Journal of Low Temperature Physics, Vol. 146, Nos. 5/6, March 2007 (© 2007)
DOI: 10.1007/s10909-006-9276-7

The Discovery of Superfluidity

Sébastien Balibar

Laboratoire de Physique Statistique de l'Ecole Normale Supérieure associé aux Universités Paris 6 et 7 et au CNRS, 24 Rue Lhomond, 75231 Paris Cedex 05, France
E-mail: balibar@lps.ens.fr

Superfluidity is a remarkable manifestation of quantum mechanics at the macroscopic level. This article describes the history of its discovery, which took place at a particularly difficult period of the twentieth century. A special emphasis is given to the role of J.F. Allen, D. Misener, P. Kapitza, F. London, L. Tisza and L.D. Landau. The nature and the importance of their respective contributions are analyzed and compared. Of particular interest is the controversy between Landau on one side, London and Tisza on the other, concerning the relevance of Bose–Einstein condensation to the whole issue, and also on the nature of thermal excitations in superfluid helium 4. In order to aid my understanding of this period, I have collected several testimonies which inform us about the work and attitude of these great scientists.

PACS NUMBERS: *67.40.-w; 01.65.+g.*

1. INTRODUCTION

Scientific progress has become a collective process. No physicist can ever pretend that he has achieved something, that he had a personal idea or made any original discovery independently of his colleagues. Recognizing this situation does not mean that it is impossible to identify the authors of scientific discoveries, but one should do it carefully. Instead, there is some tendency to attribute discoveries to single persons, an attitude which is not fair enough. Moreover, our prize tradition is certainly very nice, stimulating and generous, but it has some drawbacks: one is tempted to forget those among our colleagues who did not win. With these ideas in mind, I have found particularly interesting to inquire about the history of the discovery of superfluidity.

As we shall see, I am not saying that Kapitza in 1978 or Landau in 1962 were awarded the Nobel prize for the discovery of superfluidity, nor criticizing this choice. In fact, for Kapitza it was "for his basic inventions and discoveries in the area of low-temperature physics" and for Landau "for his pioneering theories for condensed matter, especially liquid helium." Furthermore, in the official presentation speech of Kapitza's prize, it was mentioned that "The same discovery was made independently by Allen and Misener." However, since superfluidity occupies a large part in the official presentation of their prizes, and since nobody else was recognized at that level for the discovery of superfluidity, there is a general tendency to forget that other great scientists have achieved major contributions to this discovery. It is this tendency which I wish to criticize. One example is the article *Superfluidity* in the Encyclopaedia Britannica, which starts with the sentence: "*Superfluidity in helium-4 was discovered in 1938 by the Soviet physicist Pyotr Leonidovich Kapitsa.*" Another striking example is the presentation speech of the 1996 Nobel prize to Lee, Osheroff, and Richardson, where one reads: "*it was not until the end of the 1930s that Pjotr Kapitsa (Nobel Prize 1978) discovered experimentally the phenomenon of superfluidity in helium-4*" (no mention of Allen and Misener this time). As we shall see below, Landau also considered that superfluidity had been discovered by Kapitza only and he must have had a strong influence on the opinion of his colleagues; for example E.M. Lifshitz wrote: "*I have been asked by the editors of Scientific American to give a short survey of what has been learned about superfluidity, first discovered in 1937 by Peter L. Kapitza at the Institute for Physical Problems in Moscow*".[1] As for the attribution of the theoretical understanding of superfluidity to Landau, the situation is more subtle, especially since the discovery of superfluidity in alkali gases[2,3] where the existence of Bose-Einstein condensation is obvious, but it is somewhat similar. For example, R. Donnelly wrote[4] : "*Finally, there was no great scientific leader active in understanding liquid helium in the early days. When Kapitza and the great theoretical physicist Landau, followed by physicists such as Fritz London, Lars Onsager, Richard Feynman and other greats, came on board, there was a tremendous surge of excitement, which lasted for many years and helped bring the subject to its present state of understanding.*" I wish to explain that the contributions by London and by Tisza, which were published three years before Landau's, were major breakthroughs in the understanding of superfluidity. Fortunately, my opinion seems to be shared by several other authors.[5,6]

Some aspects of this issue have already been considered by several authors, especially by R. Donnelly in the article mentioned above,[4] by K. Gavroglu in his biography of Fritz London,[7] by A. Griffin at a summer school on Bose–Einstein condensation (BEC)[8] and in his study of "John C. McLennan and his pioneering research on superfluid helium,"[9] and by

J. Matricon and G. Waysand in their book.[10] When trying to go deeper into it, I distinguished three more precise questions:

1. Who made the experimental discovery ?
2. Who has initiated its theoretical understanding ?
3. How did all this happen in a period (the late 1930s and early 1940s) where the world was torn apart by conflicts and wars ?

One usually considers that superfluidity was discovered in December 1937, the submission date of the two articles on the flow of liquid helium which appeared side by side in *Nature* on January 8, 1938. On page 74 was the article by P. Kapitza[11] and on page 75 the one by J.F. Allen and A.D. Misener.[12] As we shall see, very important work was also done before, especially in Toronto and in Leiden, but it is really the publication of these two articles which triggered the theoretical work of London, Tisza, and Landau. The purpose of this article is to put everyone's work back in its historical and scientific context, so that the importance of each contribution could be judged. It is also to analyze the very interesting controversy which opposed Landau to London and Tisza about the role of BEC in superfluidity and about the nature of excitations in superfluid ^{4}He. In order to understand it I have recently inquired from Tisza himself, from D. Shoenberg, and from A. Abrikosov whose testimonies are reproduced here. I am also grateful to L. Pitaevskii, G. Volovik, A. Griffin, H. Meyer, and G. Gorelik for several fruitful discussions. I cannot pretend that I have fully understood the role and the attitude of every actor in the discovery of superfluidity but I hope that this article will stimulate further research on this very important event in the history of twentieth century physics.

2. EXPERIMENTS

The two articles published in *Nature* are, respectively, entitled: "Viscosity of liquid helium below the lambda point" (page 74), received December 3, 1937, by P. Kapitza (Institute for Physical Problems, Moscow, Russia) and "Flow of liquid Helium-II" (page 75), received on December 22, 1937, by J.F. Allen and A.D. Misener (Royal Society Mond Laboratory, Cambridge, UK). Both the expressions "lambda point" and "helium II" refer to the work of W. H. Keesom and his group in Leiden. T_λ is the temperature now known as 2.17 K where Keesom, Wolfke and Clusius[13,14] discovered an anomaly in the properties of liquid ^{4}He : the graph of the temperature variation of its specific heat has a sharp maximum with the shape of the Greek letter λ. Thanks to a series of experiments, Willem Keesom had realized that ^{4}He had two different liquid states which he called "Helium I" above T_λ, and "Helium II" below (for a review, see Keesom's book[15]). It must have been rather surprising to find two different liquid states for liquid helium which is made of simple spherical atoms without chemical properties.

In 1937, Kapitza tried to understand why, a year earlier in Leiden, the same Willem Keesom had found with his daughter Ania[16] that the thermal conductivity of helium II was anomalously large, a phenomenon which had also been studied by B.V. Rollin in Oxford[17] and by J.F. Allen, R. Peierls, and M.Z. Uddin in Cambridge.[18] Kapitza thought that convection in this liquid could be important if its viscosity was small and that it could be responsible for the large apparent thermal conductivity. He thus tried to measure this viscosity by flowing liquid helium from a tube through a slit about 0.5 μm thick, between two polished cylinders pressed against each other. In his article,[11] Kapitza writes:

"*The flow of liquid above the λ-point could be only just detected over several minutes, while below the λ-point the liquid helium flowed quite easily, and the level in the tube settled down in a few seconds. From the measurements we can conclude that the viscosity of helium II is at least 1500 times smaller than that of helium I at normal pressure.*

The experiments also showed that in the case of helium II, the pressure drop across the gap was proportional to the square of the velocity of flow, which means that the flow must have been turbulent. If, however, we calculate the viscosity assuming the flow to have been laminar, we obtain a value of order 10^{-9} cgs, which is evidently still only an upper limit to the true value. Using this estimate, the Reynolds number, even with such a small gap, comes out higher than 50,000, a value for which turbulence might indeed be expected."

These two paragraphs are a little difficult to understand. Kapitza does not give any value for the flow velocity in the slit, nor for the height difference which drove the flow. Since the Reynolds number is $R = UL/\nu$ where U is the velocity, L a typical length scale and ν the kinematic viscosity ($\nu = \eta/\rho$ where η is the viscosity and ρ the density), I understand that, with $\eta = 10^{-9}$ cgs, $\rho = 0.15 g/cm^3$, and $L = 5 \times 10^{-5}$ cm, he must have measured velocities U up to about 7 cm/s. As we shall see, this is comparable to what had been measured by Allen and Misener and confirmed by later work, although it depends very much on the size of the flow (see the review in the book by Wilks,[19] p. 391). As far as I know, Kapitza's square law for the pressure drop has not been confirmed by any later work, but since he does not give much quantitative information on his measurements, it is difficult to appreciate the accuracy at which this square law could fit his data. Given what is known today, I suppose that he approximated the pressure dependence of the velocity–which shows a threshold–with a square law. Anyhow, Kapitza finally proposes that:

"*by analogy with superconductors, … the helium below the λ-point enters a special state which might be called superfluid.*"

This is a famous sentence where Kapitza introduces the word "superfluid" for the first time. His intuition was quite remarkable because superfluids and superconductors are indeed analogous states of matter, but Kapitza wrote this sentence long before the BCS theory of superconductivity was established, *a fortiori* before any demonstration of such an analogy.

