Superfluidity: how quantum mechanics became visible.

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Abstract :

In December 1937, J.F. Allen and A.D. Misener in Cambridge and simultaneously P. Kapitsa in Moscow discovered the superfluidity of liquid helium. In March 1938, F. London proposed that superfluidity was a consequence of a quantum phenomenon called "Bose-Einstein condensation" (BEC). This was a major step in Physics because, if London was right - and it is now accepted that he was right - quantum mechanics had to be at play at the macroscopic scale of our visible world, not only at the microscopic scale of atoms or molecules.

This major discovery was made possible by the progress of low temperature techniques, especially the construction of helium liquefiers. London's ideas were soon developed by L. Tisza who invented the "two fluid model" to explain most of the helium properties that were known at that time. In 1941, L.D. Landau made further progress in the understanding of superfluidity but, surprisingly, he never agreed with London and Tisza on the possible relation of superfluidity to BEC. Among these five great physicists, only Landau and Kapitsa received a Nobel Prize.

The history of this discovery is quite interesting because it illustrates the way how modern science progresses, especially how controversies could be solved, also because this discovery was made at a time when the world was torn apart by conflicts and wars. Seventy years later, superfluidity has been found in several other quantum fluids. It appears as closely related to superconductivity, another macroscopic property of quantum matter, and superfluid helium can be used to cool down matter at an industrial scale.

1- Introduction

Why is it that physicists keep trying to study matter at lower and lower temperature? One likely explanation is that, as temperature goes down, thermal fluctuations progressively vanish so that new phenomena appear as a landscape when clouds go up. When approaching the absolute zero (0 Kelvin = -273.15 degrees Celsius) the behaviour of matter becomes sensitive to minute interactions that would be irrelevant at higher temperature when fluctuations are larger. This is how many new properties of matter have been discovered, not only superfluidity and superconductivity.

In 1908, Kammerlingh Onnes succeeded in liquefying helium gas (Kammerlingh 1908). This was actually natural helium, which comes from oil wells. It contains mostly 4He, which results from the α-decay of Uranium in the Earth. The light isotope 3He was not available in large enough quantities until Tritium, which decays into 3He, was used in the military nuclear industry after World War II. Kammerlingh Onnes wanted to see if it was possible to liquefy the last gas that had not yet been liquefied. The transition from gaseous 4He to liquid 4He occurred at 4.2K under atmospheric pressure. In 1911 and at the same temperature (4.2K) he made the major discovery for which he received his Nobel prize, that is the superconductivity of mercury (Kammerlingh 1911): at low temperature, mercury is a metallic solid and he discovered that its electrical resistance vanishes below 4.2K (see the article by G. Waysand in this book).

In order to reach temperatures lower than 4.2K, Kammerlingh Onnes simply pumped on liquid helium. This time, one could say that he really entered the era of artificial cold because the lowest temperature in the Universe is that of the cosmic background radiation, now known as 2.7 K (Fixsen 2009).

It is only a little further down in temperature - at -2.2K and in December 1937 - that superfluidity was discovered simultaneously by Allen and Misener in Cambridge (Allen 1938a) and by P. Kapitsa in Moscow (Kapitsa 1938).

Superconductivity and superfluidity are far from being the only two phenomena that have been discovered below 3 K, but they appear as the most important manifestations of quantum mechanics at the macroscopic scale. One can see with a naked eye that liquid helium is a normal liquid down to 2.17 Kelvin (- 271 Celsius) and that it changes into a different liquid below this temperature (Fig. 1). When pumping on any liquid, one reduces its vapour pressure so that its temperature goes down. Since the thermal conductivity of classical liquids is usually poor, the surface is usually colder than the inside, especially the walls of the container where the probability that bubbles nucleate becomes high. As result, a classical liquid is invaded by bubbles which have grown on nucleation sites before being driven up by the buoyancy force. The result is the turbulent mixture of liquid and gas that everyone has in mind when talking about a boiling liquid. This is exactly what happens if one pumps on normal liquid helium above 2.17K. But as soon as it is cooled below this temperature, boiling stops and the liquid only evaporates from the free surface, without any bubble formation (see Fig.1). This property was discovered in 1932 by J.C. McLennan (McLennan 1932) at the Toronto University. Some years later, one realized that this phenomenon is due to the unusually high thermal conductivity of superfluid helium below 2.17K.

