COMMENT ON "SOLUTION OF THE SPECIFIC MODEL OF FIVE-BODY PROBLEM TO INVESTIGATE THE EFFECTIVE ALPHA–NUCLEON INTERACTION IN A PARTIAL-WAVE ANALYSIS"*

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The authors argue that the five-body binding energies obtained from the solution of the coupled Yakubovsky integral equations by E. Ahmadi Pouya and A.A. Rajabi [*Acta Phys. Pol. B*, **48**, 1279 (2017)] are incorrect and should be discarded. The theory and formalism of the paper have serious mistakes and the numerical results are not trustable and cannot be validated.

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Ahmadi Pouya and Rajabi have recently reported the binding energy of a five-body system using few spin-independent nucleon–nucleon interaction models [1]. To this aim, they have used the simplified form of a six-body bound state formalism in Yakubovsky scheme, developed by Witała and Glöckle [2]. As it is shown in Ref. [2], the six-body Yakubovsky formalism leads to five coupled equations for the bound state of six identical particles, which can be reduced to two coupled equations for two-neutron halo nucleus of ^{6}He. Similar to the six-body bound state problem, for a full solution of a five-body bound state problem, one needs to consider five coupled Yakubovsky components.

Similar to two other *retracted* papers published by Ahmadi Pouya and Rajabi about the solution of the six-body Yakubovsky equations [3, 4], as we have discussed in two other comments [5, 6], the new paper has also serious mistakes in the theory, formalism, and the numerical implementation.

The main mistake of the paper is that the authors have used just two of five Yakubovsky components which can be applied only to describe one nucleon–halo bound nucleus composed of ^{4}He and a nucleon, whereas such a system does not exist in nature! If the fifth nucleon be a neutron, the five-body system ^{5}He is proven to be unbound [7–13]. As it is discussed in Ref. [9], there is no bound state for ^{5}He and the n–α two-body transition amplitude has a shallow $p_{3/2}$ resonance pole at -0.8 MeV. Otherwise, if the fifth nucleon is a proton, clearly the bound nucleus ^{5}Li does not have any halo structure. So, the solution of two coupled Yakubovsky equations does not describe any physical system. However, it can be used to get a naive solution for ^{5}Li, whereas for a full solution, one should consider all five coupled Yakubovsky components.

Beside the mentioned obvious mistake in the theory of the paper, in the following, we have addressed few of the mistakes and flaws in the text, the formalism and the numerical implementation of the paper.

1. The mistakes in the title of the paper:

    (a) How the solution of a five-nucleon bound state can provide any information about the alpha–nucleon interaction? It is not a two-body problem, *i.e.* α–N, it is a five-body problem by considering pair nucleon–nucleon interactions.

    (b) Part of the formalism and discussion of the paper is about the solution of the Yakubovsky integral equations in a partial wave decomposition and some part of the discussion is related to the solution of the Yakubovsky integral equations in three-dimensional

(3D) scheme [14–42], without using a partial wave (PW) decomposition. So, it is not clear in which scheme, PW or 3D, the authors have presented the Yakubovsky equations in momentum space.

2. The mistakes in the abstract:

   (a) The Yakubovsky formalism for a five-nucleon system leads to a set of five coupled equations [43], not four coupled equations! It is almost similar to the six-body formalism discussed in Ref. [2].

   (b) As we have mentioned before, considering two of five Yakubovsky components for describing $\alpha + N$ structure is just valid for the description of one nucleon halo nucleus, which does not exist for (^{4}He + N) system.

   (c) The difference between four- and five-body binding energies does not provide any information about alpha–nucleon interaction. How have the authors concluded that alpha–nucleon interaction is attractive? What does 13 MeV mean? Is it the strength of the interaction?!

   (d) The agreement between the results reported by authors using the spin-independent interactions with the results obtained by other groups with spin-dependent interactions is meaningless, especially when the authors have not solved full five coupled Yakubovsky equations.

3. The mistakes in the introduction:

   (a) Again, all the discussion about the alpha–nucleon interaction in the introduction of the paper is irrelevant, because five-nucleon bound state problem cannot provide any insight into the alpha–nucleon interaction.

   (b) What is the particular representation of the high-dimension eigenvalue matrix? What does it mean that it is systematic with respect to the number of components?!

   (c) What does it mean that the Hamiltonian operator is systematic with respect to the number of components?

   (d) When the authors address the basis states in a PW scheme, they do not realize that they have discussed in Section 4 the solution of integral equations in a 3D representation.

