There is no paradox

Dear Friends,

Since Dima's idea of holding a small workshop did not materialize, let me at least try to reply to his note[1] of May 5, 2019.

I consider a scalar field ψ_l on a lattice of N sites ($l = 1, 2, ...N$). The field is CLASSICAL, so ψ_l are just complex numbers. The dynamics is given by

$$i\frac{d\psi_l}{dt} = -\sum_j J_{lj}\psi_j + \chi |\psi_l|^2 \psi_l, \quad l = 1, \cdots, N \tag{1}$$

where the coefficients $J_{lj} = J_{jl}^*$ (for $l \neq j$) represent couplings between different sites and $J_{ll} \equiv \omega_l$ is the frequency corresponding to site l. The last term in Eq. (1) describes the nonlinearity. Both J_{lj} and ω_l can contain some randomness, so we are dealing here with what Dima calls a "generic system".

The energy corresponding to a configuration $\{\psi_l\}$ of the field (the Hamiltonian) is

$$\mathcal{H}\{\psi_l\} = -\sum_{l,j} J_{lj}\psi_l^*\psi_j + \frac{1}{2}\chi \sum_l |\psi_l|^4. \tag{2}$$

The dynamics in (1) conserves the total energy $\mathcal{H}\{\psi_l(t)\} = E$ and the total norm $\mathcal{N}\{\psi_l(t)\} = \sum_l |\psi_l(t)|^2 = A$.

It has been known, since the work of Rasmussen et al[2], that a system defined by Eqs. (1), (2) will thermalize if initially prepared with appropriate values of E and A. That is, after sufficiently long time, the system reaches the state of thermal equilibrium and its properties are determined by the grand partition function

$$\mathcal{Z} = \int \left(\prod_{l=1}^N d\psi_l^* d\psi_l\right) e^{-\beta\mathcal{H} + \beta\mu\mathcal{N}} \tag{3}$$

where the inverse temperature $\beta = 1/T$ and the chemical potential μ are related to E and A by the standard thermodynamic relations.

I assume, as Dima does, weak nonlinearity whose contribution to the total energy and norm is negligible (the nonlinearity is of course crucial for the thermalization process). One can introduce the set of eigenmodes $f_\alpha(l)$ (i.e. stationary solutions of (1) in the absence of nonlinearity), with eigenfrequencies ε_α, and expand $\psi_l(t) = \sum_\alpha C_\alpha(t) f_\alpha(l)$. The set $\{C_\alpha(t)\}$ provides the description of the system in the mode representation. The total energy and norm (with the nonlinear contribution being discarded) in the mode representation are

$$E = \sum_{\alpha=1}^N \varepsilon_\alpha |C_\alpha|^2, \qquad A = \sum_{\alpha=1}^N |C_\alpha|^2. \tag{4}$$

and the partition function is given by the product of independent modes contributions

$$\mathcal{Z} = \prod_{\alpha=1}^N \left[\int dC_\alpha^* dC_\alpha e^{-\beta(\varepsilon_\alpha - \mu)|C_\alpha|^2}\right] = \prod_{\alpha=1}^N \left[\frac{\pi}{\beta(\varepsilon_\alpha - \mu)}\right], \tag{5}$$

It immediately follows from Eq. (5) that the average of the norm in mode α obeys the Rayleigh-Jeans distribution

$$\langle |C_\alpha|^2 \rangle = \frac{T}{\varepsilon_\alpha - \mu}. \tag{6}$$

This result was obtained by many authors, see the list of references in our resent work[3]. (I stress again that we are dealing here with a field and not with discrete particles.)

Thus, I consider Eq. (6) a well established result, whose derivation involves nothing more than the use of standard statistical mechanics (plus, of course, the assumption that the system had reached thermal equilibrium). There is nothing paradoxical in this result: a classical field, in thermal equilibrium, obeys a classical Rayleigh-Jeans distribution. A somewhat unusual feature in Eq. (6) is the appearance of a chemical potential. This is due to the fact that in our problem we have a second conserved quantity (norm), in addition to energy. That's why the distribution (6) does not obey equipartition and, under appropriate conditions, can exhibit a phenomenon of condensation (see[3] and references therein). Dima doesn't appreciate the importance of this second conserved quantity and he keeps saying that a classical system, like considered here, is expected to obey equipartition.

Instead of Eq. (6) Dima advocates a Bose-Einstein distribution[1,4] and then announces his "DTC paradox". But, as I already said, everything is classical in this problem- the Hamiltonian, the equations of motion and the distribution of the norm over the modes, Eq. (6). True, the spectrum of the modes is discrete but there is nothing quantum in this fact- just classical waves in a cavity. I am adding this caveat because in[1] Dima implies that discreteness of the frequency spectrum means something "quantum": he writes "QUANTUM system (linear modes)").

However, having said all this, I must admit that I am never completely sure what Dima really has in mind. I am not even clear about his interpretation of the basic quantity ψ_l. At the beginning it seems that Dima interprets ψ_l as a classical field. Indeed, Ref. 4 contains "CLASSICAL nonlinear lattices" even in it's title! But then, in the same context, he talks about the single particle problem of Anderson localization, wave packet spreading, etc. For instance, in Ref. 4, below Eq (10) he writes "As usually for any QUANTUM system with energy levels ϵ_m we have...". A "quantum system" is also mentioned in Ref. 1. When I asked Dima to confirm if his ψ_l is a classical field, his answer was:

formally yes, psi is a classical field, BUT nonlinearity is weak/moderate and we are close to the quantum system with quantum eigenlevels (we may say that this underlined Quantum system can be called hidden quantum system or HQ system not to mix you; we main call the whole system classical field CF-system)

Well, this sounds a bit ambiguous, to say the least, and the repeated use of the misnomer "quantum Gibbs" doesn't help to clarify the matter. Even normalizing $\sum_l |\psi_l(t)|^2$ to unity and interpreting $|C_\alpha|^2$ as occupation probabilities, as Dima does, is not appropriate for the problem at hand. Such normalization is appropriate for the problem of spreading of an initially localized wave packet but not for the thermalization problem we've been dealing with. The latter pertains to a MACROSCOPIC system with extensive E and A, proportional to the system volume N. The distinction between the two problems was particularly emphasized in Ref. 5,6.

In conclusion, I stated my position as clearly and explicitly as I could. As for Dima's note[1] I find it barely comprehensible and, in any case, "orthogonal" to the work on thermalization, as presented in Refs. 2,3,5,6 and references therein. I realize that this is my subjective judgment but it is the only judgment I can offer(-:

With best wishes for a happy year 2020: Good health, good research and no paradoxes(-:

Boris

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[1] D. Shepelyansky, *Dynamical thermalization conjecture (DTC) paradox*, http://www.quantware.ups-tlse.fr/dima/myrefs/myunp007.pdf.

[2] K. Rasmussen, T. Cretegny, P. Kevrekidis, and N. Grønbech-Jensen, Phys. Rev. Lett. **84**, 3740 (2000).

[3] A. Ramos, T. Kottos and B. Shapiro, arXiv:1912.10569

[4] L. Ermann and D. Shepelyansky, New Jour. Phys. **15**, 123004 (2013).

[5] D. M. Basko, Ann. Phys. **326**, 1577 (2011)

[6] T. Kottos and B. Shapiro, Phys. Rev. **83** 062103 (2011).