As for the article by Allen and Misener, it starts with the sentence:

"*A survey of the various properties of liquid helium II has prompted us to investigate its viscosity more carefully. One of us*[*1*] *had previously deduced an upper limit of* 10^{-5} *cgs units for the viscosity of helium II by measuring the damping of an oscillating cylinder. We had reached the same conclusion as Kapitza in the letter above; namely that, due to the high Reynolds number involved, the measurements probably represent non-laminar flow.*"

Before arguing on the question of priority between Moscow and Cambridge, I wish to comment on the note [1]. It refers to the article by E.F. Burton published in 1935 by Nature.[20] In this short article, Burton explains that, by measuring the damping of an oscillating cylinder which was suspended by a thin rod, it was possible to measure the viscosity of liquid helium. This method was later improved by Dash and Taylor[21] and again by Andronikashvili and by Reppy[22–24] for extensive studies of superfluidity. He further explains that, below T_λ, the viscosity drops down by several orders of magnitude. He finishes with the sentence:

"*This work was carried out by Messrs. Wilhelm, Misener and A.R.Clark.*"

Burton was the head of the Toronto Physics Department where Misener was a Master's graduate student at that time, and Wilhelm and Clark were two technicians in cryogenics. The details of this work were later published by Wilhelm, Misener and Clark in the Proceedings of the Royal Society[25] and I am rather surprised that, at that time, the head of a physics department could publish work by members of his department without including their names in the list of authors. Since the three real authors of the work published without including Burton as a co-author, one could imagine that there was a conflict between them but this is probably not true since, two years later in his Nature article with Allen, Misener referred to Burton instead of referring to his own article… I have to suppose that publication policies have evolved a lot since that time. It remains clear that, as soon as in 1935, the existence of an anomaly in the viscous dissipation in helium II had already been demonstrated in Toronto. However, in 1935, no one had realized that the hydrodynamics of helium II was so anomalous that its viscosity could not be measured with classical methods.

At the beginning of his article, Burton also explains that liquid helium stops boiling when cooled below T_λ. This phenomenon had been observed by McLennan 3 years earlier in Toronto[26] and it was later attributed to its very large thermal conductivity. For all physicists working on liquid helium, it remains the spectacular manifestation of quantum order taking place in this remarkable liquid (see Fig. 1).

More important is the reference to Kapitza at the beginning of the article by Allen and Misener. We understand that they had read Kapitza's article before writing their own, or at least that they had heard of its content. Together with the 19 days difference in the submission date, this has sometimes been taken as a proof that Kapitza had some priority on Allen and Misener in the experimental discovery of superfluidity.[22] However, as we shall see, I do not agree with such a statement.

The Cambridge article contains a detailed study of the flow through two different capillaries with sections respectively equal to 6×10^{-4} and $0.8\,\text{mm}^2$. Measurements are given at two different temperatures (1.07 and 2.17 K) and at series of ten different pressures. Flow velocities range from 0.4 to 14 cm/s. Their main findings were that, contrary to Poiseuille's law which describes laminar situations, the velocity was nearly independent of pressure, also independent of the capillary section. The measurements by Allen and Misener could obviously not be done in 19 days. I cannot imagine that they started their study after hearing of Kapitza's article. If a proof is needed, it is in their notebook which shows that Allen and Misener had obtained results already on November 24, 1937.[27]

Let us now comment on Kapitza's work. Kapitza had graduated as an electrical engineer in Saint Petersburg under the supervision of F. Ioffe (1918). In 1921, Ioffe suggested that Kapitza goes to Cambridge where he could work with Rutherford. There, he proved to be a brilliant experimental physicist. For example, he made the first detection of the bending of alpha-particle paths in a magnetic field thanks to a cloud chamber. Then, he built a pulsed magnetic field installation and a hydrogen liquefier with his student John Cockcroft. He was quickly elected Fellow of Trinity College (1925) and Fellow of the Royal Society (1929) "*a rare distinction*

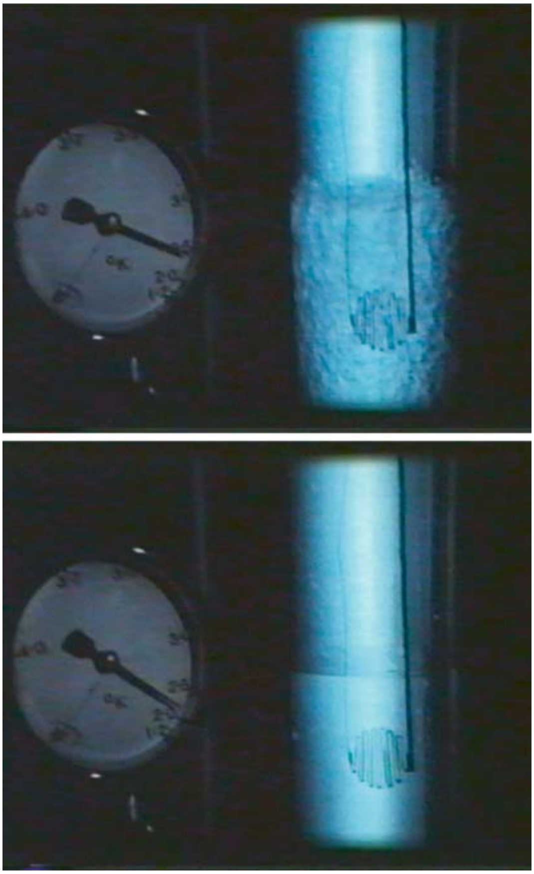

Fig. 1. As shown by these two images from a film by J.F. Allen and J.M.G. Armitage, superfluid helium stops boiling below T_λ. This is due to its large thermal conductivity. The top image is taken at 2.4 K as indicated by the needle of the thermometer on the left. The bottom image is taken just below the lambda transition.

for a foreigner, especially for one who became a Corresponding Member of the Soviet Academy of Sciences in the same year," as explained by David Shoenberg.[28] Then, Rutherford obtained from the Royal Society that part of the donation from Ludwig Mond was used to build the "Royal Society Mond Laboratory" where Kapitza could develop his low temperature and high-magnetic field studies. In this laboratory, he constructed a new type of helium liquefier which produced its first drops of liquid helium on April 19, 1934, and made such experiments much easier.[29]

In the summer of 1934, Kapitza went back to Leningrad. He had come to see his mother and to participate in a symposium celebrating the centenary of Mendeleiev. However, on September 24, 1934, 5 months only after the first operation of his liquefier in Cambridge, he was not allowed to return to England from the Soviet Union.[29] The reasons for this are a little unclear but, according to Ref. 28, "*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 that he must in the future work for them, although in fact none of his work was secret and it was available to everyone*". Later, Stalin would need Kapitza for his nuclear program and conflicts with Beria triggered Kapitza's disgrace. But in 1934, Kapitza started a fight with Stalin and Molotov to obtain support for his research. Two years later, the "Institute for Physical Problems" was built for Kapitza in Moscow. Thanks to the help of Rutherford, he could also arrange that part of his equipment be purchased from Cambridge and transferred to Moscow, so that he could start his research again.

At that stage, one major problem for Kapitza was that Cambridge had kept his liquefier. In 1935, liquefiers existed only in Leiden, Toronto, Cambridge, Oxford, and Kharkov. But Kapitza also obtained the right to invite his student David Shoenberg and two technicians, E.Ya. Laurmann and H.E. Pearson, in order to build a new helium liquefier in Moscow. They made a better liquefier which produced liquid helium on February 22, 1937.[29] Meanwhile, Cambridge had used Kapitza's rather high salary (800 pounds a year) to hire two younger scientists, Rudolf Peierls and J.F. Allen who seemed satisfied with 400 pounds a year each.[30]

John Franck ("Jack") Allen was born in Winnipeg (Canada) and he had obtained his Ph.D on superconductivity in Toronto (1933). Then, he tried to join Kapitza in Cambridge but when he arrived in the fall of 1935, Kapitza was already detained in USSR. In 1936, he attracted Donald Misener to work toward a Ph.D degree in Cambridge with him. We thus realize that Kapitza was competing with two Canadian physicists who were using his former liquefier in his former laboratory where he was still in close contact with other people. Of course, this situation was very painful to him ("*… I often see my laboratory in my dreams, and painfully want to work…*" as he wrote to his wife[29] in March 1935). Anyhow, when Kapitza sent his letter to *Nature*, he wrote in the accompanying letter to the editor:

"*Dear Gregory,*

I am sending herewith a short note: 'Viscosity of liquid helium below the λ-point', which I hope you will kindly publish in your 'letters to the editor.' I think this is an important note and I should be glad if you could arrange it to be published as soon as possible, and with the day of dispatch. Please *do not bother to send the proofs to me here to Moscow, it takes too much time. If necessary please send them either to Prof. P.A.M. Dirac, Dr. J.D. Cockcroft, or to Dr. W.L. Webster … All my good friends [are] sufficiently competent to make the necessary corrections. I hope you will kindly help me in publishing this note very soon …*"

As explained by Allen himself[30] and by Shoenberg,[28] it was John Cockcroft[31] who took care of the proof-reading. He was the new director of the Mond Laboratory since Kapitza had left. In December 1937, he showed Kapitza's letter to Allen and Misener and asked them to write down their own results as quickly as possible. He finally asked *Nature* to publish the two papers side by side.