4. The mistakes in the formalism:

   (a) As we have mentioned before, the authors have used a simplified form of six-body Yakubovsky equations developed by Witała and Glöckle [2]. It is weird that the authors have taken the main part of their formalism from Ref. [2], but they have not cited the paper!

   (b) The simplification of five coupled Yakubovsky components from six-body bound state to five-body is not performed correctly. In the final form, the authors should have a set of five coupled Yakubovsky equations, see Ref. [43] for more details, whereas they have obtained a set of four coupled equations which is obviously incorrect. Consequently, Fig. 1 is not accurate.

   (c) How is it possible that the total five-body wave function be antisymmetric for spinless particles? Equations (3.1) and (3.2) are not valid for the bosonic particles, and all the negative signs for permutation operators P_{34} and P_{45} should be changed to the positive sign.

   (d) How is it possible that the total angular momentum of five-body system be 1, *i.e.* $L = 1$, when the authors have ignored the spin and isospin degrees of freedom and have set all the angular momentum quantum numbers to zero? So, what the authors have discussed after equation (B.6) is totally wrong.

   (e) Derivation of equations (3.3) and (3.4) from equations (3.1) and (3.2) is absolutely wrong. Why have the authors ignored the permutation operator P_{45}? How does this lead to neglecting the interaction of the fifth particle? Beyond that, why the interaction of the fifth particle should be neglected in the five-body bound state? It seems the authors even do not know how to connect the transition operators $\mathcal{T}^{123}$ and $\mathcal{T}^{12+34}$ to the two-body t-matrices, used in three- and four-body Faddeev–Yakubovsky equations. Moreover, what is the idea of the derivation of equations (3.3) and (3.4), when the authors have not used them in their calculations?

5. The mistakes in the numerical implementation:

   (a) The authors are completely confused for representation of the coupled Yakubovsky equations in momentum space. When they have introduced the basis states in a PW representation, *i.e.* the basis state given in equations (B.5) and (B.6), they have discussed the solution of the coupled integral equations in a 3D scheme. In equation (4.3), they have clearly presented the dimension of the eigenvalue problem in a 3D scheme, where each Yakubovsky component is dependent to 4 Jacobi momentum vectors, or nine independent variables including four variables for the magnitude of the Jacobi momentum vectors, three spherical angles, and two azimuthal angles. This confusion indicates that the authors have not done any calculations and what they have reported for binding energies are not the output of computer codes!

(b) The authors have mentioned that 10–20 iterations are needed to reach the convergence in the binding energies, but they have not presented any numerical evidence for that. We believe 7–10 iterations is quite enough to reach the convergence.

(c) The authors have not addressed any numerical details on the implementation of Padé approximation in the calculation of the transition operators $\mathcal{T}^{123}$ and $\mathcal{T}^{12+34}$.

6. The challenges in the numerical results:

(a) It is almost impossible to see how the results of five-body binding energies, just by considering two of five Yakubovsky components, are in agreement with the results obtained by other methods for full five-body solution. For example for Volkov potential, the difference between authors' result for five-body binding energy with other methods is about 1 MeV, which means the contribution of other three Yakubosky components as well as spin–isospin degrees of freedom which are not considered in the calculations is about 2%, which is almost impossible.

(b) By considering the difference between four- and five-body binding energies, the authors have concluded that the alpha–nucleon interaction is attractive and its value is about 13 MeV!

(i) First, the difference between four- and five-body binding energies obtained from four- and five-body calculations, using two-nucleon interactions, does not provide any information about the alpha–nucleon interaction.

(ii) Second, the authors expect that alpha–nucleon interaction be just a constant parameter, without any functional form!

(c) The authors have not presented the explicit form of the total five-body wave function and how it can be obtained from the Yakubovsky components.

(d) The reported five-body binding energy for MT-V potential is -44.30 MeV and they have verified the stability of the results as a function of the number of grid points in Table IV. It is obviously impossible to reach the convergence in the five-body binding energies with four significant digits using just 20 mesh points for the magnitude of the Jacobi momenta and 14 mesh points for the angle variables. As it is shown in Ref. [16], even in three–body calculations to reach the convergence in binding energy with four significant digits one needs to consider at least 40 mesh points for the magnitude of the Jacobi momenta and the angle variables.

In summary, by considering the major mistakes in the theory, formalism and the numerical implementation of the paper, we believe the authors have not done any calculations for the solution of the coupled Yakubovsky integral equations and similar to what they have published in the *retracted* papers [3, 4], they have reported fabricated results which are not trustable.

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