Pis'ma v ZhETF, vol. 84, iss. 6, pp. 416 – 417

Comment on "Solution of the problem of catastrophic relaxation of homogeneous spin precession in superfluid ^{3}He-B"

I. A. Fomin

P.L. Kapitza Institute for Physical Problems RAS, 119334 Moscow, Russia

Submitted 29 June 2006
Resubmitted 21 August 2006

The argument of Bunkov, L'vov and Volovik contains errors, making the obtained result non-convincing.

PACS: 67.57.Lm, 76.50.+g

The paper of Bunkov, L'vov and Volovik (BLV, following their own abbreviation) [1] is the second paper, published recently in "Pis'ma v ZhETF" and addressing the problem of theoretical explanation of "catastrophic relaxation" in superfluid ^{3}He-B. It differs from the paper [2], published before in setting of the problem. In Ref. [2] a problem of stability of spatially uniform precession of spin in ^{3}He-B with the order parameter following motion of spin in the Brinkman and Smith configuration is analyzed. It is shown in this paper, that the described precession at sufficiently low temperatures experiences Suhl instability, i.e. decays in parametrically excited spin waves. Coupling between the uniform precession and spin waves is provided by the anisotropy of spin-wave velocity. Brinkman and Smith stationary solution of Leggett equations serves as a basis for interpretation of pulsed NMR experiments in ^{3}He-B. For other configurations precession of spin is stationary only on the average, in the limit $\Omega/\omega_L \ll 1$, where Ω is the Leggett frequency and ω_L – the Larmor frequency. Even in this limit for most of configurations, including those, considered by BLV, the uniform precession is unstable [3] due to the same mechanism, which is responsible for the instability of precession in the A-phase. So, the result of Ref. [2] means that Suhl instability sets a limit in temperature for possibility to realize precession of spin in ^{3}He-B even in the ideally uniform conditions. Only this limiting temperature can be taken as a physically meaningful definition of T_{cat}.

In Ref. [1] a different question is addressed: why the stationary precession of spin can not be observed below certain threshold temperature in the particular conditions of experiments Refs. [4, 5], which are considered as typical? Figure in Ref. [1] shows that at these conditions the walls of container perturb Brinkman and Smith configuration in a significant part of the container. BLV argue, that in the "typical" situation the idealization of Ref. [2] does not apply and suggest instead their own treatment of the problem also based on Suhl instability and on a locally uniform precession, but out of Brinkman and Smith configuration. The essential point in the argument is that for configurations different from that of Brinkman and Smith there exist additional coupling of a uniform precession to spin waves, originating from the dipole energy and this coupling can be dominant in "typical" experimental conditions.

There is no doubt that analysis of validity of an idealization for a particular experimental set-up is important part of interpretation of experimental data and that a proper generalization of theory in a way, that makes possible its application to a wider class of experimental conditions is very useful. Unfortunately this can not be said about the generalization, suggested in Ref. [1]. The argument of BLV contains ambiguous assumptions and non justified approximations. The most disturbing of these are the following:

1. The increment $V_{BLV} \sim \Omega^2/\omega_L$. BLV argue that for small anisotropy of spin-wave velocity μ there exist a region of magnetic fields $\mu\omega_L \ll \Omega \ll \omega_L$, where this increment is greater then that, originating from the anisotropy itself. But they do not take into account the existence of another instability [3], which was mentioned above. This instability also has increment of the order of Ω^2/ω_L. Why this mechanism can be disregarded?

2. For comparison with experiments BLV average the found increment over the volume of the cell. According to the figure in Ref. [1] variation of the increment V is not small in comparison with the average increment $\langle V\rangle$. In this situation substitution of $\exp(\langle V\rangle t)$ instead of $\langle\exp(Vt)\rangle$ is not correct operation – two ways of averaging can give very different results.