It is clear to me that the Cambridge work was independent of Kapitza's work in Moscow. My main reason is that, as an experimental physicist in the same field, I know that it is not possible to make all the measurements which are presented by Allen and Misener in 19 days only.

Now, was Kapitza's work independent of the Cambridge work? After all, Kapitza's insistence to be published with a mention of the date of receipt indicates that he probably knew that his competitors were working on the same subject. Furthermore, his letter presents qualitative ideas which could have been written down quickly. One should also note that the ability of helium II to flow through narrow slits (the existence of "superleaks") had been discovered in 1930 by Willem Keesom.[32] But one does not know if Kapitza was aware of Keesom's observation. Could Kapitza have written his letter *after* hearing of the progress made by Allen and Misener in Cambridge? Nobody having ever mentioned such a possibility, I wish to consider it carefully. When I asked David Shoenberg to tell me about this period, he answered:

"*My memory of the events is not entirely reliable, though I do remember that I helped translate the Russian version of Kapitza's letter into English. Kapitza's letter was sent to Nature with a request that proofs should go to Cockcroft rather than back to Kapitza in Moscow. Also W.L. Webster who had been briefly visiting Kapitza in Moscow took a copy to show to Cockcroft. Cockcroft had not, I think, known of Kapitza's work and showed the note to Jack Allen who had obtained basically the same result ('superfluidity' below the λ point) and suggested that he writes a brief note (with Misener) in which he commented on Kapitza's note. Cockcroft asked Nature to print the two notes side-by-side but it is quite clear that Kapitza's note had 'official' priority (a) because of the dates of receipt by Nature and (b) because evidently Allen had seen Kapitza's note before he wrote his own. It is a pity Allen never got adequate recognition of his quite independent* *discovery of superfluidity–he and Kapitza could well have shared a Nobel prize! I don't think Allen and Kapitza ever met till much later. I know that Kapitza was at first rather cross that his He liquefier was used while he had to wait a long time before he had liquid helium in Moscow (see one of his letters to his wife in 1934 or 5). I don't think Kapitza and Allen ever communicated directly by letter. I myself know Kapitza was getting exciting new results while I was in Moscow (as a guest visitor) and knew that Allen was continuing his work on He (fountain effect etc. was already published) but I have no memory of discussing the work of either with the other. At that time it would have been dangerous to write to anyone about work going on in Moscow. I was in Moscow from September 1937 to September 1938. I did not travel out of the Soviet Union at all during that time.*"[33]

According to Shoenberg, the work in Moscow was thus independent from the one in Cambridge because there were no contacts between Cambridge and Kapitza, but I cannot believe this because letters have been published by Rubinin[29] which show the opposite. For example, Rutherford sent a letter to Kapitza on October 9, 1937, where he wrote:

"*My dear Kapitza,*

… Bohr told me about his trip to you [in June 1937], and I am very interested to hear of the work that you have been able to accomplish. No doubt Pearson, when he returns, will be able to give us the latest information about your big helium liquefier. The Mond laboratory is very flourishing, and a large amount of work is in progress… Some interesting experiments are also in progress on the extraordinary heat conductivity of helium at low temperatures. The conductivity is very large for small differences of temperature, and falls rapidly with the quantity of heat transmitted…"

This was ten days only before Rutherford died, and Kapitza must have known this death rather quickly because he sent a letter to John Cockcroft on the first of November where he wrote:

"*My dear John,*

It is difficult to believe that there is no more Rutherford… Things in the lab are not going badly at all. We just started the new liquefier and the first time it gave four liters per hour. Now it is quite certain that Pearson will be free before the new year, I will not claim his services any more after that…"

Obviously, there were regular contacts between Kapitza and his friends in Cambridge. Furthermore, according to Rubinin,[29] Webster had visited Kapitza in Moscow in September 1937. As a consequence, it seems to me that Kapitza might well have known something about Allen's results.

However, I am not saying that Allen and Misener have a priority on Kapitza, in particular because of a letter from Kapitza to Niels Bohr, dated December 10, 1937, where he says:

"*Dear Bohr,*

I had your letter about the death of Rutherford, which apparently crossed with mine. I had a number of letters from friends, and it is indeed wonderful how much the people appreciated Rutherford… All this time I was very busy working on the viscosity of helium below the λ-point. May be you remember what I was telling you during your visit here about the idea of the work, the experiments are in full progress, but the preliminary results are quite interesting. It appears that really below the λ-point the viscosity of helium drops more than a 1000 times… I made the experiments about 20 times varying the conditions and looking for some possible errors, but could not find any. I am sending herewith a copy of my preliminary note to Nature, so if you will be interested you could glance through it… Yours very sincerely,

P. Kapitza"

Since Bohr's visit was in June 1937, this letter proves that Kapitza was at least planning his experiments 6 months earlier. It also shows that Kapitza did much more than a single experiment before sending his letter to *Nature*. My conclusion on the priority issue is that there is no priority in either way, the two works are independent.

Let us finally summarize the content of the four experimental contributions to the problem of superfluidity. In 1930 Keesom had observed that helium II was highly fluid and in 1935 Wilhelm, Misener, and Clark had measured in Toronto a sharp drop of the viscosity below the λ-point. Then, in December 1937, Kapitza claimed that the flow of liquid helium II was turbulent and that its viscosity could not be larger than 10^{-9} cgs units. As for Allen and Misener, they presented the results of a series of measurements, from which they concluded:

"*the observed type of flow … in which the velocity becomes almost independent of pressure, most certainly cannot be treated as laminar or even as ordinary turbulent flow. Consequently any known formula cannot, from our data, give a value of the viscosity which would have much meaning.*"

In my opinion, it is Allen and Misener who discovered that, below T_λ, the hydrodynamics of helium required a totally new interpretation. At that time, everyone else kept considering that liquid helium was a liquid with a small viscosity. Here is the real experimental breakthrough.

It would be very interesting to understand how Kapitza had the intuition that helium II had something in common with superconductors.

The idea that superconductors were quantum systems which had to be described by a macroscopic wave function had been put forward by Fritz London and his brother Heinz[34] but, as we shall see now, London had not yet considered that it could be the case for liquid helium also. Furthermore, and as we shall see when considering Landau's work, this was not the way how one liked to think about liquid helium in Moscow.

London's new ideas[35] were triggered by the next article[36] published by Allen in the same volume 141 of *Nature* on February 5, 1938. Entitled "New phenomena connected with heat flow in helium II", this new letter described the discovery of what is now known as the fountain effect: together with Misener again for the experiments, but published with H. Jones, the new young theorist who replaced Peierls in Cambridge, Allen discovered that, when heat was applied to liquid helium II on one side of a porous plug, the pressure increased proportionally to the heat current so that the level of the free surface went up (it was later realized that the fountain pressure was in fact proportional to the temperature difference between the two sides). A liquid jet could even occur if the pressure was high enough. If the liquid had been classical, the vapor pressure would have been higher on the warm side so that, in order to maintain hydrostatic equilibrium in the liquid, its level would have had to go down. Allen and Jones explained that the opposite was observed. For London, it was no longer possible to doubt that this liquid had totally anomalous properties for which a radically new interpretation was needed. In previous work[37], Fritz London had proposed that helium II was more ordered than helium I (its specific heat decreased sharply below T_λ) and perhaps some kind of crystal with a diamond lattice. However, on March 5, 1938, London sent a letter to *Nature* which was published on April 9. There, he explained that liquid helium II was not crystalline before proposing that it was undergoing some kind of Bose-Einstein condensation at T_λ.[35]

3. LONDON AND TISZA

In the introduction of his first book,[38] London, writes:

"*In 1924, Einstein developed a very strange concept of a gas of identical molecules, which were assumed to be indistinguishable... Einstein remarked that this removal of the last vestige of individuality from the molecules of a species would imply a statistical preference of the molecules for having the same velocity, even if any interaction between them were disregarded, and this preference would lead, at a well-defined temperature to a kind of change of state of aggregation; the molecules would 'condense' into the lowest quantum state, the state of momentum zero. Einstein did not give a very detailed proof, and his remark received little* *attention at the time. Most people considered it a kind of oddity which had, at best, an academic interest, for at the extremely low temperatures or high pressures in question there are no gases, all matter being frozen or at least condensed by virtue of the intermolecular interaction forces. In addition, doubt was cast on the mathematical correctness of Einstein's remark, and hence the matter was disposed of as if there were no 'Bose-Einstein condensation.'*"

