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Comments

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Comment on "Equilibrium order parameters and chemical potentials in rotating superfluids"

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We show that there is no ground to the doubts raised by Ho and Mermin about the validity of the recently proposed modifications of ^{3}He hydrodynamics. Contrary to what they claim, the modified equations do have rotating equilibrium as a solution.

In a recent paper[1] Ho and Mermin have studied rotating equilibrium in superfluids. They claim to have demonstrated that recently proposed modifications of ^{3}He hydrodynamics induced by microscopic results are incorrect because the proposed equations do not have rotating equilibrium as a solution. Actually there is no demonstration in their paper that rotating equilibrium is not a solution of these equations, there is only a repeated claim. This claim is wrong and, as we shall see, rotating equilibrium is actually a solution of the proposed equations.

Let us first remark that in order to compare microscopic and macroscopic theories, it is necessary that the latter provide explicit results in terms of the variables that appear in microscopic theories. By explicit results, we mean that macroscopic theories must provide explicit equations giving the time derivative of all the conserved quantities and of all the components of the order parameter in terms of the conserved quantities, the order parameter, and their spatial derivatives. Conjugate variables like the temperature or the normal velocity can be used in the equations if one has explicit equations relating them to the previous quantities. For example, orbital theories of ^{3}He-*A* by Hu and Saslow[2] and Ho[3] are explicit and allow for comparison with microscopic theories. The same is true of ^{3}He-*B* hydrodynamic theory by Liu and Cross.[4] This is because all these theories provide an explicit expression for the free energy which should be used. On the other hand, Lhuillier's paper[5] does not give such an expression and therefore is not explicit and cannot be compared to microscopic theories. Ho and Mermin's article (if it is considered as independent of Ref. 3) only considers thermal equilibrium where the chemical potential μ is constant. However, the value of this constant is not known in terms of the variables that appear in microscopic theories. For this reason, Ho and Mermin cannot determine the validity of the microscopic results (they can only deduce that some combination of the variables should be constant in equilibrium). In order to do so, they have to make an additional assumption relating μ to the variables of the microscopic theories.

We consider now the controversial "phase" equation for ^{3}He-*A*. In the linear regime, two recent microscopic theories[6–8] now agree to find[9] ($\hbar = m = 1$)

$$-\frac{1}{2}\frac{\partial\phi}{\partial t} = \frac{\delta\rho}{\kappa} + \frac{1}{4}[1 - a_S(T)]\hat{l}\cdot\vec{\nabla}\times\vec{v}_n \ , \qquad (1)$$

where we have assumed an homogeneous $\hat{l}$ for simplicity. From weak-coupling microscopic theory, we have

$$a_s(T) = \frac{1 - Y(T)}{1 + F_1^S\phi_\perp(T)/3} \ , \qquad (2)$$

and $\kappa = N_0(1 + F_0^S)$ with standard notations.[6,7] On the other hand macroscopic theories[2,3,5] find

$$-\frac{1}{2}\frac{\partial\phi}{\partial t} = \mu + \frac{1}{4}\hat{l}\cdot\vec{\nabla}\times\vec{v}_n \qquad (3)$$

in the linear regime. In this regime, explicit[2,3] macroscopic theories give also

$$\mu = \mu_0 + \frac{\delta\rho}{\kappa} \ , \qquad (4)$$

where μ_0 is a constant. By changing the zero of energy we can take $\mu_0 = 0$ and, from Eq. (3) and Eq. (4), the results of macroscopic theories[2,3] is

$$-\frac{1}{2}\frac{\partial\phi}{\partial t} = \frac{\delta\rho}{\kappa} + \frac{1}{4}\hat{l}\cdot\vec{\nabla}\times\vec{v}_n \ , \qquad (5)$$

which is in disagreement with Eq. (1). Ho and Mermin[1] have characterized the disagreement by writing

$$-\frac{1}{2}\frac{\partial\phi}{\partial t}=\mu+\frac{\gamma}{2}\hat{l}\cdot\vec{\nabla}\times\vec{v}_n \tag{6}$$

and claiming that microscopic theories obtain $\gamma\neq\frac{1}{2}$ in contradiction with the result [Eq. (3)] of macroscopic theories. This is an incorrect and misleading statement: microscopic theories claim only the result [Eq. (1)] and since Ho and Mermin claim only Eq. (3) without giving any explicit expression for μ, there is no possible confrontation and therefore no contradiction.[10]