It is really London who understood for the first time that quantum mechanics shows up at the macroscopic scale, not only at the scale of atoms and molecules. When proposing that superfluids and superconductors could be represented by a single macroscopic wave function he made a giant step forward. More precisely he explained that this wave function had to be related to « Bose Einstein condensation », an astonishing phenomenon. This was in 1938. It took 20 Years to the famous trio BCS (John Bardeen, Leon Cooper and Robert Schrieffer) to understand in details that a similar condensation was taking place in superconductors so that these two phenomena were close to each other, as first imagined by London. Today, one usually considers that superconductivity is superfluidity in a charged fluid.

If necessary, one could judge the importance of superfluidity and superconductivity from the number of Nobel Laureates whose work was dealing with superfluidity (Landau in 1962, Kapitsa in 1978, Lee, Osheroff and Richardson in 1996, Cornell, Wieman, and Ketterlee in 2001, Leggett in 2003) and superconductivity (Kammerlingh Onnes in 1913, Barden, Cooper, Schrieffer in 1972, Giaever and Josephson in 1973, Bednorz and Mueller in 1987, Abrikosov and Ginzburg in 2003).

In this article, I wish to recall how superfluidity was discovered experimentally in liquid helium 4. Then how it was progressively understood. I leave most of the history of superconductivity to G. Waysand in his contribution to this book. In a last part I describe shortly some important developments after the discovery : the quantization of vortices (1949-61) the discovery of superfluid helium 3 (1972) , the superfluidity and the very clear evidence for Bose Einstein condensation in quantum gases (1995), and most recently supersolidity.

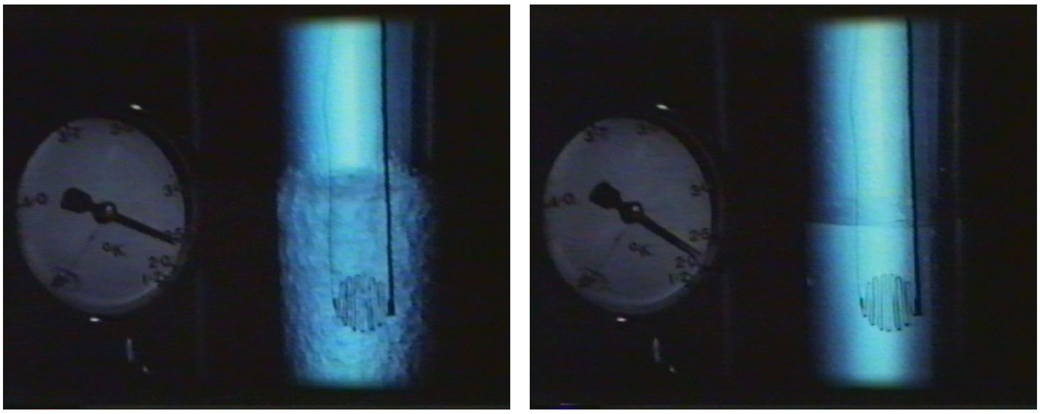

Fig. 1. As shown by these two images from a film by J.F. Allen and J.M.G. Armitage (Allen 1982), superfluid helium stops boiling below Tλ . This is due to its large thermal conductivity. The left picture is taken at 2.4 K as indicated by the needle of the thermometer on the left. The right picture is taken just below the lambda transition.

At the very end, I briefly describe some very important applications of superfluidity and superconductivity, mainly very high field magnets leading to a revolution in medical imaging, and the impressive presence of superconductivity and superfluidity in the 27 km long LHC near Geneva.

2- Before the discovery of superfluidity.

This discovery has already been described by various authors with various points of view (Donnelly 1995, Balibar 2007, Griffin 1995 and Griffin 2005, Rubinin 1997, Matricon 2003). Before describing it here, let us mention some of its prehistory and present the two main characters involved in the experimental discovery (Kapitsa and Allen).