On November 29, 1924, Einstein himself had sent a letter to his friend Paul Ehrenfest in Leiden, where he wrote:

"*From a certain temperature on, the molecules 'condense' without attractive forces, that is, they accumulate at zero velocity. The theory is pretty, but is there also some truth in it ?*"[39]

By generalizing the calculation by the young Bengali physicist Satyendra Nath Bose[40] to massive particles, Einstein had found[41] that, for an ideal gas of Bose particles, a macroscopic fraction of these particles accumulates in the ground state below the critical temperature

$$T_{\mathrm{BEC}} = \left(\frac{2\pi \hbar^2}{1.897 m k_B} \right) n^{2/3} \tag{1}$$

At that time, the theory of phase transitions was still in its infancy, and, in his Ph.D work,[42] Uhlenbeck had argued against the BEC being a true phase transition by saying that it would not occur in a finite size system. Uhlenbeck was a graduate student of Paul Ehrenfest and, apparently, his criticism was generally accepted, even by Einstein himself.[8] In November 1937, a conference took place in Amsterdam in honor of van der Waals (Johannes Diderik van de Waals was born 100 years before, on November 23, 1837 in Leiden). Fritz London was there[7] and he must have heard discussions including Ehrenfest and Kramers about the thermodynamic limit in connection with phase transitions, also that Uhlenbeck had withdrawn his argument against BEC (see his publication with Boris Kahn, his student who was later killed by the Nazis[43]). This must be what triggered London's interest in Einstein's forgotten paper on BEC.[8]

In a message which he sent me on the September 4, 2001, Tisza made the following comment on the discovery of superfluidity:

"*The novelty of the effect became strikingly apparent in the Allen and Jones fountain effect that started London and myself on our speculative spree...*"

In his letter to *Nature*,[35] Fritz London first recalled that ^{4}He atoms were Bose particles, then that liquid ^{4}He was a quantum liquid because the quantum kinetic energy of the atoms was large, something he had explained in a previous article.[37] This large "zero point energy" was responsible for the absence of crystallization at low pressure, something which had been also noticed by Franz Simon.[44] Then London explained that, although BEC had "*rather got the reputation of having only a purely imaginary existence… it actually represents a discontinuity in the temperature derivative of the specific heat*," meaning that it was a phase transition of third order (according to the classification by Ehrenfest). Then he calculated the transition temperature T_{BEC} at which an ideal Bose gas with the same density as liquid ^{4}He would condense in Einstein's sense and he found 3.1 K, a value close to T_λ. He further noticed that the singularity in the specific heat of helium resembled the cusp predicted for BEC. He then concluded that, "*Though the λ-point resembles rather a phase transition of second order, it seems difficult not to imagine a connexion with the condensation of the Bose–Einstein statistics. The experimental values of the temperature of the λ-point and of its entropy seem to be in favor of this conception.*" Keeping this modest attitude, he also estimated that his model, "*which is so far from reality that it simplifies liquid helium to an ideal gas*," was a rough approximation which could not give quantitative agreement with experimental measurements. To a modern eye, everything looks right in this letter to *Nature*. Shortly afterwards, he expanded his letter into a longer article published the same year.[45] London's new ideas created considerable interest,[7,8] in particular from Laszlo Tisza.

Fritz London was born in Breslau (now Wroclaw in Poland) in 1900 and he had started studies in philosophy before switching to physics.[7] He was educated at the universities of Bonn, Frankfurt, Göttingen, and Munich where he graduated in 1921. Together with Walter Heitler in Zurich, he had devised the first quantum mechanical treatment of the hydrogen molecule in 1927. He then joined Schroedinger in Berlin but in 1933, when the Nazis took power, he escaped to Oxford where Lindemann found support for him till 1936. Then, he was quite happy to find a position at the Institut Henri Poincaré in Paris where he was attracted by a group of intellectuals linked to the "Front populaire" (the coalition of political parties from the French left), namely Paul Langevin, Jean Perrin, Frédéric Joliot, and Edmond Bauer.

Laszlo Tisza had arrived in Paris in 1937 for similar reasons. He was born in 1907 and he had studied in Budapest before attending Max Born's course in Göttingen. Later, he worked in Leipzig under Heisenberg and wrote his first paper with Edward Teller, just before being arrested by the Hungarian fascist government under the accusation of being a communist.[46] In 1935, he was liberated and Teller strongly recommended him to his friend Lev Landau in Kharkov. There, Tisza entered as number 5 the famous school of theoretical physics which Landau had founded.

But in March 1937, both Landau and Tisza left Kharkov. At this time, Tisza must have tried to protect himself from anti-Semitism, just like London. In September 1937, Paul Langevin and Edmond Bauer offered him a position at the Collège de France in Paris. This is where he met Fritz London; the Collège de France is about 300 m from the Institut Henri Poincaré.

Laszlo Tisza told me[47] that they liked discussing physics together during long walks. On one such occasion, London explained his ideas about BEC to him and he had soon the intuition that, if BEC took place, there should be two independent velocity fields in liquid helium. One part would have zero viscosity and zero entropy; the other part would be viscous and would carry entropy; the proportion of each fluid would be related to temperature. He sent this as a short note to *Nature* on April 16, 1938, which introduced for the first time what is now known as the "*two fluid model*".[48] He announced there more detailed publications which were presented in French by Paul Langevin at the Académie des Sciences on November 14, 1938, and indeed published in its Comptes-Rendus.[49]

On the basis of his model, Tisza solved the apparent contradiction between different types of measurement of the viscosity of helium II: in the Toronto experiment (Wilhelm, Misener, and Clark, 1935), the damping of the oscillations of the cylinder was related to the viscosity of the whole fluid while in a flow through a thin capillary (Allen and Misener, 1937) or through a thin slit (Kapitza, 1937) only the non-viscous component of the fluid could flow. He further explained in this *Nature* note that the independent motion of the two fluids allowed one to understand the fountain effect. He eventually predicted an inverse phenomenon, namely that "*a temperature gradient should arise during the flow of helium II through a thin capillary*." The latter was to be named the "thermomechanical effect" by Fritz London[45] and his brother Heinz;[50] evidence for its existence was found by Daunt and Mendelssohn in Oxford[51] and further studied by Kapitza[52] in 1941. In the following articles to the Comptes-Rendus,[49] Tisza predicted that, in helium II, heat should propagate as "*temperature waves*," another revolutionary idea. In July 1938, Tisza "*presented this prediction at a small low temperature meeting in London… and offered it to make or break* [*his*] *theory*."[53] His temperature waves were later renamed "second sound" by Landau, discovered by Peshkov in 1946 and were indeed taken as a crucial test of his theory (see below).

At least qualitatively, the 1938 papers by London and Tisza could explain all the experimental observations which had been already made at that time, namely the flow and heat conduction experiments, the heat capacity measurements, also the fast motion of films adsorbed on a wall by Rollin,[54] confirmed by Daunt and Mendelssohn.[55] But still, when

London first heard about Tisza's two fluid model, he could not believe that, in a liquid which was pure and simple, there could be two independent velocity fields;[56] this was indeed quite a revolutionary idea. Later, Tisza wrote a more elaborate version of his theory, which he submitted as two articles[57] to the Journal de Physique et du Radium on October 23, 1939, but he could not see them printed till the end of the war. In June 1940, part of Langevin's laboratory was evacuated to Toulouse, in the south part of France which was not yet occupied by the Nazi army. In another e-mail (March 17, 2005), Laszlo Tisza told me that:

"*Jacqueline Hadamard, the daughter of the mathematician Jacques Hadamard, was a member of the lab and she offered to me and my wife a ride to Toulouse. M. and Mme Hadamard traveled with their other daughter, but I had the privilege to travel as a virtual member of the Hadamard family. Just before leaving we had signed up for an American visa at the Budapest consulate without any definite plans for using it. By a fortunate coincidence the Clipper connection between Lisbon and New York started in the summer 1940 and suddenly we received air mail letters from friends and relatives in Cambridge in two days! We must have notified the Marseille consulate of our address and sometimes in October we got a telegram that our visa was authorized. After finishing all paper work we left Marseille early February 1941 for Madrid and Lisbon. Mid-March we sailed on a Portuguese boat to New York and joined friends and relatives in Cambridge. In a few months in September I was appointed instructor at MIT, to become eventually professor....*"

As for Fritz London, Frederic Joliot offered him a position of "Directeur de recherches" at the Institut Henri Poincaré in November 1938. He was strongly attached to France where his wife Edith had entered a group of painters led by André Lhote.[7] But he found it wiser to accept an offer from Paul Gross, the head of the Chemistry Department at Duke University. He could escape just in time from France, in September 1939, on the boat "Ile de France" to New York, fortunately not on the "New Amsterdam" which was destroyed by a submarine on September 3, three days after the beginning of the war.[58] In October 1939, Fritz London was teaching at Duke as a professor of theoretical chemistry. Nearly at the same time, Landau was coming out of Stalin's jails.