Let us now explain how the puzzle due to the contradiction between Eqs. (1) and (5) has been solved. This has been done by Combescot and Dombre[11] when they pointed out that, in order to obtain a consistent hydrodynamic theory, it is necessary to add a number of new terms (which they called "third order terms") to the expression of the energy functional. Accordingly they found out in ^{3}He-A, at $T=0$, a new term in the current expression which, in the linear regime, reads now[12]

$$g_i=\rho v_i^s-\tfrac{1}{4}\epsilon_{ijk}l_j\partial_k\rho \tag{7}$$

(we have again assumed an homogeneous $\hat{l}$). This new term gives an additional contribution to μ, since[13]

$$\mu=\left.\frac{\partial\epsilon}{\partial\rho}\right|_{g_i}=\left.\frac{\partial(\epsilon-v_j^n g_j)}{\partial\rho}\right|_{v_i^n}=\left.\frac{\partial\epsilon}{\partial\rho}\right|_{v_i^n}-\left.\frac{\partial}{\partial\rho}(v_j^n g_j)\right|_{v_i^n}\ , \tag{8}$$

where the first equality is the definition of μ, and the second one is an identity resulting from a Legendre transformation. To linear order, we have

$$\left.\frac{\partial\epsilon}{\partial\rho}\right|_{v_i^n}=\mu_0+\frac{\delta\rho}{\kappa} \tag{9}$$

(actually ϵ does not depend on v_i^n at $T=0$) and

$$\left.\frac{\partial(v_j^n g_j)}{\partial\rho}\right|_{v_i^n}=\tfrac{1}{4}\epsilon_{ijk}l_j\partial_k v_i^n\ , \tag{10}$$

where we have actually taken the functional derivative[14] with respect to ρ [there is also in Eq. (10) a nonlinear contribution $v_i^n v_i^s$ from the first term of Eq. (7) which we drop here]. Finally we obtain

$$\mu=\mu_0+\frac{\delta\rho}{\kappa}-\tfrac{1}{4}\epsilon_{ijk}l_j\partial_k v_i^n \tag{11}$$

which replaces Eq. (4). Together with Eq. (3), this gives

$$-\frac{1}{2}\frac{\partial\phi}{\partial t}=\frac{\delta\rho}{\kappa}\ . \tag{12}$$

In Eq. (12), the $\hat{l}\cdot\vec{\nabla}\times\vec{v}_n$ has disappeared and there is a complete agreement with Eq. (1) since $a_s(T)=1$ at $T=0$. This argument can be generalized[7] at finite temperature. Combescot[7] and Nagai[8] found the following generalization of Eq. (7) for finite temperature:

$$g_i=\rho_{ij}^s v_j^s+\rho_{ij}^n v_j^n-\tfrac{1}{4}a_s(T)\epsilon_{ijk}l_j\partial_k\rho\ . \tag{13}$$

Repeating the argument Eqs. (8), (9), and (10), we obtain at finite temperature

$$\mu=\mu_0+\frac{\delta\rho}{\kappa}-\tfrac{1}{4}a_s(T)\epsilon_{ijk}l_j\partial_k v_i^n\ , \tag{14}$$

which gives, together with Eq. (3)

$$-\frac{1}{2}\frac{\partial\phi}{\partial t}=\frac{\delta\rho}{\kappa}+\frac{1}{4}[1-a_s(T)]\hat{l}\cdot\vec{\nabla}\times\vec{v}_n \tag{15}$$

in exact agreement with Eq. (1).

To summarize there is at the present time no contradiction[15] between Eqs. (1) and (3) because one should use Eq. (14) for μ instead of Eq. (4). Since Eq. (3) is satisfied by microscopic equations, it follows automatically that rigid-body rotation is a solution of these equations, contrary to what Ho and Mermin have stated in Ref. 1, that it is not (but consistent with the general theorem on rotating equilibrium established by them in the same reference). Naturally, one has to make sure that, in rotating equilibrium, μ, as given by Eq. (14), is indeed a constant over the sample. This is easily checked[7] by writing that, in rotating equilibrium with angular velocity $\vec{\Omega}$. $F-\vec{\Omega}\cdot\vec{L}$ is minimum with respect to density fluctuations (F is the free energy and $\vec{L}$ the angular momentum).