In 1927 at Leiden, M. Wolfke and W.H. Keesom (Wolfke 1927, Keesom 1927) measured the specific heat of liquid helium as a function of temperature and they found that this quantity had a singularity in the shape of the Greek letter lambda (λ). This singularity had to separate two different liquid states and this observation must have been rather astonishing: how could it be that a liquid made of simple spherical atoms with no chemistry - only weak van der Waals long range attraction and a hard core repulsion at short distance - could liquefy in two different phases? Anyhow, W.H. Keesom called "helium I" the liquid above Tλ and "helium II" the mysterious liquid below Tλ. It took ten years to realize – a little more to understand - that helium II was a fundamentally new state of matter.

Some years later, another striking property was discovered, this time by J.C. McLennan in Toronto (McLennan 1932) : when pumping on liquid helium I, one could see it boiling as any classical fluid would do. But below Tλ, liquid helium II was not boiling (Fig. 1). When any of us, researchers in low temperature physics, see this boiling stopping, it is some kind of evidence that quantum order sets in. We now know what it is: the quantum coherence of a macroscopic standing wave of matter.

In 1935 at the Toronto University Physics Department directed by E.F Burton, an undergraduate student named Don Misener attempted to measure the viscosity of helium II with two technicians (J.O. Wilhelm who had already observed the absence of boiling with McLennan, and A.R. Clark). Burton actually published the results in Nature without the authors of the measurements (Burton 1935, Wilhelm 1935). Anyhow, Wilhelm, Misener and Clark measured the damping of the oscillations of a cylinder immersed in liquid helium and discovered that the viscosity of liquid helium dropped down as the system cooled below Tλ. Apparently, helium II had surprising mechanical and thermodynamical properties.

At the same time, a high thermal conductivity was found by B.V Rollin in Oxford (Rollin 1935), followed by W.H. Keesom and his daughter Ania (Keesom 1936) and by J.F. Allen, R. Peierls and Z. Uddin in Cambridge (Allen 1937). As far as I know, Toronto, Cambridge, Oxford, Leiden and Kharkov were the only five places in the world where liquefiers produced liquid helium for such studies. It was not yet available in Moscow for reasons that will become clear after introducing Piotr Kapitsa and Jack Allen.

Piotr Leonidovith Kapitsa had graduated as an electrical engineer in Saint Petersburg under the supervision of F.Ioffe (1918). In 1921, Ioffe suggested that Kapitsa 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 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 (Shoenberg 1994). 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 Kapitsa 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 (Rubinin 1997).

In the summer of 1934, Kapitsa went back to Leningrad where he could see his mother and participate in a symposium celebrating the centenary of Mendeleiev. However, on September 24, 1934, five months only after the first operation of his liquefier in Cambridge, he was not allowed to return to England from the Soviet Union (Rubinin 1997). The reasons for this are a little unclear but, according to D. Shoenberg (Shoenberg 1994), "*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 Kapitsa for his nuclear program and conflicts with Beria triggered Kapitsa's disgrace. But in 1934, Kapitsa started a fight with Stalin and Molotov to obtain support for his research. Two years later, the ``Institute for Physical Problems" was built for Kapitsa 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 Kapitsa was that Cambridge had kept his precious liquefier. But Kapitsa 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 one, which produced liquid helium on February 22, 1937 (Rubinin 1997). Meanwhile, Cambridge had used Kapitsa'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 (Allen 1988).

John Franck (``Jack") Allen was born in Winnipeg (Canada) and he had obtained his PhD on superconductivity in Toronto (1933). Then, he tried to join Kapitsa in Cambridge but when he arrived in the fall of 1935, Kapitsa was already detained in USSR. In 1936, he attracted A.D. Misener to work towards a PhD degree in Cambridge with him. We thus realize that Kapitsa 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 (Rubinin 1997) in March 1935.

3- Who discovered superfluidity ?

On January 8th, 1938 *Nature* published two articles side by side (Allen 1938a, Kapitsa 1938). The first one, on page 74, received December 3, 1937, was entitled: "*Viscosity of liquid helium below the lambda point*" and signed by P. Kapitsa (Institute for Physical Problems, Moscow). The second one, entitled "*Flow of liquid Helium-II*" (page 75), received December 22, 1937, and signed by J.F. Allen and A.D. Misener (Royal Society Mond Laboratory, Cambridge, UK).