4. LANDAU

Lev Davidovitch Landau was born in Baku on January 22, 1908. He graduated from the Physics Department of Leningrad in 1927, at the age of 19! He then traveled thanks to a Rockefeller fellowship to

Germany, Switzerland, England, and Copenhagen where he worked with Niels Bohr. From 1932 to 1937, he was the head of a theory group in Kharkov. There, Alexander S. Kompaneets, Evgueny M. Lifshitz, Alexander I. Akhiezer, Isaak Ya. Pomeranchuk, and Laszlo Tisza formed the first core of Landau's famous school; to enter it, they had to pass the "*Teorminimum exam*." At the same time Landau was also teaching in Moscow and Kapitza invited him to come in his new Institute in 1937. However, in March 1938, Landau was arrested by the NKVD (later called KGB).[59,60] He had been accused of being one of the authors of a leaflet criticizing the Soviet regime.[60]

Kapitza had already written some letters to Stalin in order to obtain the scientific equipment he needed for his research in Moscow. After Landau's arrest, Kapitza started another fight to liberate him and eventually sent a letter to Molotov on April 6, 1939, where, as published in English by P.E. Rubinin,[29] he wrote:

"*In my recent studies on liquid helium close to the absolute zero, I have succeeded in discovering a number of new phenomena… I am planning to publish part of this work… but to do this I need theoretical help. In the Soviet Union it is Landau who has the most perfect command of the theoretical field I need, but unfortunately he has been in custody for a whole year. All this time I have been hoping that he would be released because, frankly speaking, I am unable to believe that he is a state criminal… It is true that he has a very sharp tongue, the misuse of which together with his intelligence has won him many enemies… but I have never noticed any sign of dishonest behavior… the Soviet Union and worldwide has been deprived of Landau's brain for a whole year. Landau is in poor health and it will be a great shame for the Soviet people if he is allowed to perish for nothing…*"

Three weeks later, Kapitza was summoned to the NKVD headquarters where he tried to defend Landau as much as he could in a discussion where he was asked "*Do you understand for whom you are pleading? He's a most dangerous criminal, a spy who confessed to everything….*"[61] Around 4 o'clock in the morning, it was said to him: "*All right, Kapitza, if you pledge your word for Landau, then give us a written guarantee. If anything happens, you will be held responsible*." Kapitza wrote a letter to Beria on April 26, and Landau returned to the Institute on April 28, 1939. The NKVD decision said:

"*Landau Lev Davydovitch, born in 1908 in Baku, prior to arrest professor of physics, non-Party member, and citizen of the USSR, has been convincingly exposed as a member of anti-soviet group, guilty of sabotage and of attempt to publish and disseminate an anti-soviet leaflet. However, taking into account that* (*1*) *Landau LD is a major specialist in the field of* *theoretical physics and may be useful in the future of the Soviet Science;* (*2*) *Academician Kapitza PL has consented to pledge his word for Landau LD;* (*3*) *acting on orders of the People's Commissar… 1st rank Comrade LP Beria to release Landau in the trust of Academician Kapitza; we hereby order that detainee Landau LD be discharged from custody, the investigation discontinued, and case files sent to archive… captain of State security Vizel.*"[29]

This allowed Landau to survive and to come back to work. On June 23, 1941, Kapitza[52] and Landau[62] sent two letters together for publication in the Physical Review. They were published next to each other and Landau's letter announced a more elaborate paper to be published in the Journal of Physics of the USSR.[63]

The two 1941 articles by Landau start with nearly the same sentence: "*It is well known that liquid helium at temperatures below the λ-point possesses a number of peculiar properties, the most important of which is superfluidity discovered by P.L. Kapitza.*" For Landau, superfluidity had thus been discovered by the man who had saved his life – P.L. Kapitza – and only by him. Landau continues with:

"*L. Tisza*[*2*] *suggested that helium II should be considered as a degenerate ideal Bose gas… This point of view, however, cannot be considered as satisfactory… nothing would prevent atoms in a normal state from colliding with excited atoms, i.e., when moving through the liquid they would experience a friction and there would be no superfluidity at all. In this way the explanation advanced by Tisza not only has no foundation in his suggestions but is in direct contradiction to them*".[63]

Landau *never* cited Fritz London. Here as everywhere he attributes to Tisza instead of F. London the proposal that superfluidity is a consequence of BEC. Why is it that Landau never believed in the relevance of BEC ? This is a major and quite interesting question. Moreover, why Landau needed to be so abrupt in his criticism of his former postdoc Tisza? This is a related question which is no less interesting in my opinion.

After the above introduction, Landau's article starts with a first section entitled "The quantization of the motion of liquids." Everybody considers what follows as a brilliant breakthrough in the theory of quantum liquids. He quantizes the hydrodynamics of quantum liquids and arrives to the statement "*Every weakly excited state can be considered as an aggregate of single 'elementary excitations'* " which he divides in two different categories: sound quanta which he calls "*phonons*" and which have the linear dispersion relation

$$\epsilon = cp \tag{2}$$

and elementary vortices which his friend I.E. Tamm suggested be called "*rotons*" and for which he proposes the relation

$$\epsilon = \Delta + \frac{p^2}{2\mu} \tag{3}$$

In the above equations, ϵ is an energy, p a momentum, c the sound velocity, μ an effective mass and Δ the minimum energy of rotons (later called the "*roton gap*").

Six years later,[64] Landau modified the roton spectrum into

$$\epsilon = \hbar\omega = \Delta + \frac{(p - p_0)^2}{2\mu}, \tag{4}$$

and included them as part of the phonon spectrum. But already in 1941, Landau could calculate the specific heat of liquid helium and obtained a good fit of experimental measurements by W.H. and A.P. Keesom[65] if $\Delta \approx$ 8–9 K and $\mu \approx$ 7–8 times the mass of helium atoms. In his 1941 article, Landau then claims that, for a superfluid flowing at a velocity V at zero temperature, dissipation can only result from the emission of either phonons or rotons, so that, from the conservation of energy and momentum in this process, dissipation is only possible if

$$V > V_{cp} = c \text{ (phonons) or } V > V_{cr} = \sqrt{\frac{2\Delta}{\mu}} \text{ (rotons)}. \tag{5}$$

Landau has thus introduced a possible explanation why helium II flows at a velocity which is is independent of pressure or capillary section: his "critical velocity" V_c is a property of the helium itself. However, he also notices that the value he predicts for V_c is much larger than observed in experiments and "[*left*] *aside the question as to whether superfluidity disappears at smaller velocity for another reason*."

In the next section he calculates the properties of superfluid helium at finite temperature. For this he introduces a two fluid model : he distinguishes a "*normal component*" with density ρ_n, which is made of phonons and rotons, from a "*superfluid component*" with density $\rho_s = \rho - \rho_n$ (ρ is the total density of the liquid). The superfluid component carries no entropy and moves without dissipation while the normal one is viscous and carries a non-zero entropy. The ratio ρ_s/ρ_n depends on temperature since, at $T = 0$, $\rho_s = \rho$ and $\rho_n = 0$, while, at $T = T_\lambda$, $\rho_n = \rho$. Given the values for the phonon and roton parameters which he had adjusted to fit specific heat data, Landau calculates an approximate value for T_λ (2.3 K) also in agreement with experiment.[66] He finally explains the thermomechanical effects – the fountain effect and the reverse phenomenon – and he predicts that heat should propagate as "second sound" instead of diffusing as in classical fluids.

Landau's theory is a remarkable success, and it is still in use nowadays. Its main features are common to Tisza's previous version but there is one major difference. The common features are : the existence of two independent velocity fields; the temperature variation of the two fluid densities; the non-dissipative flow of the superfluid component (but Tisza could not predict the existence of a critical velocity for this); the fact that all the entropy is carried by the normal component and the propagation of heat as a wave. When deriving the equations which describe thermomechanical effects, Landau writes : "*The formulae 6.1 and 6.4 were deduced already by H. London* (*Proceedings Royal Society 1939*) *starting from Tisza's ideas*." Let me remark that Landau cites Heinz London,[67] Fritz London's young brother, and it is very hard to believe that Landau had not noticed the work of Fritz London, whom he had met in 1932. The absence of reference to Fritz London must be intentional. He had perhaps personal reasons for this, but I have tried to understand why he never believed in the relevance of Bose Einstein condensation in the theory of superfluidity. The above sentence also means that Landau knew the existence of the two notes published in the Comptes-Rendus by Tisza[49] in 1938, which are cited by Heinz London.[67]

The major difference between Landau's theory and Tisza's is in the nature of the normal component: according to Landau it is made of "quasiparticles," a new concept he introduces to quantize the elementary excitations of quantum fluids. In contrast, Tisza thinks in terms of ideal gases and proposes that the normal component is made of the noncondensed atoms.