Let us also comment on the gauge wheel of Liu and Cross.[16] They treat $\bar{\mu}=\mu+\vec{v}_n\cdot\vec{v}_s+\frac{1}{4}\hat{l}\cdot\vec{\nabla}\times\vec{v}_n$ as an effective chemical potential and therefore deduce that currents will be associated with variations of this $\bar{\mu}$ (in practical situations, they only consider the last term without taking into account the influence of μ itself). We think that this is an incorrect procedure and leads to incorrect conclusions. For example, consider a sample for which in region (1) $\hat{l}$ is uniform (and $\vec{v}_s=0$), parallel to the z axis, and in region (2) $\hat{l}$ is uniform, antiparallel to the z axis. If this sample is set in rotation with an angular velocity parallel to the z axis, according to Liu and Cross,[16] we would conclude that there is a difference $\Delta\bar{\mu}=\bar{\mu}_1-\bar{\mu}_2=\Omega$ in effective chemical potential between the two regions, leading to accelerated supercurrent flowing (presumably) from region (1) to region (2). Actually, once rotating equilibrium is reached, there should not be such supercurrent (as mentioned by Ho and Mermin[1] themselves). The real chemical potential μ will be constant over the whole sample. From Eq. (14) we obtain[7] that the density of region (1) will increase by an amount $\kappa a_s(T)\Omega/2$ while the density of region (2) will decrease by the same amount:

$$\delta\rho_1=-\delta\rho_2=\frac{\kappa}{2}a_s(T)\Omega\ . \tag{16}$$

Therefore, before equilibrium settles, currents will flow from region (2) to region (1) in order to produce the density changes [Eq. (16)]. We note that the effect [Eq. (16)], which has been predicted in Ref. 7, is strongly temperature dependent and disappears at $T = T_c$, in contrast to the effect that one would predict if the last term in $\bar{\mu}$ alone is considered, as is done in Ref. 16.

In the case, considered by Liu and Cross, where Ω is not uniform over the sample, we cannot draw a simple conclusion as with Eq. (16). But as before, currents are more likely to be driven by differences in μ rather than in $\bar{\mu}$, and anyway variations of μ over the sample should not be neglected. Moreover, it is not obvious that, in such a nonequilibrium situation, dissipative terms will be negligible.

We have stated in several occasions[9,11] that hydrodynamics should allow for the disappearance of all the reactive terms containing $\vec{v}_n$ from the explicit hydrodynamic equations when $T \to 0$. This means that the coefficients of all these terms should go to zero when $T \to 0$. This is just what happens in Eq. (1) since $a_s(0) = 1$. Combescot and Dombre[11] have shown that this property is satisfied by the hydrodynamic equations of ^{3}He-*B*. Finally, it is well known that this occurs also in superfluid ^{4}He. Naturally such a property is not a requirement of hydrodynamics itself. This is an additional[17] idea coming from the two-fluid model which underlines all our microscopic understanding of superfluid ^{3}He. Ho and Mermin reject this idea on the ground that, even at $T = 0$, "$\vec{v}_n$ is not deprived of an hydrodynamical role." We do not understand why this argument would work for ^{3}He, but not for ^{4}He (where it is easy to show that $\vec{v}_n$ disappears at $T = 0$, when ρ, $\vec{v}_s$, and $\vec{v}_n$ are taken as independent variables). We have seen actually that Eq. (1) is by no way in disagreement with hydrodynamics, while it allows the coefficient of $\hat{l} \cdot \vec{\nabla} \times \vec{v}_n$ to go to zero at $T = 0$. Ho and Mermin feel that the $\vec{v}_n$ terms are essential to allow the superfluid to exchange momentum with moving walls at zero temperature. This is not at all necessary. The superfluid will feel the motion of the wall through boundary conditions expressing, for example, the fact that the superfluid cannot go through the wall. Then this information will propagate in the bulk superfluid through hydrodynamic equations which contain themselves no information on the wall motion.

