PHYSICAL REVIEW VOLUME 75, NUMBER 5 MARCH 1, 1949

Letters to the Editor

PUBLICATION of brief reports of important discoveries in physics may be secured by addressing them to this department. The closing date for this department is five weeks prior to the date of issue. No proof will be sent to the authors. The Board of Editors does not hold itself responsible for the opinions expressed by the correspondents. Communications should not exceed 600 words in length.

On the Theory of Superfluidity

L. LANDAU
Institute for Physical Problems Academy of Sciences of the U.S.S.R., Moscow, U.S.S.R.

November 19, 1948

SINCE my paper on the theory of the superfluidity of helium *II* was published in 1941[1] and in later papers,[2,3] this problem has been repeatedly discussed in scientific literature. I should like to express my opinion on some of the statements which have been put forward, especially in connection with the last paper by L. Tisza.[4] *

It follows unambiguously from quantum mechanics that for every slightly excited macroscopic system a conception can be introduced of "elementary excitations," which describe the "collective" motion of the particles and which have certain energies ϵ and momenta p (leaving aside the question as to the actual dependence $\epsilon(p)$, i.e., the actual form of the energy spectrum). It is this assumption, indisputable in my opinion, which is the basis of the microscopical part of my theory. On the contrary, every consideration of the motion of individual atoms in the system of strongly interacting particles is in contradiction with the first principles of quantum mechanics.**

As to the actual form of the energy spectrum, the general principles allow one merely to assert[1] that for small energies the dependence $\epsilon(p)$ must be of the "phonon" type, i.e., $\epsilon = cp$, c being the velocity of sound. This fact is in itself sufficient to deduce strictly the superfluidity of the liquid at sufficiently low temperatures (reference 1, § 5). It is useful to note that N. N. Bogoliubov[5] has succeeded recently, by an ingenious application of second quantization, in determining the general form of the energy spectrum of a Bose-Einstein gas with a weak interaction between the particles. As it should be, the "elementary excitations" appear automatically, and their energy ϵ as a function of the momentum p is represented by a single curve, which has a linear initial part. Although the model of such a gas does not have any direct bearing on the actual liquid helium *II*, it shows the manner in which the quantum-mechanical mathematical formalism leads, in fact, from a macroscopical body to an energy spectrum with the indicated properties.

The further trend of the ϵ *vs.* p curve cannot be established in a general form by purely theoretical considerations. The spectrum with two branches ($\epsilon = cp$ and $\epsilon = \Delta + p^2/2\mu$), which I originally postulated, consisted of two intersecting curves; the latter fact alone makes this spectrum unsatisfactory. These considerations, together with an elaborate investigation of the new experimental data, lead[3] to a spectrum consisting of a single curve; after a linear initial part, the function $\epsilon(p)$ passes through a maximum, then has a minimum and increases again.***

Apart from the microscopic theory and the calculation of the thermodynamic quantities based on this theory, my paper of 1941 contained also the derivation of the hydrodynamic equations for helium *II*. This part of the theory does not depend on the assumptions concerning the energy spectrum and the equations can be deduced starting merely from the conservation laws and the Galilean relativity principle. From these hydrodynamic equations the formula which determines the velocity of the "second sound" in terms of the thermodynamic quantities of helium *II* was deduced. I would like to emphasize that, at present this formula can be directly checked by the experimental data on the entropy and the specific heat of helium *II* and the values of ρ_n directly measured by E. Andronikashvilli.[6] Such a comparison shows excellent agreement between the theory and the experiment well inside the limits of the experimental error. Therefore, one must consider as a mere misunderstanding Tisza's assertion that this formula is in conflict with experiment.

