father, for it led to his dismissal from the Institute, since he was not ready to participate as demanded in the Bomb. Now I think our science has to face many of the consequences of its reliance for support from the military – industrial complex, when scaling up numbers was expected to lead to quality of research. That does not happen, as my father well knew. It was another precious lesson he learned in Cambridge, a lesson passed on to those who followed him in directing the Institute, Professors Andreev and Borovik-Romanov, who are here today.

Earlier I mentioned fire extinguishers. The end of my father's difficulties, as they were euphemistically called, was signalled when on the fine morning of 26 June 1953 a car brought two people from Moscow to our laboratory in the garage at his dacha (country house). One was the chief of security from the Academy of Sciences, and he introduced the inspector for fire fighting equipment. I was asked to help the inspector, but very soon I saw that he did not really understand the difference between a foam and a carbon dioxide fire extinguisher! My father was always very particular with safety matters in our laboratory. But fire extinguishers were only an excuse for the presence of these people, who abruptly left at 4 o'clock. Later I drove to town, only to see tanks in battle order on the streets of Moscow withdrawing from the city. Definitely, strange things were happening. Next morning my mother-in-law confided to me that a good friend of hers, who was working as an internal decorator on contract for the Police Club of Moscow, was asked by the manager to remove the portrait of Beria discreetly! He was the head of the Soviet police, the arch enemy of my father, and after the dragon had been shot things changed for the better. I still do not know what was the real mission of those people who came to us on the day of the arrest of Beria, but recently it was revealed that in 1953 he had planned to assassinate my father. Very probably it was to be done by the person who now writes disreputable spy stories about the nuclear arms race.

When my father returned to the Institute, he did not resume low-temperature work. He pursued instead the research he had started in his dacha laboratory on microwave electronics and plasma physics. Unfortunately, this work, which has now ceased, did not lead to any great breakthroughs, probably because a long break in research and in contact with the academic community can have very adverse effects on scientific work. This is an important point in view of the present large and sudden disruption now being experienced by Russian science. This major disruption is the result of the profound social changes now happening in our country where the support of science has all but gone. With the breakdown of the former system of support, this is why a new understanding between science and society is now so necessary. I wonder now what my father would do today. I am sure that at all cost he would continue with research, however difficult it might be. He would also be confronted by the brain drain of scientists both from science and from Russia, but would probably take this as a new departure in their careers and see no great tragedy in these changes. For those who leave the country, my father's life is an instructive story of the brain drain. He profited much from his stay in England and it shows how important such exchanges can be. However traumatic and even ruthless was his detention in 1934, it can be seen as an example of reversing the brain drain, though by methods that can hardly be justified. But what happened was that he managed to get the resources for his work in Moscow through difficult negotiations with the government and due to the generous help and understanding of Lord Rutherford. In less than two years the Institute, now named after Kapitza, was built. It was there that superfluidity was discovered, later to be recognised by the Nobel Prize, and many other discoveries were made by a remarkable group of scientists, which included Landau.

Kapitza was always internationally minded. In the early 30s he did much to help refugees from Germany and I am sure today he would have been similarly concerned with people leaving our country today simply because they have no money for their research. With the lack of support for science, we now have this massive brain drain from Russia and have to see what will be the long-term consequences of this efflux of highly trained manpower. On the other hand, it can be seen as a compensation for the lack of real contacts of Russian science, and for the long years of self-imposed isolation. But I am sure he would have given all support to the training of the next generation, finding means for students and postgraduates and, first of all, helping the young ones through these difficult times, as difficult as he himself experienced early in his life. Like our young generation, my father was fortunate in the education he got in Petrograd at the Polytechnical Institute, a remarkable school founded on the lines of the German Technische Hochschule. Later my father had the great experience of being here in Cambridge, and what he learned here was important for the influence he had on education in Russia and in the founding of the Institute of Physics and Technology in Moscow. He discussed these issues with Cockcroft, who was in a similar way engaged in setting up Churchill College, where the idea was to bring together teaching and research for training engineers in what we now call high technology. The continuity of teaching and research is most important at times of change and crisis. In the Soviet Union we had a very unfortunate experience with the disruption of biology and genetics during the Lysenko affair in 1948. Only 40 years later with much support nationally and internationally did we manage to revive these sciences. German science, in ruins after World War II and in the aftermath of Nazi rule, has only now regained the position in science that could be expected from its past. This shows how perilous can be any disruption in the development of science and how long it takes to rebuild a lost tradition.

In Moscow a decision has recently been passed by the government to establish a Kapitza Foundation. Its charter is yet to be worked out, but it is to support research and teaching in science both nationally and internationally. I do hope it will develop into a useful instrument for the support of science in these difficult years of transition. I am sure that much can be done to provide funding for science in Russia and help to develop its international relations. I hope that with the decisions now made and the recognition of the necessity of new departures for the funding of science, the Kapitza Foundation could make a significant contribution. Here it should be mentioned that the Royal Society has independently established the Royal Society Kapitza Fellowships. These have given unique opportunity for nearly a hundred Russian scientists to visit the UK for research and I would like to express the sincere thanks of all my colleagues for this generous help. We can expect that this important initiative will teach us how to act for future developments in setting up international ties between scientists, appropriately linked with the name of my father. Today, the highest priority for science in Russia is to provide for the younger generation of scientists. At this time of transition it is they to whom the future belongs. It is the younger generation that can be really incorporated into world science and the story of my father shows how important this had been in his life.

Of all institutions of science and culture universities are the most significant and permanent. In no place such as here, in the magnificent hall of Trinity College, can this meaning and tradition be experienced so fully. When my father after so many years away came back for the first time to Cambridge in 1966, it was here that his homecoming was symbolised by the appearance of his gown, waiting so long for its master. Today the memories that my father evokes bring us back together for the occasion of his 100th birthday to see what has happened since and what we can expect in the future. My father came to Cambridge as a young man and the University of Cambridge became his home for 13 years. It was here that he became a member of the world of science, a Fellow of the Royal Society and later, on his return to Russia he was a messenger of all he learned here. I think he also brought something to this country in the all-important traffic of ideas and customs. A university will always be the training ground for students, a centre of excellence in science, a place for stepping into the future and the unknown.

To the University of Cambridge!