In conclusion we have shown that Ho and Mermin have actually no valid argument against the recently proposed modifications of ^{3}He hydrodynamics and the result from microscopic theories. These theories do have rotating equilibrium as a solution.

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[1] T. L. Ho and N. D. Mermin, Phys. Rev. B 21, 5190 (1980).

[2] C. R. Hu and W. M. Saslow, Phys. Rev. Lett. 38, 605 (1977).

[3] T. L. Ho, in *Quantum Fluids and Solids,* edited by S. B. Trickey, E. D. Adams, and J. W. Duffy (Plenum, New York, 1977), p. 97.

[4] M. Liu and M. C. Cross, Phys. Rev. Lett. 41, 250 (1978).

[5] D. Lhuillier, J. Phys. (Paris) L38, 121 (1977).

[6] K. Nagai, J. Low Temp. Phys. 38, 677 (1980).

[7] R. Combescot, Phys. Lett. 78A, 85 (1980).

[8] K. Nagai (unpublished).

[9] In a rather old, but only recently published work [R. Combescot, J. Phys. (Paris) L41, 207 (1980)], we did not find any $\hat{l} \cdot \vec{\nabla} \times \vec{v}_n$ term at all. This has been corrected now and we have therefore several independent ways of deriving Eq. (1). In any case, Eq. (1) (without Fermi-liquid corrections) is in Ref. 6 which is criticized by Ho and Mermin and the $T = 0$ limit of Eq. (1) is given in Ref. 11 which is also criticized by Ho and Mermin. Moreover, our point is that Ho and Mermin's general theorem alone is insufficient for determining the validity of the microscopic theories of the hydrodynamics of ^{3}He.

[10] A possible source of confusion stems from the fact that Refs. 6 and 9 used the notation $\delta\mu$ for $\delta\rho/\kappa$ as suggested by the result [Eq. (4)] from macroscopic theories. This should not be confused with the term μ in Eq. (3), the correct relation being given by Eq. (14).

[11] R. Combescot and T. Dombre, Phys. Lett. 76A, 293 (1980).

[12] About the time of submission of this paper, Mermin pointed out to me that Ishikawa *et al.* [Proceedings of the Hakone International Symposium 1977, edited by T. Sugawara, S. Nakajima, T. Ohtsuka, and T. Usui (Phys. Soc. Jpn. Tokyo, 1978), p. 159] had in their expression for the current (but not quite explicitly) a term $\hat{l} \times \vec{\nabla} \rho$. However, the trouble was at the time that the main term of their result was $\rho \vec{\nabla} \times \hat{l}$, in contradiction with the conventional result $\overleftrightarrow{C} \vec{\nabla} \times \hat{l}$ and therefore it looked incorrect. We were led quite independently to introduce the $\hat{l} \times \vec{\nabla} \rho$ by following the procedure which gave us also an intrinsic angular momentum term in the *B* phase, and did not realize before Mermin's remark that Ishikawa *et al.* had such a term. After submitting our paper, we received a recently published work by Mermin and Muzikar [Phys. Rev. B 21, 980 (1980)], where they had corrected Ishikawa *et al.*'s calculation and found also a $\hat{l} \times \vec{\nabla} \rho$ term. In retrospect the result of Ishikawa *et al.* looks much nearer to the truth than what it was at the time and quite precursory.

[13] See Eqs. (9) and (10) of Ref. 11. In Eq. (10), what they call μ_H is what is called μ in the present paper, and what they call μ is $(\partial\epsilon/\partial\rho)_{v_l^n}$ [see Eqs. (1) and (13) of Ref. 11 for these definitions].

[14] See R. Combescot, J. Phys. C 14, 1619 (1981) for a consistent treatment of these derivatives.

[15] One could go the other way around and, taking Eqs. (1) and (3) for granted, deduce that μ is necessarily given by Eq. (14).

[16] M. Liu and M. C. Cross, Phys. Rev. Lett. 43, 296 (1977).

[17] We stress that microscopic theories check that the results are in agreement with this principle, but never use it in the calculations. In other words, this is an output of these theories and not an input.