The hydrodynamic equations given by Tisza are, in my opinion, quite unsatisfactory. It is easy to see that in their exact form they even violate the conservation laws!**** If one tries to obtain my results starting from these equations, it can be done only as far as equations of the first approximation are concerned, in which the terms of the second order in the velocities are neglected. In this case Tisza's equations can be readily reduced to my equations by means of a suitable definition of the arbitrary quantity p_n, which enters Tisza's theory; this is exactly what he does in his recent paper. Unfortunately, however, he obtains the correct result by using an incorrect assumption of the proportionality between the entropy and the normal part of the density ρ_n of helium *II*. Tisza's effort to give a thermodynamical foundation for this assumption is quite unconvincing and the formulas given[1,3] actually show that such a proportionality is absent.†

Tisza excludes phonons from the "normal part" of the liquid, the argument being that the phonons are "associated with the liquid as a whole," contrary to the "elementary excitations" which "correspond to helium atoms in translational Bloch-type states." Such exclusion of the phonons evidently presumes either that (1) the phonons penetrate freely through narrow slits, without scattering by the walls (I do not mention their mutual collisions, which can be calculated hydrodynamically and turn out to be by no means improbable), or (2) the moving phonon gas has no momentum, whereas the opposite ("the sound wind"!) is well known. Both alternatives are so obviously incorrect that I can hardly imagine which is the one adopted by Tisza. It should also be noted that the part of the density ρ_n which is due to phonons can be strictly calculated.[1] †† The experimental data which are available at present are yet insufficient to disprove Tisza's assertion, because of the comparatively small role of the phonons in the temperature region explored. But I have no doubt whatever that at temperatures of 1.0–1.1°K the second sound velocity will have a minimum and will increase with the further decrease in temperature. This follows from the values of the thermodynamic quantities of helium *II* calculated by me.

Tisza's paper contains also some considerations concerning the viscosity of helium *II*. These considerations are, however, confined to some unconvincing remarks on the necessity of distinguishing the "liquid-type viscosity" from the "gas-type or transport viscosity," and as a result Tisza concludes that the viscosity must decrease with decreasing temperature. Actually, this problem is theoretically rather complicated, and its solution requires an elaborate investigation of different elementary collision processes of phonons and rotons with each other. Such an investigation shows that the viscosity coefficient of helium *II* can be represented as a sum of two parts—the "roton part" and the "phonon part." The first one turns out to be independent of the temperature, whereas the second increases experimentally with decreasing temperature (a temperature region which is not too near to the λ-point is implied, thus allowing one to consider the phonons and rotons as a "perfect gas"). These results are entirely in accord with the recent viscosity measurements by E. Andronikashvilli,[7] which correspondingly are in conflict with Tisza's considerations.

Finally, I should like to dwell upon the question of behavior of foreign atoms dissolved in helium *II* (e.g., atoms of the isotope He^3). In a recent paper by I. Pomeranchuk and the author,[8] it was shown by considering the energy spectrum of a quantum liquid, together with a foreign atom that the presence of such atoms gives rise to the appearance of a new kind of "elementary excitation" connected with these atoms. These atoms enter the "normal part" of the liquid together with the phonons and rotons and thus cannot penetrate narrow slits (a fact actually observed by Daunt *et al.*[9]). It is to be emphasized that this fact has nothing to do with the question as to the superfluidity of the substance of the admixture in itself (in particular of the pure He^3), contrary to the opinion expressed in the literature (J. Franck[10] and Tisza[4]).

---

[*] 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 U.S.S.R. until 1943 owing to war conditions, and I regret having missed seeing his previous short letter (Comptes Rendus **207**, 1035, 1186 (1938)).] However, his entire quantitative theory (microscopic as well as thermodynamic-hydrodynamic) is in my opinion, entirely incorrect.

[**] Such reasonings are also present in Tisza's recent paper. No quantum meaning can be given to such assertions, as, e.g., "every vortex element can be associated with a definite mass contained in the volume in which the vorticity is different from zero" (reference 4, p. 852).

[***] Tisza's remark that this assumption "tends to modify the theory in the wrong direction" (reference 4, p. 852) can hardly be justified.

