COMMENTS

Comments are short papers which comment on papers of other authors previously published in the **Physical Review**. *Each Comment should state clearly to which paper it refers and must be accompanied by a brief abstract. The same publication schedule as for regular articles is followed, and page proofs are sent to authors.*

Comment on "Unified treatment of bound-state and scattering problems"

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(Received 27 October 1988)

Recently Adhikari and Tomio have claimed that our *W*-matrix representation of the *T* matrix is a special case of previous results proposed by these authors. However, the early papers to which they refer do not go beyond the well-known Kowalski representation, and the more recent publication, which was the first of their papers to sketch the decisive modifications characteristic for the *W*-matrix representation, was submitted years after the first presentation of our approach. The priority claims by Adhikari and Tomio, therefore, are incorrect.

Preliminary remarks. Practical treatments of Faddeev-type equations are usually based on separable expansions of the subsystem *T* operators. In this context a splitting of the *T* matrix into *one* separable term and a negligibly small remainder appears as the most ideal choice, both with respect to practicality and physical interpretation. Generalizing a representation of the fully off-shell *T* matrix by Kowalski,[1] such a representation has been found by the present authors.[2] In test calculations, performed for neutron-deuteron scattering, the efficiency of this "*W*-matrix approach" was demonstrated for both elastic[3] and breakup[4] channels. The decisive new aspect lies in the treatment of the negative-energy region, i.e., of a region which is known to be quite essential in Faddeev calculations. In contrast to the Kowalski representation, the unrealistic analytic continuation of the on-shell *T* matrix in the separable part is replaced by an adequate propagator governed by an expression which can be considered as a *generalized Jost function*.

Priority claims. In a recent publication Adhikari and Tomio claim that our treatment is not original.[5] They, in fact, state that it represents a special case of representation supposedly contained already in a series of papers by Adhikari and Tomio.[6–8] In order to support this statement they write down what seems to be their previous result. Inspection of Refs. 6–8, however, proves that this is by no means true. In their earlier publications[6,7] these authors arrived at formulations which show some structural similarity with our final representation. But inevitably they end up with the original Kowalski form with all its inconvenient features when going to negative energies.

This holds true also with respect to the last paper of the series[8] (published after our work[2]). What is to be found there is again a recapitulation of their previous results, and consequently of the Kowalski representation. Now, however, these results are verbally reinterpreted so as to give all expressions a more general meaning and to accommodate the required modifications in the negative-energy region. This is done by just describing the relevant steps, without changing the definitions or the notation necessary in this context. In doing so, moreover, Adhikari and Tomio's terminology becomes very similar to that used in our paper.[2] They, for example, now recognize (and even emphasize this fact in the title of Ref. 8) that the denominator of the separable part can be considered a "Jost-like function," an observation essential for the adequate transition to negative energies (see above). To our knowledge, none of the papers before 1986 by these authors contain any statements to that effect.

The whole content of our approach was presented publicly in 1985 at the *Xth European Symposium on the Dynamics of Few-Body Systems* (Balatonfüred, Hungary), and a written version can be found in the proceedings of this meeting[9] (see also Ref. 10). It was in Refs. 9 and 10 that the importance of a generalization of the concept of Jost functions for the practical transition to negative energies was first recognized and emphasized. Neither Ref. 9 nor 10 are quoted in any of Adhikari and Tomio's papers.

Given this sequence of events it is obvious that Adhikari and Tomio's claim that we had failed to realize that our final *T*-matrix representation was a special case of Eq. (2.3) in Ref. 5 is without substance. We could not possibly have known in 1985 that these authors would suggest two years later (and would publish in 1988) a result identical to our extension of the Kowalski representation. We agree, however, that there is a "striking similarity"[5] between the papers.

Some details. In order to prevent further misunderstandings of the type propagated in Ref. 5, some more details are called for. As pointed out above, the essential modification of our T-matrix representation concerns the negative-energy region.[2] For $E<0$ the variable k, crucially entering the Kowalski representation,[1] is no longer identified with $\sqrt{E}$, but is chosen as an independent variational parameter which allows one to *optimize* the splitting of T into a separable part and a negligible remainder. At first glance this may appear a small modification, but it has dramatic consequences for the analyticity and unitarity properties of the separable part. It is just this seemingly small but in fact decisive difference which makes this separable approximation an almost ideal two-body input for three-body calculations.