In 1937, Kapitsa tried to understand why the thermal conductivity of helium II was anomalously large. An explanation could be that convection in this liquid was important if its viscosity was small, as already proposed by Wilhelm, Misener and Clark. He thus tried to measure this viscosity by flowing liquid helium from a little reservoir through a slit about 0.5 micrometres thick, between two polished cylinders pressed against each other.

In his article (Kapitsa 1938), Kapitsa writes:

"The flow of liquid above the lambda-point could be only just detected over several minutes, while below the lambda-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. Kapitsa 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 I understand that 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 on the size of the flow as reviewed in the book by Wilks, page 391 (Wilks 1967). As far as I know, Kapitsa'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, Kapitsa finally proposes that:

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

This is a famous sentence where Kapitsa introduces the word ``superfluid" for the first time. His intuition was quite remarkable because superfluids and superconductors are indeed analogous states of matter, but Kapitsa wrote this sentence long before the BCS theory of superconductivity was established, *a fortiori* before any demonstration of this 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 Kapitsa in the letter above; namely that, due to the high Reynolds number involved, the measurements probably represent non-laminar flow".

The note [1] refers to the article by E.F. Burton (Burton 1935) on the work by Wilhelm, Misener and Clark, which I mentioned above. More important for our present purpose is the reference to Kapitsa at the beginning of the article by Allen and Misener. We understand that they had read Kapitsa'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 Kapitsa had some priority on Allen and Misener in the experimental discovery of superfluidity (Andronikashvili 1989). 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 $6x10^{-4}$ and 0.8 mm^2. Measurements are given at two different temperatures (1.07 and 2.17K) 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. 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 Kapitsa'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 (Griffin 2006).

When Kapitsa sent his letter to *Nature*, he wrote in the accompanying letter to the editor:

``I am sending herewith a short note: `Viscosity of liquid helium below the lambda-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 (Allen 1988) and by Shoenberg (Shoenberg:1994), it was John Cockcroft who took care of the proof-reading. He was the new director of the Mond Laboratory since Kapitsa had left. In 1951, he received the Nobel prize for having verified Einstein's famous relation $E=mc^2$. In December 1937, he showed Kapitsa'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 Kapitsa's work in Moscow. 19 days delay in the article submission is not a serious reason to doubt of that.

Now, was Kapitsa's work independent of the Cambridge work? After all, Kapitsa'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. Was Kapitsa aware of Keesom's work

(Keesom 1930) on the ability of helium II to flow through narrow slits (the existence of ``superleaks")? Could Kapitsa have written his letter *after* hearing of the progress made by Allen and Misener in Cambridge?

I have carefully inquired about this possibility (Balibar 2007). It appears that there were frequent contacts between Kapitsa in Moscow and his friends in Cambridge. Letters from one place took about one week to arrive at the other place. Kapitsa probably knew that he was competing with Allen and Misener but it is unlikely that he knew Allen and Misener's results. Anyhow, in December 1937, when Rutherford died, Kapitsa sent a letter to Niels Bohr, which proves that Kapitsa was already working on this problem in June 1937. In my opinion, there is no real priority in either way, the two works are independent. But they are not equivalent as we shall see now.

Let us 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 lambda-point. Then, in December 1937, Kapitsa 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 $T\lambda$, the hydrodynamics of helium required a totally new interpretation. Here is the real experimental breakthrough. At that time, everyone else kept considering that liquid helium was a liquid with a small viscosity. In 1941, Kapitsa published another article (Kapitsa 1941) where he kept considering the viscosity of superfluid (he found it at least 100 times lower than in 1938).

It would be very interesting to understand how Kapitsa 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 (London 1935) 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 at all the way how one liked to think about liquid helium in Moscow.

4- Who initiated the understanding of superfluidity?

In my opinion, it was Fritz London and Laszlo Tisza, followed by Lev Landau.

4a- London and Tisza.

Fritz London was born in Breslau (now Wroclaw in Poland) in 1900 and he had started studies in philosophy before switching to physics (Gavroglu 1995). 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 until 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 nazi government under the accusation of being a communist (Teller 1998). 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 that 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 meters from the Institut Henri Poincaré.

