Reply to “Comment on ‘Reexamining the relation between the binding energy of finite nuclei and the equation of state of infinite nuclear matter’ ”

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Reply to Comment on "Reexamining the relation between the binding energy of finite nuclei and the equation of state of infinite nuclear matter"

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In their comment to our paper [1], Bertsch and Stroberg [2] provide three criticisms. Two of these concern the interpretation of our dispersive optical model (DOM) results and their relation to the liquid drop model (LDM) parameters. The third criticism focuses on the potential systematic uncertainties on our DOM results associated with missing three-body contributions. Before addressing these critiques, we want to state that the key message of our paper remains whether or not the DOM results agree with the LDM predictions in the nuclear interior. The key point is that the standard determination of the saturation energy from the LDM is not ideal since the total binding energy has minimal contribution from the core of the nucleus, as pointed out in Figs. (1-3) of our paper.

The first two observations have to do with the results presented in our Figs. (5) and (6). In Fig. (5), the dashed line is indeed obtained by subtracting an LDM symmetry energy contribution obtained from Eq. (1) in our paper to the canonical value of the saturation energy, −16 MeV, to get the dashed line at approximately −15 MeV. The authors of the comment would rather use the value of a_V = -15.6 MeV instead, raising the dashed line to a value around −14.5 MeV which would lie closer to (but not significantly overlap with) our DOM results. They also indicate that the symmetry energy term in the LDM should not be used for an infinite system because it contains both surface and volume contributions. When using the quoted values in the comment, they claim that the dashed lines should be closer to a band centered around −14.4 MeV with a width of 0.8 MeV. Using the central value in this band, there is not a significant overlap with the DOM results presented in Fig. 5. We claim that this does not impact any of our conclusions.

The authors criticize the lack of an explicit three-body term in our calculation of the binding energy. We would first like to point out that the one-body self-energy does not exclude contributions from any n-body interactions (1,2,3,4, etc.) [3]. Since we parametrize the self-energy, we are, in principle, including all possible contributions. Thus, our single-particle propagator is not excluding three-body forces, so their effect is not absent from our calculation. In this sense, quantities such as the charge density and particle number are including the effects of higher-body forces. However, we indeed do not include an explicit three-body term in our calculation of the binding energy. With this in mind, the main point of displaying the three-body contribution to the energy density in Figs. (3-4) is to show that it will not change the *shape* of the total binding-energy density. Therefore, the lack of a three-body contribution does not alter our main point that the core of the nucleus minimally contributes to the total binding energy.

The authors then quote infinite nuclear matter (NM) calculations using three-body forces that show significant three-body contributions to the saturation energy [4, 5]. These values are quite variable. We do not think that these values of the three-body interaction will change the fact that the saturation energy is not necessarily the canonical value of 16 MeV. Furthermore, the calculation in Ref. [5] using chiral interactions fails to reproduce the NM saturation density of 0.16 fm^{-3} (which is not surprising since calculations using these realistic chiral interactions fail to reproduce the interior density of finite nuclei [6]). This is not uncommon since chiral interactions have so far not been able to simultaneously reproduce nucleon-nucleon scattering data, binding energies of intermediate-mass nuclei, and the NM saturation density [7]. Even when uncertainties from chiral interactions are propagated into the many-body problem, the saturation density is not reproduced [8]. As argued in our paper, the saturation density is well constrained by experimental data. Therefore, if a calculation results in a saturation point with a density different from 0.16 fm^{-3}, then we would claim that the three-body contributions to the corresponding saturation energy are not applicable to our results.

In conclusion, we do not claim that we predict the correct value of the saturation energy in our paper [1]. Rather, we point out that the saturation energy is not necessarily 16 MeV.

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