Reply to "Comment on 'Lagrangian formulation and symmetrical description of liquid dynamics'"

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We make three remarks in reply to the comment by Bryk, Duviryak, and Mryglod (BDM) [Bryk, Duviryak, and Mryglod, Phys. Rev. E **99**, 036102 (2019)]: (a) the discussion of shear liquid dynamics cannot be incorrect for the reason that this discussion does not include other effects such as longitudinal fluctuations; (b) the same point of relaxation time has been already discussed and published by Bryk *et al.* in their earlier comment [Bryk, Mryglod, Ruocco, and Scopigno, Phys. Rev. Lett. **120**, 219601 (2018)] and in our related reply [Yang, Dove, Brazhkin, and Trachenko, Phys. Rev. Lett. **120**, 219602 (2018)]; and (c) the field transformation for the complex scalar field theory used by BDM is incorrect.

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In reply to three comments by Bryk, Duviryak, and Mryglod (BDM) [1] in relation to our earlier paper [2], we note the following.

(a) BDM propose that a discussion of shear liquid dynamics is "incorrect" because it focuses on shear dynamics and does not include other effects such as longitudinal fluctuations. Its unclear why this makes the discussion incorrect. We have stated throughout (including Abstract, Introduction, and Summary) that our focus is on the shear liquid dynamics. Shear response and shear waves in liquids is one of the main topics discussed in this area (see, e.g., Ref. [3] for review).

BDM state that a viscoelastic expression for the velocity field is due to Frenkel. Instead, this expression is from earlier Maxwell work as discussed in our paper. BDM propose that Maxwell interpolation can be applied to transverse dynamics only and not to other effects such as longitudinal modes and positive sound dispersion. This is incorrect: Frenkel, in fact, used Maxwell interpolation idea to discuss (a) longitudinal modes in both hydrodynamic and solidlike elastic regimes and (b) positive sound dispersion (see pages 208–235 of Ref. [4]).

(b) We make three points regarding the BDM discussion of the relationship between Maxwell relaxation time η/G and liquid relaxation time τ, the microscopic time between particle rearrangements. First, this discussion is irrelevant as far as our paper is concerned. τ is simply a parameter in all equations. The entire discussion in Ref. [2] remains unchanged regardless of how τ is interpreted.

Second, BDM erroneously attribute the proposal to relate η/G to τ to us. The attribution is not done by us: it was first done by Frenkel [4]. Supported by numerous data that followed, this has become an accepted view (see, e.g., Refs [3,5] for review). Therefore, BDM's issue is not related to our results.

Third and importantly, this discussion is the same as that published recently by Bryk *et al.* [6] in their other comment on a different paper of ours. In our reply to that comment [7], we noted that there are several methods to calculate microscopic relaxation times τ which return somewhat different τ depending on the method and cutoff used [8,9]. Indeed, τ calculated as the microscopic time needed by an atom to gain or lose a neighbor returns τ very close to τ_M [9]. BDM chose not to state this result, albeit they cited Ref. [9] in their previous comment [6]. BDM choose to use only one possible cutoff to calculate τ but unfortunately do not report τ for different cutoffs. It is well-known that τ depends on the cutoff used and that using an appropriate cutoff results in τ close to the structural relaxation time or Maxwell relaxation time [8].

(c) We make three points regarding the BDM discussion of our Lagrangian. First, the field transformation that BDM propose to use is not the one used in the complex scalar field theory and is incorrect. Our Lagrangian is based on the two-scalar-field theory or, equivalently, the complex-scalar-field theory as stated in the paper. This theory is widely used in quantum field theory (QFT): $L_2=\frac{\partial\phi_1}{\partial t}\frac{\partial\phi_2}{\partial t}-c^2\frac{\partial\phi_1}{\partial x}\frac{\partial\phi_2}{\partial x}$. L_2 follows from $L_1=\frac{1}{2}\left(\left(\frac{\partial\psi_1}{\partial t}\right)^2-c^2\left(\frac{\partial\psi_1}{\partial x}\right)^2+\left(\frac{\partial\psi_2}{\partial t}\right)^2-c^2\left(\frac{\partial\psi_2}{\partial x}\right)^2\right)$ using the correct standard transformation $\phi_1=\frac{1}{\sqrt{2}}(\psi_1+i\psi_2)$ and $\phi_2=\frac{1}{\sqrt{2}}(\psi_1-i\psi_2)$ (see, e.g., Refs. [10,11]). Here, L_1 is the sum of two field Lagrangians, in contrast to the result of BDM. The dissipative term in Eq. (21) in our paper [2] becomes $L_d=\frac{i}{2\tau}\left(\psi_2\frac{\partial\psi_1}{\partial t}-\psi_1\frac{\partial\psi_2}{\partial t}\right)$ using the above transformation. The Hamiltonian corresponding to L_1+L_d is $H=\frac{1}{2}\left(\left(\frac{\partial\psi_1}{\partial t}\right)^2+c^2\left(\frac{\partial\psi_1}{\partial x}\right)^2+\left(\frac{\partial\psi_2}{\partial t}\right)^2+c^2\left(\frac{\partial\psi_2}{\partial x}\right)^2\right)$, where terms with τ cancel out. H is the sum of two wave energies as expected, in contrast to the BDM proposal.

Another way to see the flaw in the BDM argument is to consider L_1+L_d above, where L_1 is the standard two-field Lagrangian. Applying the Euler-Lagrange equations to L_1+L_d gives the system of coupled equations for ψ_1 and ψ_2. These equations are decoupled by using the same transformation, $\phi_1=\frac{1}{\sqrt{2}}(\psi_1+i\psi_2)$ and $\phi_2=\frac{1}{\sqrt{2}}(\psi_1-i\psi_2)$, as above and subsequently adding and subtracting the resulting equations for ϕ_1 and ϕ_2. The result is the same equations for ϕ_1 and ϕ_2 as in Eq. (22) in our paper [2], the equations which follow from the proposed Lagrangian (21).

Second, the incorrect field transformation used by BDM leads them to a fallacious conclusion. Indeed, setting $\tau\to\infty$ in their final Lagrangian, $L_{\rm BDM}$ becomes the transformed Lagrangian for the complex-field-theory Lagrangian L_2

(see above). L_{BDM} is unphysical as noted by BDM, implying that the complex-field-theory Lagrangian L_2 is equally unphysical. This is in contradiction with the wide use of the complex-scalar-field theory in QFT including the description of electromagnetism [10,11].

Finally, commenting that selecting a trivial solution $\phi_2 = 0$ in the complex-field-theory Lagrangian L_2 gives zero energy and therefore makes the energy and the Hamiltonian based on L_2 "not a useful quantity" is at odds with the wide use of L_2 in QFT [10,11].

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