London's first ideas on superfluidity (London 1938) were triggered by the next article published by Allen in the same volume 141 of *Nature* on February 5, 1938 (Allen 1938b). 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 vapour 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 (London 1936), Fritz London had proposed that helium II was more ordered than helium I. This was probably because its specific heat decreased sharply below $T\lambda$. Perhaps it had some kind of crystalline structure 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 $T\lambda$ (London 1938).

In the introduction of his first book (London 1950), 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?"* (Païs 1982). By generalizing a calculation by the young Bengali physicist Satyendra Nath Bose (Bose 1924) to massive particles, Einstein had found (Einstein 1925) that, for an ideal gas of Bose particles, a macroscopic fraction of these particles accumulates in the ground state below a certain critical temperature. At that time, the theory of phase transitions was still in its infancy, and, in his PhD work, Uhlenbeck had argued against the BEC being a true phase transition by saying that it would not occur in a finite size system (Uhlenbeck 1927). Uhlenbeck was a graduate student of Paul Ehrenfest and, apparently, his criticism was generally accepted, even by Einstein himself (Griffin 1999). But in November 1937, a conference took place in Amsterdam in honour of van der Waals (Johannes Diderik van de Waals was born hundred years before, on November 23, 1837 in Leiden). Fritz London was there (Gavroglu 1995) 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. This must be what triggered London's interest in Einstein's forgotten paper on BEC (Griffin 1999).

In a message that he sent me on the September 4th, 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 (London 1938), Fritz London first recalled that 4He atoms were Bose particles, then that liquid 4He was a quantum liquid because the quantum kinetic energy of the atoms was large, something he had explained in his previous article (London 1936). This large ``zero point energy" was responsible for the absence of crystallization at low pressure, something which had been also noticed by Franz Simon (Simon 1934). 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 at which an ideal Bose gas with the same density as liquid 4He would condense in Einstein's sense and he found 3.1 K, a value close to Tλ. He further noticed that the singularity in the specific heat of helium resembled the cusp predicted for BEC. He then concluded that, *" Though the lambda-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 lambda-point and of its entropy seem to be in favour 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 (London 1938b).

London's new ideas created considerable interest (Gavroglu 1995, Griffin 1999}, in particular from Laszlo Tisza. Laszlo Tisza told me (Tisza 2001) 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 (Tisza 1938a), which introduced for the first time what is now known as the " two fluid model" (Tisza 1938a). He announced there more detailed publications which were presented in French by Paul Langevin at the Académie des Sciences in Paris on November 14th, 1938, and indeed published in its Comptes-Rendus (Tisza 1938b).

On the basis of his model, Tisza solved the apparent contradiction between different types of measurements 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 1937) or through a thin slit (Kapitsa, 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 (London 1938b) and his brother Heinz (H.London 1938); evidence for its existence was soon found by Mendelssohn and Daunt in Oxford (Mendelssohn 1938) and further studied by Kapitsa (Kapitsa 1941). In the following articles to the Comptes-Rendus (Tisza 1938b), 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"* (Tisza 2000). 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 (Rollin 1936), confirmed by Daunt and Mendelssohn (Daunt 1938). 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 (Tisza 1991). This was indeed quite a revolutionary idea. Later, Tisza wrote a more elaborate version of his theory, which he submitted as two articles (Tisza 1940) 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 airmail 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 (Gavroglu 1995). 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 (Meyer 2005). 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, and it is time now to introduce the fifth important character in our history.

4b- 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 travelled 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. At the same time Landau was also teaching in Moscow and Kapitsa invited him to come in his new Institute in 1937. However, in March 1938, Landau was arrested by the NKVD, later called KGB (Pitaevskii 1992, Gorelik 1997). He had been accused of being one of the authors of a leaflet criticizing the Soviet regime (Gorelik 1997).

Kapitsa 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, Kapitsa 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 (Rubinin 1997), 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 behaviour… 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, Kapitsa was summoned to the NKVD headquarters where he tried to defend Landau as much as he could in a very hard discussion. Around 4 o'clock in the morning, it was said to him: *"All right, Kapitsa, if you pledge your word for Landau, then give us a written guarantee. If anything happens, you will be held responsible"* (Rubinin 1997). Kapitsa wrote a letter to Beria on April 26, and Landau returned to the Institute on April 28, 1939.