Shortly after the war, Peshkov did experiments to discriminate between the predictions by Landau and by Tisza.[69] Indeed, in the limit where T tends to zero and according to Landau, the second sound velocity c_2 should tend to $c/\sqrt{3}$ where c is the velocity of the ordinary sound, while Tisza predicted that c_2 should tend to zero. At the low temperature meeting which Allen organized in Cambridge in 1946, a meeting which was called "LT0" by Russell Donnelly 50 years later, Fritz London was asked to give the opening talk.[68] He explained that Peshkov's preliminary results[69] where not yet done at low-enough temperature to discriminate between Landau and Tisza, but Peshkov's experiments soon showed that Landau was right.[70] In fact, Fritz London was very critical about Landau's theory: "*an interesting attempt to quantize hydrodynamics… based on the shaky grounds of imaginary rotons*." London must have been rather upset by Landau's attitude, in particular by his rough rejection of Tisza's model. Some authors consider that the two fluid model has been *independently* discovered by Tisza and by Landau, but this is not true as we shall see now. In 1949, Landau wrote a brief report to Physical Review[71] which contains the following note:

"*I am glad to use this occasion to pay tribute to L. Tisza for introducing, as early as 1938, the conception of the macroscopical description of helium II by dividing its density into two parts and introducing, correspondingly, two velocity fields. This made it possible for him to predict two kinds of sound waves in helium II.* [*Tisza's detailed paper* (*J. de Phys. et Rad. 1, 165, 350* (*1940*) *was not available in USSR until 1943 owing to war conditions, and I regret having missed seeing his previous short letters* (*Comptes-Rendus 207, 1035 and 1186* (*1938*)).] *However, his entire quantitative theory* (*microscopic as well as thermodynamic-hydrodynamic*) *is in my opinion entirely incorrect.*"

He thus keeps his very abrupt criticism and partly justifies his former attitude by saying that he was not aware of the details of Tisza's two fluid model. But these two letters to the Comptes-rendus, which Landau pretends that he "missed," are those which H. London cited as his starting point when he derived the "formulae 6.1 and 6.4" (see above)! Since Landau refers to H. London's formulae, he had read H. London's paper and, consequently, he knew the existence of Tisza's letters to the Comptes-Rendus. Could it be then that he had not read them because they were written in French? I inquired about this possibility from A. Abrikosov who sent me the following answer by mail, on January 15, 2001:

"*Dear Dr. Balibar,*

Landau was very able to languages. He knew German, English, French and Danish. Therefore he could read Tisza's papers in French, the more so that Lifshitz, whom he often ordered to read papers, instead of doing that himself, didn't know French…

Sincerely yours. Alex Abrikosov"

Even if E. Lifshitz had perhaps not read these French papers, Landau knew their existence and it is hard to believe that he had not read them. Furthermore, Kapitza also refers to them in his 1941 article published just before the one by Landau in the Physical Review. Kapitza measured the thermomechanical effect which is the inverse of the fountain effect, namely the temperature difference which appears when superfluid helium flows in a small slit where the normal component is blocked. Kapitza uses Landau's theory which is published as the next article in the same issue. In his figure, he shows a fit with a calculation by Landau. His article was sent the same day (June 23, 1941) as Landau's, which probably means in the same envelope. It is clear that Kapitza and Landau had a very close collaboration on this subject. I cannot believe that they did not share all information, or that Landau had not read Kapitza's article which contained his own calculation. The reference by Kapitza to the two French articles by Tisza which Landau had "missed" is further evidence that Landau cannot have "missed" them. Even if Landau's theory is more rigorous and more correct than Tisza's, I consider that these two works are not *independent*, and that Tisza has a priority on the two fluid model.

Landau's absence of reference to Fritz London is a different issue of greater scientific interest. At this stage, we have to realize that Landau's 1941 work never mentions Bose nor Fermi statistics. In fact he derives his quantization of hydrodynamics without making any difference between Bose and Fermi fluids. Today, of course, we know that degenerate Fermi liquids such as liquid ^{3}He are highly viscous while degenerate Bose fluids are superfluid. It means that there is a mistake or some misunderstanding somewhere in Landau's article. Where?

After discussing this issue with Grisha Volovik,[72] I understand that the weak point occurs when Landau claims that there is a gap between irrotational states and states where the circulation of velocity is non-zero. Landau does not justify this statement. As is now well known, it is Bogoliubov[73] who showed for the first time in 1947 that in a degenerate Bose gas with weak interactions, there is BEC and there are no individual excitations at low energy, only collective modes, that is phonons with a non-zero velocity. Bogoliubov showed that if dissipation results from the emission of elementary excitations, it can only occur beyond a certain critical velocity (the sound velocity in this case), and that the motion of the condensate fraction is non-dissipative and irrotational below this critical velocity. In 1951, BEC was generalized by Penrose[74] as "off-diagonal long range order" (ODLRO) in the formalism of the density matrix. This approach was further developed by Penrose and Onsager in 1956.[75] It allows the condensate fraction to be much smaller than one (the total mass) and irrotational dissipationless motion to occur below a certain critical velocity. One has also realized that in most macroscopic systems, the emission of quantized vortices is another mechanism which is responsible for a critical velocity smaller than Landau's. In other words, the existence of superfluidity is really linked to BEC, at least to the Bose statistics and the quantization of vortices. One could argue, of course that superfluidity exists in 2D-Bose fluids, where, strictly speaking, there is no BEC. But there are long range quantum correlations so that vortices are quantized, and dissipation cannot occur in practice below a certain velocity. In summary, the superfluidity is certainly linked to the Bose statistics, contrary to Landau's statement.

As for Fermi liquids, it is in fact the hydrodynamics itself which breaks down. As Landau was to realize later,[76] the excitations of a degenerate Fermi liquid are Fermi quasiparticles which travel ballistically over a certain distance and which are responsible for the divergence of the viscosity in the low-temperature limit. The existence of an energy gap between rotational and irrotational states in quantum fluids is simply not true in Fermi liquids. This takes us back to the already mentioned question: how can it be that Landau never referred to BEC nor mentioned Bose statistics in his theory of superfluidity?

Perhaps Landau could simply not believe that a theory of quantum ideal gases (BEC) could apply to liquids with strong interactions between atoms? This is the spirit of his criticism of Tisza's approach (there should be collisions between excited atoms and condensed atoms). Furthermore, as would show up later from Bogoliubov's work,[73] it is true that an ideal Bose gas with no interactions at all would have a sound velocity equal to zero, consequently a zero critical velocity: it would not be superfluid! Eventually, we now know that there is no continuous path from a low-density helium gas to a higher density helium liquid: it has been predicted[77] and experimentally verified[78] that there is a range of densities for which helium is unstable, between two spinodal lines which, respectively, limit the range of possible metastability of either liquid or gaseous helium. For all these reasons, the most likely interpretation of Landau's absence of reference to BEC is just that he could not consider that a theory of quantum gases could apply to a liquid.

However the absence of reference to the Bose statistics needs a further explanation. In his 1992 article[59] on Landau's theory of superfluidity, Lev Pitaevskii writes that "*Landau was only one step from the very interesting subject of macroscopic quantum phenomena. But he never made this step. And there is no sense now to guess why…*" Coming back to this issue with Landau, Lev Pitaevskii proposed to me another idea which is the following. Kapitza and most probably Landau as well considered superfluidity as a phenomenon analogous to superconductivity. This was long before the BCS theory and of course superconductivity occurs in a Fermi system of electrons. Since the same phenomenon occurred in both quantum fluids (Bose and Fermi), Landau could perhaps not admit that superfluidity was related to the quantum statistics. Whatever Landau really thought, a possible comparison of ^{3}He and ^{4}He progressively appeared as a crucial test. In his book, London insisted on the importance of such a test.[38] As soon as ^{3}He was available in large enough quantities, a test was made of the possible superfluidity of ^{3}He, which was found to be non-superfluid down to 1 K, in strong support to London's and Tisza's approach. This experiment was done by Osborne, Weinstock, and Abraham[79] in 1949.

As an aside, let me mention that B. Abraham had joined the Manhattan project during the war and owned a patent for the Lithium–Tritium compound to be used in H-bombs. Let me mention further that Landau also participated to the building of the H-bomb, but the Soviet one of course, and despite the severe conflict which opposed Kapitza and Beria in this enterprise. Beria forced Kapitza to leave his scientific position and activity at the Institute for Physical Problems because of their conflict. Landau kept working for the bomb, apparently because this was a way for him to be protected against any further problems with the Soviet regime.[60] Later, Beria was assassinated and Kapitza recovered his position at the Institute for Physical Problems. When Stalin died, Landau left the Soviet H-bomb program.[60]

Coming back to superconductivity and the superfluidity of ^{3}He, we know that the BCS theory considers the condensation of Cooper pairs which obey the Bose statistics, and that superfluidity was also discovered in liquid ^{3}He at a temperature low enough (about 2.5 mK) that ^{3}He atoms could form pairs.[80,81]