[****] For instance, the time derivative of the total momentum $\int(\rho_n\mathbf{V}_n+\rho_s\mathbf{V}_s)dV$ is not equal to zero.

[†] Accidentally, the temperature dependences of the roton parts of ρ_n and of the entropy are very similar (the only difference being the factor $1+3T/2\Delta$). It is this circumstance that enabled Tisza to attain a good agreement with the experimental data on the second sound velocity in the region of not too low temperatures, where the rotons prevail over the phonons.

[††] Tisza remarks that the argument given in reference 1 "is not convincing as it tends to obtain information on a kinetic coefficient (viscosity) from equilibrium considerations" (reference 4, p. 852). However, this is a mere misunderstanding. It is generally known that the uniform rotation admits a thermodynamic consideration, and the argumentation given[1] uses such considerations only for the calculation of that part of the helium mass which rotates together with the rotating vessel, whereas no conclusions on the magnitude of the helium viscosity can be obtained in such a way. Of course, my paper does not make an attempt of this kind.

[1] L. Landau, J. Phys. U.S.S.R. **5**, 71 (1941).

[2] L. Landau, J. Phys. U.S.S.R. **8**, 1 (1944).

[3] L. Landau, J. Phys. U.S.S.R. **11**, 91 (1947).

[4] L. Tisza, Phys. Rev. **72**, 838 (1947).

[5] N. Bogoliubov, J. Phys. U.S.S.R. **11**, 23 (1947).

[6] E. Andronikashvilli, J. Exper. Theor. Phys. **16**, 780 (1946); **18**, 424 (1948).

[7] E. Andronikashvilli, J. Exper. Theor. Phys. **18**, 429 (1948).

[8] L. Landau and I. Pomeranchuk, Comptes Rendus Acad. Sci. U.S.S.R. **59**, 669 (1948).

[9] Daunt, Probst, Johnston, Aldrich, and Nier, Phys. Rev. **72**, 502 (1947).

[10] J. Franck, Phys. Rev. **70**, 561 (1946).

On the Theory of Superfluidity*

L. Tisza
Massachusetts Institute of Technology, Cambridge, Massachusetts

December 20, 1948

In order to appreciate the relation of Landau's theory[1,2] of superfluidity to ours[3–5] one has to keep in mind a fundamental difference in attitude. We started from the conviction that the present techniques of handling the quantum-mechanical many-body problem are inadequate for a theory of liquid helium and formulated a few assumptions which enabled us to correlate and predict experimental facts.

On the other hand Landau attacked the helium problem from a more fundamental point of view and tried to derive the properties of this system from the principles of quantum mechanics. Landau criticizes our ideas not so much because of their internal inconsistency but because they do not follow from his theory of phonons and rotons. We are frankly impressed by the audacity and power of Landau's approach but we feel that he has introduced into his theory more or less disguised assumptions which cannot claim the same degree of certainty as the principles of quantum mechanics. These assumptions need an experimental verification no less than the assumptions formulated by ourselves.**

There are essentially two issues awaiting experimental decision. The first is whether or not the Bose-Einstein statistics is essential for the superfluidity of He*II*. We have pointed out[5] that the macroscopic effects cannot decide either for or against this assumption, since their interpretation depends only on certain general features of the two-fluid model. Landau is right in pointing out that the experiments with He^3, He^4 mixtures are not quite conclusive in this respect either. Recently, however, the liquefaction of pure He^3 has been reported[6] and the question is likely to be decided before long.

The second group of experiments concerns the low temperature behavior of helium *II*. In contrast to Landau, we predicted that somewhere between 0.6 and 1°K, helium *II* gradually becomes a homogeneous liquid and the thermomechanical effect vanishes. Thus heat transfer should occur by the conventional mechanism and not by second sound. Also the cooling method based on the thermomechanical effect[3,7] should fail in this temperature range.

The origin of this divergence is in a rather subtle difference in the interpretation of the two-fluid concept. We