This allowed Landau to survive and to come back to work. On June 23, 1941, Kapitsa (Kapitsa 1941) and Landau (Landau 1941a) 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 (Landau 1941b).

The two 1941 articles by Landau start with nearly the same sentence: *"It is well known that liquid helium at temperatures below the lambda-point possesses a number of peculiar properties, the most important of which is superfluidity discovered by P.L. Kapitsa".*

For Landau, superfluidity had thus been discovered by the man who had saved his life - P.L. Kapitsa - and only by him. Landau continues with :

"L. Tisza 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". (Landau 1941b)

Landau *never* cited Fritz London. Here as everywhere he attributes to Tisza instead of F. London the proposal that superfluidity is a consequence of Bose-Einstein condensation. 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 elementary vortices which his friend I.E. Tamm suggested be called "*rotons"* .

Six years later (Landau 1947), Landau modified the roton spectrum 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 (Keesom 1935). In his 1941 article, Landau then proposes 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 the fluid velocity V is larger than a critical velocity v_c, which is the phase velocity of rotons and today known as "Landau's critical velocity".

Landau has thus introduced a possible explanation why helium II flows at a velocity that is independent of pressure or capillary section: his critical velocity 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"*, which is made of phonons and rotons, from a *"superfluid component"*. The superfluid component carries no entropy and moves without dissipation, while the normal one is viscous and carries a non-zero entropy. The ratio of the respective densities of the two components depends on temperature since, at T = 0, all the density of the fluid is superfluid while, at the lambda point the superfluid component disappears and all the fluid is normal. Given the values for the phonon and roton parameters, which he had adjusted to fit specific heat data, Landau calculates an approximate value for the lambda point temperature (2.3K) also in agreement with experiment. 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 it); 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 (H.London 1939), 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 (Tisza 1938b) in 1938, which are cited by Heinz London (H.London 1939).

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 non-condensed atoms. Shortly after the war, Peshkov did experiments to discriminate between the predictions by Landau and by Tisza (Peshkov 1946). Indeed, according to Landau, the second sound velocity should increase as temperature tends to zero, while Tisza predicted that it vanishes. At the low temperature meeting, which Allen organized in Cambridge in 1946, Fritz London was asked to give the opening talk (London 1946). He explained that Peshkov's preliminary results (Peshkov 1946) where not yet done at low enough temperature to discriminate between Landau and Tisza, but Peshkov's experiments soon showed that Landau was right (Peshkov 1948). 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. I have demonstrated that Landau knew Tisza's work, whatever he declared later (Balibar 2007).

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 3He 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?

I think 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. It is the later work of Bogoliubov (Bogoliubov 1947), which showed for the first time 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 (Penrose 1951) 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 (Penrose 1956). 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 (Landau 1956), 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 (Bogoliubov 1947), 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 by H.J. Maris (Maris 1995) and experimentally verified in our research group (Caupin 2001) 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 (Pitaevskii 1992) 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. Kapitsa 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 3He and 4He progressively appeared as a crucial test. In his book, London insisted on the importance of such a test (London 1950). As soon as 3He was available in large enough quantities, a test was made of the possible superfluidity of 3He, 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 D.W. Osborne, B. Weinstock and B.M. Abraham in 1949 (Osborne 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 Kapitsa and Beria in this enterprise. Beria forced Kapitsa 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 (Gorelik 1997). Later, Beria was assassinated and Kapitsa recovered his position at the Institute for Physical Problems. When Stalin died, Landau left the Soviet H-bomb program (Gorelik 1997).

Coming back to superconductivity and the superfluidity of 3He, 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 3He at a temperature low enough (about 2.5 mK) that 3He atoms could form pairs (Osheroff 1972, Leggett 1972).

As for rotons, their existence was proven by inelastic neutron scattering experiments (Henshaw 1961) 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 (Balibar 1978). This phenomenon had been predicted by P.W. Anderson as an analogue of the photoelectric effect (Anderson 1969). A.F.G. Wyatt and his group have performed a long quantitative study of (Hope 1984, Brown 1990, Tucker 1999). 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 (Feynman 1955). 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" (Nozières 2004). This is because Feynman showed that, under certain approximations, the dispersion relation for elementary excitations is related to the static structure factor of liquid helium by the simple relation. As a consequence, 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 distance from another atom, in other words a large peak in the structure factor, then there has to be a roton minimum in the dispersion relation of elementary excitations. 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 (Cornell 1995, Ketterle 1995) there is superfluidity and no rotons because the system has weak interactions and consequently no short range order. 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).