As for rotons, their existence was proven by inelastic neutron scattering experiments.[82] It also happens that, for my PhD work, I studied quantum evaporation and obtained the first experimental evidence that, at low enough temperature, a heat pulse decomposes into ballistic phonons and rotons, and that individual rotons can evaporate atoms with a minimum kinetic energy of 1.5 K.[83] This phenomenon had been predicted by P.W. Anderson as an analogue of the photoelectric effect.[84] A.F.G. Wyatt and his group have performed a long quantitative study of it.[85–87] Today, there is no doubt that rotons exist, only controversies remain on their physical picture. Landau had first proposed that they were vortices of atomic size and later considered them as part of the phonon spectrum. Surprisingly, Feynman insisted on Landau's first picture by considering that a roton could be an elementary vortex loop.[88] In my opinion, rotons are phonons with a wavelength equal to the interatomic distance. Their low energy is a signature of the local order which had already been mentioned by London. As expressed by Nozières, rotons are "ghosts of a Bragg peak"[89] (in fact, this idea was already present in the work of other authors[90,91]). This is because Feynman showed that, under certain approximations, the dispersion relation $\omega(q)$ for elementary excitations is related to the static structure factor $S(q)$ of liquid helium by the simple relation

$$\hbar\omega(q) = \frac{\hbar^2 q^2}{2mS(q)}. \tag{6}$$

As explained to me by G. Volovik,[72] this relation only requires that the wavefunction describing the fluid is symmetric as it has to be for a Bose fluid. The above equation shows that, if there is some short range order in this liquid, that is a large probability to find an atom at a distance which is the average interatomic distance from another atom, in other words a large peak in the structure factor $S(q)$, then there has to be a roton minimum in the relation $\omega(q)$. One should not associate superfluidity with the existence of a roton minimum; Landau introduced rotons to calculate the specific heat of liquid helium and then explained that their existence limits the maximum value of the critical velocity. In reality rotons are precursors of solidification, and their existence works against superfluid order. In the superfluid gases which have been discovered in 1995,[2,3] there is superfluidity and no rotons because the system has weak interactions. In the superfluid liquid, an instability is predicted to occur if the roton minimum goes to zero – if rotons become soft – in which case the dispersion relation resembles that of a crystal in the extended zone scheme. The existence of such an instability is under present investigation in my research group.[92] Above the lambda temperature, rotons still exist, they are no longer well defined modes with a long lifetime but this is also true for the rest of the dispersion curve.

Landau was right in a sense (rotons exist) but wrong concerning his first interpretation or physical picture (they are not elementary vortices, nor essential for superfluidity).

5. LATER DEVELOPMENTS

In my opinion, London and Tisza had found part of the truth and Landau had found a complementary part of the truth. Unfortunately, neither London nor Landau lived long enough to realize that a full theory should combine their respective approaches. Fritz London died of a heart attack in 1954. Landau was severely injured in a car accident shortly before receiving his 1962 Nobel prize. The car accident occurred on January 7, 1960, he was in coma for a long time and suffered so much afterwards that he could never work anymore till he died in 1968. Of course he could not go to Stockholm and receive his Nobel prize in person. In my opinion, he would probably have shared this prize with London if London had not died before. London had been proposed for the Nobel prize by Einstein. A few years before arriving to the famous BCS theory with Leon Cooper and Robert Schrieffer, John Bardeen also recognized the great importance of London's work on superconductivity (the introduction of a macroscopic wave function) as the basis of his work on the same subject; in a letter sent to London on December 9, 1950, he had written:

"*Dear Prof. London*
You may be interested in the enclosed manuscripts on superconductivity; they are both based on your approach."[58]

Bardeen's admiration for London's work must be the reason why, when he received his second Nobel prize in 1972 (he shared this one with Cooper and Schrieffer for the "BCS" theory of superconductivity but he had already shared one with Schockley and Brattain in 1956 for the discovery of the transistor), he decided to donate his part of the Nobel Prize to Duke University. The purpose was to create an endowment to enable funding a yearly lecture at Duke University in the honor of Fritz London and also to finance the Fritz London Prize for distinguished work in Low-Temperature Physics. This Prize, which has become very prestigious, was given for the first time to N. Kuerti in 1958 for his work on nuclear magnetism. I was surprised to see that the second London Prize was given to Landau in 1960 (the third one was given to John Bardeen in 1962). Of course, Landau's exceptional achievements in physics deserved more than the London prize, but it means that the London prize jury totally ignored the controversies and personal conflicts which opposed London and Landau. In an e-mail which he sent me on January 21, 2001, Tisza wrote:

"*I know that Landau had no high regard for London. I think he was wrong and hurt his own science for yielding to his spite. London disliked Landau, and I did what I could to temper his feelings when writing his "Superfluids." I suspect that they had an unpleasant interaction in 1932 when Landau traveled in the West, but this will remain an unsolved mystery.*"

I am pleased to see that science is sometimes more important than personal conflicts. On June 17, 2005, I received another message from Laszlo Tisza where he commented on the London prize:

"*Dear Sebastien,*
... Yesterday I was leafing through old correspondence and I found a letter in which I nominated Landau for the Prize. I am sure I was not alone. I was actually at LT-7 in Toronto when the Prize was announced. It is actually unconscionable of Landau not to have taken note of the remarkable Simon - London work on helium in Oxford 1934–1935! I never heard a word of it while at UFTI. All he said was that London was not a good physicist. I

am looking forward to your book to straighten out matters. With warmest regards, Laszlo"

Laszlo Tisza himself supported the nomination of Landau for the London prize! He had recognized the superiority of Landau's two fluid model on his early theory and he did not want to be upset by any personal criticism which he considered as secondary. Fifty years later, he still thinks the same way.

Kapitza was awarded the Nobel prize in 1978. This was 16 years after Landau and 41 years after he had sent his historical letter to *Nature*. In his speech, he noticed this surprising delay and talked about a different subject (nuclear fusion). I do not know if the Nobel prize jury ever considered the possibility of dividing a Nobel prize on superfluidity between Kapitza and Allen. Perhaps some physicists considered that Kapitza had some priority on Allen and it was difficult to find agreement. I have already detailed my opinion about this issue.

I wish to conclude with another quotation from Tisza. At the end of his talk for the hundredth anniversary of the Hungarian physical society in 1991,[56] he wrote:

"*If history has a lesson, it is that the "winner takes all" attitude deprives one of the pleasure of being the heir to the best of different traditions, even while avoiding their intolerance against each other*."

Tisza was squeezed between London and Landau whose approaches of the theory of superfluidity were rather different. In fact London considered the ground state of liquid helium and Landau its excited states. It took quite a long time to unify their respective points of view, even after Bogoliubov's work.[73] As for Tisza, some of his theory was wrong but he had introduced many of the fundamental ideas which were later developed by Landau. Furthermore, Landau's theory was not entirely correct either. In conclusion, one should certainly attribute the discovery of the theory of superfluidity not only to London and Landau, but also to Tisza.

6. ACKNOWLEDGMENTS

I wish to congratulate Frank Pobell for his remarkable achievements in low temperature physics and for letting me take the opportunity of this special issue of the Journal of Low-Temperature Physics to recall the long and rich history of the discovery of superfluidity. I am very grateful to many colleagues for very fruitful discussions and many suggestions after careful reading of preliminary versions of this article, especially to Laszlo

Tisza, Lev Pitaevskii, Grisha Volovik, Horst Meyer, Allan Griffin, David Edwards, and Roger Bowley.

REFERENCES

1. E. M. Lifshitz, *Superfluidity*, Scientific American **198**, 30 (1958).
2. For a review, see the Nobel lecture: E.A. Cornell and C.E. Wieman, *Rev. Mod. Phys.* **74**, 875 (2002).
3. For a review, see the Nobel lecture: W. Ketterle, *Rev. Mod. Phys.* **74**, 1131–1151 (2002).
4. R. Donnelly, *Phys. Today* **48**, 30 (1995).
5. A. J. Leggett, *Rev. Mod. Phys.* **71**, S318 (1999).
6. P. Nozières and D. Pines, *The Theory of Quantum Liquids*, Perseus Books, Cambridge, Massachusetts (1999).
7. K. Gavroglu, *Fritz London*, Cambridge University Press (1995).
8. A. Griffin, "A brief history of our understanding of BEC: from Bose to Beliaev", Proc. of the Int. School of Physics Enrico Fermi, M. Inguscio, S. Stringari and C.E. Wieman (eds), IOS press (1999) p. 1.
9. A. Griffin, *Phys. Can.* **61**, 33 (2005).
10. J. Matricon and G. Waysand, La Guerre du Froid Seuil, Paris (1994) and its English translation: *The Cold Wars: A History of Superconductivity* Rutgers University Press (2003)
11. P. Kapitza, *Nature* 141 **74** (1938).
12. J. F. Allen and A. D. Misener, *Nature* **141,** 75 (1938).
13. M. Wolfke and W. H. Keesom, *Proc. Amst.* **31**, 81 (1927). [W. H. Keesom and M. Wolfke, *Leiden. Comm.* 190b, (1927)].
14. W. H. Keesom and K. Clusius, Leiden Comm. 219e (1932). [Proc. Sect. Sci. K. Ned. Acad. Wet. **35**, 307 (1932)].
15. For a review on this work, see W.H. Keesom, *Helium*, Elsevier, Amsterdam (1942).
16. W. H. Keesom and A.P. Keesom, *Physica* **3**, 359 (1936).
17. B. V. Rollin, *Physica* **2**, 557 (1935).
18. J. F. Allen, R. Peierls and Z. Uddin, *Nature* **140**, 62 (1937).
19. J. Wilks, *The properties of liquid and solid helium*, Clarendon Press, Oxford (1967).
20. E. F. Burton, *Nature* **135,** 265 (1935).
21. J. G. Dash and R. D. Taylor, *Phys. Rev.* **105,** 7 (1957); **106,** 398 (1957).
22. F. L. Andronikashvili, *Reflections on liquid helium*, American Institute of Physics, New York (1989).
23. L. Cao, D.F. Brewer, C. Girit, E.N. Smith, and J.D. Reppy, *Phys. Rev.* **B 33**, 106–117 (1986).
24. H. Meyer, Low Temperature Measurement in *Measurement of the Transport Properties of Fluids*, IUPAC series on Experimental Thermodynamics, W. A. Wakeham, A. Nagashima and J. V. Sengers (eds), Blackwell Scientific Publications, Oxford, (1991), Vol. III, p. 391.
25. J. O. Wilhelm, A. D. Misener and A. R. Clark, *Proc. R. Soc.* **A 151,** 342 (1935).
26. J. C. McLennan, H. D. Smith and J. O. Wilhelm, *Phil. Mag.* **14**, 161 (1932).
27. A. Griffin, Austin Donald Misener 1911–1996, *Proc. R. Soc. Can.*, sixth series (2001), vol. XII, p. 223 and private communication to be published.
28. D. Shoenberg, *Kapitza centenary symposium at the Cavendish laboratory, Cambridge, 8 July 1994*, Physics-Uspekhi **37**, 1213 (1994).
29. P. E. Rubinin, *The discovery of superfluidity*, *Physics-Uspekhi* **40**, 1249 (1997).
30. J. F. Allen, *Phys. World* **1**, 29 (1988).
31. In 1951, Sir John Douglas Cockcroft and Ernest Thomas Sinton Walton received the Nobel prize "*for their pioneer work on the transmutation of atomic nuclei by artificially accelerated atomic particles*". The presentation speech mentioned that "*The analysis made by Cockcroft and Walton of the energy relations in a transmutation is of particular interest, because a verification was provided by this analysis for Einstein's law concerning the*