To support their claim (which is inconsistent with the definitions given in these publications) that their earlier results have to be interpreted already in this way, Adhikari and Tomio refer to a brief paper in which they discuss the two-body bound state problem.[11] It is true that in this context they mention that k may be chosen independently of $\sqrt{E}$. But, in contrast to our Ref. 2, where we use a similar starting point to construct a negative-energy T-matrix representation optimal for three-body calculations, this paper exclusively deals with the solution of the bound-state problem within the usual wave-function approach. It does not contain the slightest hint on possible applications or modifications for the negative-energy T matrix. Despite the fact that it had no influence on our work, we mentioned this paper in Ref. 2 in an endeavor to give proper credit to all original sources however remotely connected with the subject matter of our article. One can hardly construe this, as did Adhikari and Tomio, as an argument for priority claims.

There is no doubt that these authors have made some observations which, being combined and properly extended, might have led them to such an efficient T-matrix representation as is achieved within our W-matrix approach. However, they never mentioned these implications before the publication of our Ref. 9.

Moreover, Adhikari and Tomio were not the first to observe that the variable k in the Kowalski-Noyes approach can be, and should be, chosen as a free parameter for $E<0$. This fact was emphasized and exploited long ago in a paper by E. O. Alt, P. Grassberger, and W. Sandhas.[12] A first attempt was made there to construct a modified T-matrix representation at negative energies, and to optimize it by varying k. If at all, our W-matrix representation was anticipated by this construction, and not by the comments of Ref. 11, which not only were published years later, but did not contain any hint on an adequate T-matrix representation. We, therefore, come to the following conclusions.

(i) To give up the relationship $k=\sqrt{E}$ in the Noyes-Kowalski representation at negative energies was suggested and successfully applied already in 1970.[12]

(ii) More than ten years later Adhikari and Tomio[11] made a similar comment. Their paper, however, was restricted to a pure bound state wave-function consideration. No attempt was made to modify the Kowalski T-matrix representation for $E<0$.

(iii) The other early papers by Adhikari and Tomio[6,7] concern the case $E>0$ and, therefore, require the choice $k=\sqrt{E}$. Moreover, they represent only minor modifications of the Kowalski approach,[1] which was quoted by us as the relevant original source.

(iv) The first systematic application and extension of the observation made in Ref. 12 is the W-matrix representation proposed in Refs. 2, 9, and 10.

(v) Adhikari and Tomio have suggested the same modification only after the publication of Ref. 9.

Additional comments. Beside these priority questions two further statements of the Adhikari-Tomio paper should be discussed.

It is true that we propose a special choice for the form factors, $\gamma_l(k,q)=(q/k)^l$, which were introduced originally as almost arbitrary functions in Ref. 1. In fact, an essential part of our paper[2] concerns the relevance of our choice. We only recall in this context the relationship to the coordinate space regularity condition and to traditional coordinate space treatments exhibited in our investigations. However, in the paper we also emphasize explicitly that our representation remains valid if instead of $(q/k)^l$ any other adequate choice of $\gamma_l(q,k)$ is inserted. Bearing in mind this freedom, our result is *not* a special case of Eq. (2.3) of Ref. 5, but is completely identical with this representation. The only difference is that our paper was published in 1986 while Adhikari and Tomio arrived at the same result in 1988, without suggesting our choice of $\gamma_l(q,k)$. It is a matter of logic that making an additional proposal for the choice of a function does not reduce a general paper to a special case.

A further misunderstanding is their argument that our choice may not be optimal with respect to an iterative solution of the two-body problem. This is a question completely irrelevant in our context. As emphasized again and again we are interested in an optimal two-body input to three-body calculations both with respect to the positive and the negative-energy region.

Even more serious are their comments about our definition of a generalized Jost function. The fact that our generalization does not have the same analyticity properties as the usual Jost function for local potentials—which we never claimed it did—does not in any way make it a less useful tool. Quite the contrary. The relevance of going over to the negative-energy region in a noncontinuous way was amply demonstrated by the success of our three-body applications.[3,4] These, perhaps somewhat unconventional, analytical properties of our generalized Jost function are a natural consequence of our method. The fact that they also are of extreme importance from a practical point of view clearly demonstrates that the unnecessarily restrictive definition of what might be called a "function" implied by Adhikari and Tomio's criticism is unwarranted.

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