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. I believe that he could have shared this Prize with London if London had not died before. Einstein had proposed London for the Nobel Prize. 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" (Meyer 2005)

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 honour 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 travelled 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 1934-35! 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"

It is remarkable that 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 was never upset by any personal criticism which he considered as secondary.

Kapitsa 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 Kapitsa and Allen. Some authors (Griffin 2008) believe that Kapitsa refused sharing it with Allen, which explains the 41 years delay, but I doubt that the Nobel committee asks potential laureates about their opinion. If some physicists considered that Kapitsa had some priority on Allen so that it was difficult to find agreement, I believe that this was unfair.

At the end of his talk for the hundredth anniversary of the Hungarian physical society in 1991 (Tisza 1991), Tisza 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 (Bogoliubov 1947). 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.

5- Conclusion: more recent developments and discoveries, applications.

The discovery of quantized vortices is certainly one of the most important developments in the study of superfluidity. It had been predicted by Onsager first (Onsager 1949), followed by Feynman (Feynman 1955). It is a direct consequence of the existence of a macroscopic wave function: the superfluid velocity is the gradient of its phase so that, after integration, one finds that the circulation is an integer number of quanta h/m where h is Planck’s constant and m the helium mass. It is the strict equivalent of the quantization of vortices in superconductors, which was predicted by F. London (London 1950). It explains why superfluids resist to rotation as superconductors resist to the penetration by a magnetic field. The vortex quantization was measured first by W.F. Vinen (Vinen 1961). Quantized vortices were later imaged by Yarmchuk et al. (Yarmchuk 1979). As shown by Fig.2, the same array of quantized vortices was observed in the case Bose-Einstein condensates of Rubidium atoms by Madison et al. (Madison 2000).