equivalence of mass and energy". By accelerating protons and analyzing their collisions with a lithium layer, they observed the transmutation of Li into He atoms whose energy measurement provided the first experimental verification of the famous relation $E = mc^2$.
32. W. H. Keesom and J. N. van der Ende, *Proc. R. Ac.* Amsterdam, **33**, 24 (1930).
33. D. Shoenberg, private lett, 22 Jan. 2001.
34. F. London and H. London, *Physica* **2**, 341 (1935). [F. London, *Proc. R.* **A 152,** 24 (1935)].
35. F. London, *Nature* **141**, 643 (1938).
36. J. F. Allen and H. Jones, *Nature* **141**, 243 (1938).
37. F. London, *Proc. R. Soc.* **A 153,** 576 (1936).
38. F. London, *Superfluids I*, Wiley, NY P.4 (1950).
39. A. Pais, *Subtle is the Lord*, p.432, Clarendon Press, Oxford (1982).
40. S. N. Bose, *Z. Phys.* **26**, 178 (1924).
41. A. Einstein, *Ber. Berl. Akad.* 261 (1924); 3 (1925).
42. G. E. Uhlenbeck, Dissertation, Leiden, (1927).
43. B. Kahn and G. E. Uhlenbeck, *Physica* **5**, 399 (1938).
44. F. Simon, *Nature* **133**, 529 (1934).
45. F. London, *Phys. Rev.* **54**, 947 (1938).
46. E. Teller, *Science* **280**, 1200 (1998).
47. In June 2001 I invited Tisza to give a colloquium in our Department and we had long discussions about his stay in France (1937–1941).
48. L. Tisza, *Nature* **141,** 913 (1938).
49. L. Tisza, *Comptes Rendus Acad. Sc.* **207**, 1035; 1186 (1938).
50. H. London, *Nature* **142,** 612 (1938).
51. J. G. Daunt and K. Mendelssohn, *Nature* **143,** 719 (1939).
52. P. L. Kapitza, *Phys. Rev.* **60**, 354 (1941).
53. L. Tisza, e-mail to S. Balibar, September 12, (2000).
54. B. V. Rollin, *Actes 7ième Cong. Int. du Froid*, **1**, 187 (1936); N. Kuerti, B. V. Rollin, and F. Simon, *Physica* **3,** 266 (1936).
55. J. G. Daunt and K. Mendelssohn, *Nature* **141**, 911 (1938).
56. L. Tisza, "*The History of the two-fluid concept*", Centenary meeting of the Eötvös Society (Budapest, Hungary, Oct. 19, 1991). In his communication at this meeting, L. Tisza wrote: "*I had this idea one evening… When I presented all this to London the next morning, he was unimpressed… I made the minor prediction that the thermomechanical effect ought to have an inverse… This was readily verified. However, London persisted in his opposition to the idea that two velocity fields could persist in a liquid…*".
57. L. Tisza, J. *Physique et le Radium* **1,** 164 (1940); 1, 350 (1940).
58. H. Meyer, communication at the conference "Quantique… mais macroscopique, Hommage à Fritz London, physicien en exil", Institut Henri Poincaré, Paris, 11 mai 2005.
59. L. Pitaevskii, *50 years of Landau's theory of superfluidity, J. Low Temp. Phys.* **87,** 127 (1992).
60. G. E. Gorelik, *The top secret life of Lev Landau*, (1997), pp. 72–77.
61. Account of this visit recorded by I.A. Zolitov, quoted in ref. 29.
62. L. D. Landau, *Phys. Rev.* **60**, 356 (1941).
63. L. D. Landau, *J. Phys. USSR* **5**, 71 (1941).
64. L. D. Landau, *J. Phys. USSR* **11**, 91 (1947).
65. W. H. Keesom and A. P. Keesom, *Physica* **2**, 557 (1935).
66. However, note that when writing that "*the temperature at which ρ_n/ρ equals unity is the λ-point of helium*", Landau does not necessarily mean that there is a real phase transition at this point, it could be just a crossover from a quantum to a classical behavior.
67. H. London, *Proc. Roy. Soc.* **A 171**, 484 (1939).
68. F. London, *Rep. of an Int. Conf. on Fund. Part. and Low Temp.*, Cavendish Lab., Cambridge 22-27 July 1946, p. 1 Taylor and Francis, London (1947) reprinted by R. Donnelly, Dept. of Physics, University of Oregon (1993).

69. V. P. Peshkov, *Rep. Int. Conf. Fund. Part. and Low Temp.*, Cavendish Lab., Cambridge 22-27 July 1946, p. 19 Taylor and Francis, London (1947) reprinted by R. Donnelly, Dept. of Physics, University of Oregon (1993).
70. V. P. Peshkov, *Zh. Eksp. Teor. Fiz.* **18**, 951 (1948).
71. L. Landau, *Phys. Rev.* **75**, 884 (1949).
72. G. Volovik, oral discussion at the ULTI meeting, Lammi, Finland, (2006).
73. N. N. Bogoliubov, *J. Phys. USSR* **11,** 23 (1947).
74. O. Penrose, *Phil. Mag.* **42**, 1373 (1951).
75. O. Penrose and L. Onsager, *Phys. Rev.* **104,** 576 (1956).
76. L. D. Landau, *Sov. Phys. JETP* **3**, 920 (1956).
77. H. J. Maris, *J. Low Temp. Phys.* **94**, 125 (1994); **98**, 403 (1995).
78. F. Caupin and S. Balibar, *Phys. Rev.* **B 64**, 064507 (2001).
79. D. W. Osborne, B. Weinstock, and B.M. Abraham, *Phys. Rev.* **75,** 988 (1949).
80. D. D. Osheroff, R. C. Richardson and D. M. Lee, *Phys. Rev. Lett.* **28,** 885 (1972) [Nobel Lecture, *Rev. Mod. Phys.* **69**, 667 (1997)].
81. A. J. Leggett, *Phys. Rev. Lett.* **29**, 1227 (1972) [Nobel Lecture, *Rev. Mod. Phys.* **76,** 999 (2004)].
82. D. G. Henshaw and A. D. B. Woods, *Phys. Rev.* **121**, 1266 (1961).
83. S. Balibar, J. Buechner, B. Castaing, C. Laroche and A. Libchaber, *Phys. Rev.* **B 18,** 3096 (1978).
84. P. W. Anderson, *Phys. Lett.* **A 29**, 563 (1969).
85. F. R. Hope, M. J. Baird, and A. F. G. Wyatt, *Phys. Rev. Lett.* **52,** 1528 (1984).
86. M. Brown and A. F. G. Wyatt, *J. Phys.*: *Condens. Matter* **2,** 5025 (1990).
87. M. A. H. Tucker and A. F. G. Wyatt, *Science* **283**, 1150 (1999).
88. R. P. Feynman, Prog. in Low Temp. Phys. C.G. Gorter ed., North Holland Publishers, Amsterdam (1955), vol. 1
89. P. Nozières, *J. Low Temp. Phys.* **137,** 45 (2004).
90. H. Horner, *Phys. Rev. Lett.* **29,** 556 (1972).
91. Y. Pomeau and S. Rica, *Phys. Rev. Lett.* **72**, 2426 (1994).
92. R. Ishiguro, F. Caupin, and S. Balibar, *Europhys. Lett.* **75,** 91 (2006) and references therein.