3. The numerical example of Ref. [1] which represents the "typical" experimental conditions with $\Omega =$ $= 244$ kHz and $\omega_L = 460$ kHz demonstrates a good agreement of the calculated within the BLV scheme temperature of catastrophic relaxation $T_{cat} = 0.5T_c$ with the

measured $T_{cat} = 0.47T_c$. The square of wave vector of the excited spin waves enters Eq. (28) of Ref. [1] from which T_{cat} is evaluated. In this evaluation BLV use the linear dispersion law for the γ-mode. When the dipole energy is taken into account the γ-mode acquires a gap $\omega_s(k) = \sqrt{[\Omega(s,l)]^2 + (ck)^2}$. The value of $\Omega(s,l)$ depends on s and l varying within the container as shown on the figure in Ref. [1]. The most important is a region near the maximum of the increment. Consider as representative the point, where $s = l \approx \cos 28°$. For this point $\Omega(s,l) \approx 0.90\Omega \approx 220\,\mathrm{kHz}$, i.e. $\Omega(s,l)$ is only slightly smaller then $\omega_L/2 = 230\,\mathrm{kHz}$. With these parameters conservation of energy for the gapped dispersion law requires much smaller values of k then for the linear dispersion. In the considered example the proper account of the gap brings k^2 ten times down i.e. renders additional factor of $\approx$10 in the r.h.s of Eq.(28). This factor definitely must have a damaging effect on the obtained agreement. It is important also, that for small k breaks down the local approximation, used by BLV.

In view of the above remarks the statement made in the conclusion of Ref. [1]: "Our analytical result for the onset of the parametric instability due to this mechanism is in a good quantitative agreement with experimental results" does not represent the situation correctly.

On the other hand the message of BLV that conditions in the experiments [4, 5] substantially deviate from the ideal conditions assumed in Ref. [2] is a serious warning. It means that for experimental investigation of the "intrinsic" mechanism of catastrophic relaxation, suggested in Ref. [2], larger containers and stronger magnetic fields have to be used, like in the experiments Ref. [6].

Note added after reading of Bunkov, L'vov, and Volovik reply [7]. I thank Yu.M. Bunkov, V.S. L'vov and G.E. Volovik for the explanations, which made more clear their argument. It does not mean thou that all arguments are convincing. The answer to question 1 rely on the stabilization of instability by texture. Such stabilization can take place for wave lengths longer or of the order of characteristic length of the texture itself. As one can conclude from the figure in Ref. [1] the textural length is about .1 cm, while the wave length, corresponding to the maximum increment of instability under discussion is of the order of 10^{-2} cm and instability can not be fully compensated. The answer to question 2 is quite convincing. I also agree that the error mentioned in question 3 probably does not introduce inaccuracy in the evaluation of the threshold larger then $20 \div 30\%$.

In the paper [7] BLV have analyzed dependence of $1/D_{cat}$ on magnetic field. The found ω_L^3 dependence, which is considered by these authors as a crucial argument making distinction between the surface mechanism against the bulk, indicates only that the dipole energy is essential for a mechanism of the instability. It should be remarked, that among the three channels of possible decay of the uniform precession in the bulk, considered in Ref. [2], there are two, which depend on the dipole energy and give rise to increments, proportional to Ω^2/ω_L, as for the surface mechanism. These channels of bulk decay can not be ruled out on the basis of the ω_L^3 dependence. They were discarded in Ref.[1] since they are proportional to the product of two small parameters – anisotropy μ and Ω^2/ω_L^2. Nevertheless one of these channels renders increment, which for the example, considered in Ref. [2] (cf. Eq.(37)), is only two times smaller then for the leading channel. For the moment there is no accurate data about the spin wave damping at sufficiently low temperature and in particular on the dependence of this damping on the direction of propagation of the wave. Numerical answer for T_c and even the choice of the leading channel can depend on a method of extrapolation of the existing data in the low temperature region. Without reliable data on the spin wave damping for relevant types of spin waves in a temperature region, where the catastrophic relaxation is observed, quantitative interpretation of this phenomenon is hardly possible.

1. Yu. M. Bunkov, V. S. L'vov, and G. E. Volovik, Pis'ma v ZhETF **83**, 624 (2006).
2. E. V. Surovtsev and I. A. Fomin, Pis'ma v ZhETF **83**, 479 (2006).
3. I. A. Fomin, ZhETF **78**, 2392 (1980) [JETP **51**, 1203 (1980)].
4. Yu. M. Bunkov, V. V. Dmitriev, Yu. M. Mukharsky et al., Europhysics Lett. **8**, 645 (1989).
5. Yu. M. Bunkov, J. Low Temp. Phys. **135**, 337 (2004).
6. D. A. Geller and D. M. Lee, Phys. Rev. Lett. **85**, 1302 (2000).
7. Yu. M. Bunkov, V. S. L'vov, and G. E. Volovik, Pis'ma v ZhETF **83**, 349 (2006).

От редакции

Ответ на комментарий И.А. Фомина содержится в статье публикуемой в данном номере (стр.349).