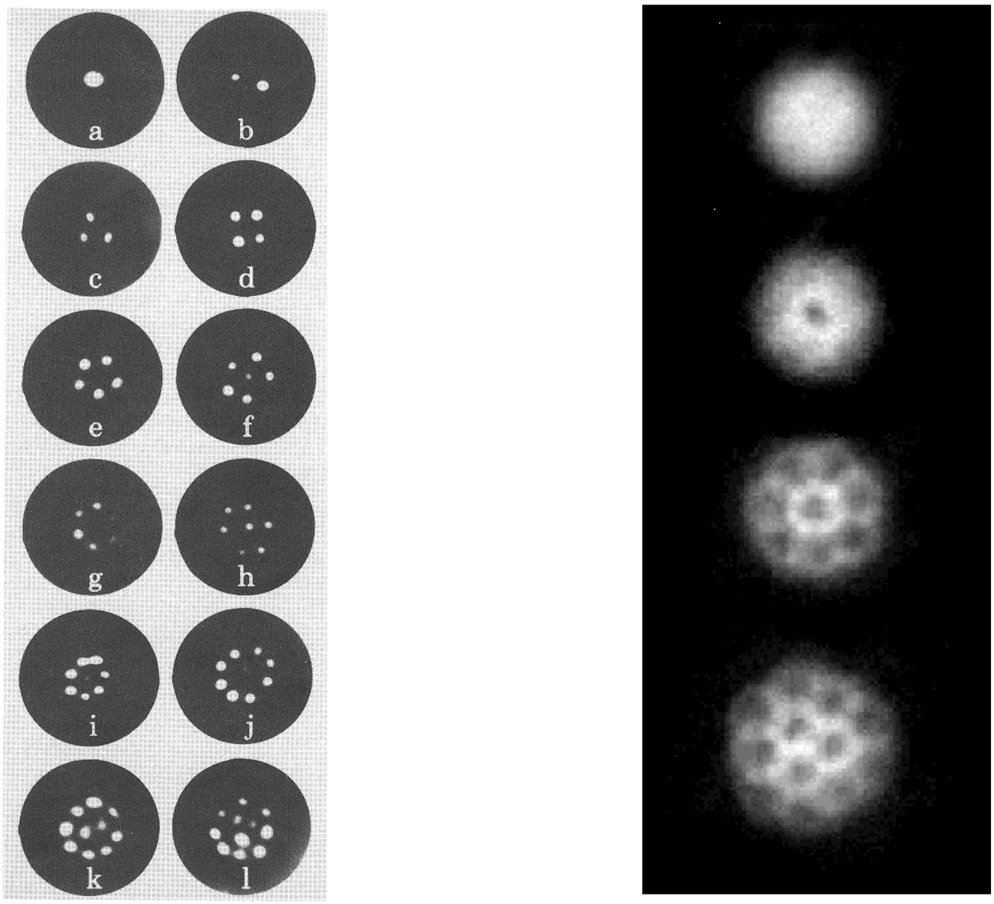

Figure 2. Images of quantized vortices in liquid helium 4 on the left (Yarmchuk 1979) and in a Bose-Einstein condensate of Rb atoms on the right (Madison 2000). As the rotation speed increases, the system is invaded by more quantized vortices forming similar arrays in both cases.

The Bose-Einstein condensation (BEC) in quantum gases of cold atoms was discovered in 1995 shortly before clear evidence for their superfluidity was demonstrated (Cornell 2002, Ketterle 2002). The superfluidity of pairs of 3He atoms was discovered in 1972 by Osheroff et al. at Cornell University (Osheroff 1972) and shown to be a close analogue of the superconductivity of electron pairs in superconductors, whose famous "BCS" theory was established in 1957 by J. Bardeen, L.N. Cooper, and J.R. Schrieffer (Bardeen 1957). The continuity from BCS to BEC was also demonstrated with quantum gases of Fermi atoms as a function of the intensity of interactions between these atoms (Zwierlein 2004). In summary, the connexion of superfluidity with Bose-Einstein condensation is well established, as well as its relation to superconductivity.

Superfluidity has been discovered in other examples of condensed matter, especially semiconductors where it concerns electron-hole pairs interacting with photons in cavities, that is so-called "polaritons" (Amo 2011). Eventually, it is usually assumed to take place in the inside of neutron stars where it is invoked to explain glitches and cooling rates (see Chamel 2011 for a recent review). In the latter case it concerns pairs of neutrons, which may be superfluid even at temperatures of order a million degrees due to their very high density, as was predicted first by A.B. Migdal (Migdal 1959).

A future development might be the superfluidity of solids, called “supersolidity” (Balibar 2010). It is indeed possible that in a quantum crystal, where atoms are not totally localized on their crystal lattice sites so that they can exchange site with their neighbours, part of the total mass is superfluid while the rest ensures elasticity, an essential property of solids. The existence of supersolidity is presently debated in the case of solid helium 4 around 0.1 Kelvin since the experiments by Kim and Chan in 2004 (Kim 2004). However its existence is still controversial in 2012. Supersolidity might also be obtained in Bose-Einstein condensates of cold molecules where one could tune the interactions to the right value.

As concerns applications, the major application of superconductivity is in building high field electromagnets. The possibility to run very high amplitude currents without Joule heating has revolutionized medical imaging and MRI machines are now ubiquitous in modern hospitals. But if we restrict ourselves to superfluidity of neutral systems, its main application is as a cooling fluid. MRI machines contain superconducting magnets that are usually cooled down to 4K with normal liquid helium 4. Only very high field experimental machines need superfluid helium as a coolant. The best-known electromagnet using superfluid helium is the 27 km long ring of magnets used in the Large Hadron Collider at CERN (Geneva) to accelerate protons and anti-protons and search for new elementary particles. From the 1930’s when superfluid helium was available in quantities not much more than a few cubic centimetres to the giant LHC, it is obvious that superfluidity is no longer a curiosity but a phenomenon of possible use at the industrial scale. Still, its main applications are for low temperature research in laboratories where studying matter from 2K down to mK temperatures would be practically impossible without superfluid helium. Not only superfluid helium has allowed to make fundamental progress in the understanding of electrical transport in matter and in magnetism, it has also allowed to cool down the very low noise detectors used in astrophysics to determine the Cosmic Background radiation in relation with the origin of the Universe.

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