

Comment on “Thermodynamics of quantum crystalline membranes”

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Amorim *et al.* [*Phys. Rev. B* **89**, 224307 (2014)] reported the theoretical investigation of quantum crystalline membranes. In this Comment we dismiss the validity of their calculations based on a “natural” estimation of the ultraviolet divergent contributions into correlation functions. We claim that such calculations give qualitatively the wrong results.

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In a recent paper [1] the authors study long-scale vibrational modes of free-standing crystalline membranes. In particular, they examine the low-temperature membrane, areal thermal expansion coefficient, and the membrane specific heat. The same systems (free-standing crystalline membranes) were investigated in our paper [2] where we demonstrated that quantum fluctuations produce logarithmic renormalization of the membrane elastic moduli regarding zero surface tension. However, the authors of the paper [1] include into consideration a contribution into the surface tension (the term proportional to k^2 in the self-energy function) that comes from short-scale fluctuations. In our Comment we warn that such terms cannot be calculated or even estimated via evaluation of the UV terms.

Let us be reminded about the Landau-Wilson paradigm for applications of the field theory in condensed-matter physics (see, e.g., the survey [3] and the monographs [4,5]). A macroscopic system can be described in terms of a few relevant variables (fields) possessing “soft” behavior, i.e., slow in time and smooth in space (in comparison with microscopic degrees of freedom). The macroscopic properties of the system can be analyzed in terms of a field theory that serves as a controllable theoretical tool for calculating correlation functions of the variables (fields). In theory, the correlation functions are presented as path integrals with a weight factor determined by the effective action or the Landau functional of the system.

Usually, the correlation functions are calculated in the framework of a perturbation expansion. Then some UV divergent integrals appear unavoidably that cannot be calculated within the long-scale field theory. Such UV divergent terms have to be included into the renormalization (redefinition) of the factors of the Landau functional or of the effective action [3,4]. This concept has frequently been misinterpreted, and it should not be mixed with renormalizability of the model Hamiltonian. The latter one is about a number of relevant interaction vertices.

What is worse is that estimations of the UV divergent contributions, based on the “natural” microscopic cutoff (Debye frequency for the problem under consideration) are, generally, incorrect. The matter is that some macroscopic parameters have to be zero due to a symmetry or due to some other physical circumstances, whereas the natural estimations give nonzero values for the parameters. This is just the case for the freely suspended crystalline membranes that have zero surface tension σ . This fact follows from the equilibrium condition

with respect to area variations for the freely suspended films that may adjust their area to minimize the film free energy.

One can say that $\sigma = \sigma_{\text{bare}} + \sigma_{\text{UV}} = 0$, where σ_{bare} is the “bare” value of the surface tension and σ_{UV} is the UV (short-scale) contribution to the surface tension. Moreover, there is no way to calculate within the long-scale theory or to determine separately the contributions σ_{bare} and σ_{UV} . Only the sum of both has physical meaning for the tensionless free-standing crystalline membranes $\sigma_{\text{bare}} + \sigma_{\text{UV}} = 0$. It is obvious that in this situation the contribution σ_{UV} cannot be used for estimating σ . Neither can σ_{UV} be used to calculate thermodynamic characteristics. For example, flexural contribution into the low-temperature specific heat C_p calculated at constant pressure (i.e., constant external stress) scales (up to logarithmic corrections) as T^2 and not linear in T as in the paper [1] with the UV divergent term σ_{UV} for the surface tension.

Next, the coefficient in front of the UV divergent k^2 term in the self-energy function analyzed in the paper [1] cannot be interpreted as the renormalized bending modulus. It is the surface tension as one can conclude by comparing the poles of the Green’s function and the phenomenological equation for bending fluctuations since both should give the same long-wavelength dispersion law. The dispersion law reads as $\rho\omega = \sigma k^2 + \kappa k^4$, where ρ is the mass density, κ is the bending module, and σ is the surface tension. For tensionless free-standing films $\sigma = 0$. If the membrane is somehow stretched, then the surface tension σ is nonzero. Then σ is determined by an external force stretching the membrane, that is, by a macroscopic factor. Then, evidently, σ is much smaller than its natural estimation made in terms of the microscopic (atomic) parameters that determine σ_{UV} .

A certain similarity of the problem under discussion and the long-scale description of continuous (second-order) phase transitions are worth noting. The transition temperature T_c is determined by microscopic material parameters and characteristics and cannot be found in the framework of the long-scale Landau-Wilson approach. Some UV divergent terms appearing in the renormalization procedure should be included into the definition of T_c : $T_c \rightarrow T_{c,\text{bare}} + T_{c,\text{UV}}$. However, neither $T_{c,\text{bare}}$ nor $T_{c,\text{UV}}$ can be found in the framework of the long-scale theory. Thus, in the long-scale theory T_c has to be considered as a phenomenological parameter, and its value can be taken from experimental data or microscopic calculations.

Note the point (although it is a minor issue for our Comment's main message) that the authors of the paper [1] state that in our paper [2] we neglect: (i) "some relevant anharmonic terms" and (ii) "the effects of retardation." In statement (i) the authors of the paper [1] probably have in mind the fourth-order interaction vertex for the bending fluctuations. We certainly do not neglect this vertex. The fact is that in the one-loop approximation the vertex appears only in the UV divergent (Hartree-like) contribution to the self-energy function, that has to be included into the redefinition of the surface tension (see above). In statement (ii) the authors of the paper [1] probably pointed to the frequency-independent

renormalized interaction vertices in our paper. The fact is that in our paper [2] we exploit the standard renormalization-group (RG) procedure that is valid if the dimensionless interaction constant is small. In this RG scheme the vertices remain frequency independent, indeed.

To conclude, we claim in this Comment that the results obtained in the paper [1] on the basis of the natural UV estimation of the surface tension are qualitatively wrong and are not applicable to free-standing crystalline membranes.

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