Reply to Comment on ‘Comparison of potential models with the pp scattering data below 350 MeV’"

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Abstract

Replying to the above Comment by G.Q. Li and R. Mackeijit, we point out that the supposed "aws" in our comparison of NN potential models with the pp scattering data [Phys. Rev. C 47, 761 (1993)] are in reality not aws at all.

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In a recent paper [4], we compared a number of NN potential models with the pp scattering data. The purpose of that paper was to show that of the numerous potential models that have appeared in the literature, only a few give a reasonably good description of the pp data. It turns out that only models which have explicitly included the pp data in their treatment of the pp data. In general, potential models which have only been fitted to the np data give a very poor description of the pp data.

In a Comment on our paper, Li and Machleidt [1] claim to have found two flaws in our comparison of potential models with the pp scattering data. The first flaw seems to be that, according to Li and Machleidt, we are not allowed to compare the I = 1 part of an NN potential to the pp scattering data, when the \( ^1S_0 \) partial wave of that potential was fitted to the np scattering length. However, without this first flaw, we would never have been able to come to one of the conclusions of our paper, i.e., that only those potentials that were explicitly fitted to the pp scattering data give a reasonable description of these data [4].

As a next flaw in our paper, Li and Machleidt claim [2] that our comparison of NN potential models with the pp scattering data [1] can be twisted in the logic. In this connection we would like to point out that our comparison of NN potential models with the pp data was written three years after completion of our partial-wave analysis of the pp data [4]. At that moment we had not yet finished our study [3] of the np data. This means that it was at that moment too early to expect from the Nijmegen group a comparison of NN potentials with the np data.

We will use the remainder of this Reply to elaborate on these points and in turn address a few flaws in the Comment.

The Comment states as the most important rule, that a pp potential must only be confronted with the pp data, while an np potential must only be confronted with the np data [4], where their main criterion for the denomination pp or np is whether the \( ^1S_0 \) partial wave of the potential has been fitted to the pp or np scattering length, respectively. This rule sounds reasonable and noble, but is actually so often violated that it is presumptuous to call it a rule. In most practical situations (e.g., in three-body, nuclear matter, and pp bremsstrahlung calculations), one usually starts with some nuclear potential model to represent the NN interaction (for pp, np, and nn), disregarding the details whether its parameters were fitted to the pp and/or np data. In our paper [4], we explicitly demonstrated that this is incorrect.

Moreover, we feel that with this first flaw we are in good company, because we have it in common with Machleidt et al. in Ref. [4]. In this paper on the Bonn NN potential, it is stated that "one measures NN observables (and not phase shifts)," and that therefore the test of the quality of an NN interaction is the comparison with these data. For their model this is done in their Fig. 16. Whereas we do a quantitative comparison to the data, they merely do a qualitative comparison in their Fig. 16, from which they then claim that it can be "seen" that an "excellent description of the data is achieved" [3]. In this Fig. 16, these authors compare 10 times with the np data and 6 times with the pp data. A curious double standard: When Machleidt et al. [3] compare with the pp data, it is allowed, whereas when we compare with the pp data, it is called a flaw.

With respect to this Ref. [4], we would also like to point out that they write about the NN potential, NN interaction, NN observables," and not about the np potential, np interaction, np observables."
In spite of their own rule, Li and Machleidt themselves write that it is valid to confront an np potential with the pp data. The only changes one has to make are to include the Coulomb b force and to readjust the $^1S_0$ scattering length to its pp value [2]. They suggest this can be achieved by a relatively small change in one of the model parameters. As an example, they state that a change of the coupling constant by as little as 1% changes the $^2N_{data}$ from 641 to 2" [2]. They have in mind here the full Bonn potential [3]. For the reader not familiar with the field, we would like to show how misleading these arguments of Li and Machleidt are.

First of all, the authors of the Comment fail to mention that we already showed the above example to be inaccurate in the general case. In our paper [1] we show that when we replace the $^1S_0$ phase shift of the Argonne potential [4] (tied to the np scattering length) by the $^1S_0$ phase shift of the Nijmegen multi-energy analysis [5] (which roughly corresponds to having a model with "perfect" $^1S_0$ phase shifts), then the quality of the model improves considerably. However, the resulting $^2N_{data}$ is still rather large," demonstrating that the other phase shifts are not too good either" [6]. As is shown in Table I, the situation for the various coordinate-space Bonn potentials is worse. In this Table we show the effect of replacing the $^1S_0$ phase shifts of the potential models by the $^1S_0$ phase shift of the latest Nijmegen partial-wave analysis [4]. Clearly, the quality of these coordinate-space Bonn potentials is rather poor.

Secondly, in our potential comparison [1] we did not include the full Bonn np potential, but only the coordinate-space version OBEPR. The $^2N_{data}$ of this Bonn potential starts at 492 (and not at 641) and after in proving the $^1S_0$ phase shift by re-tuning, the $^2N_{data}$ drops to 7.1 (and not 2). The $^2N_{data}$ of this Bonn coordinate-space np potential OBEPR of 1987 is worse than that of the 25 years older Hamada-Johnston potential [5] of 1962. This in spite of the fact that the parameters of OBEPR were fitted [5] to arrive at a "realistic" description of the NN scattering data.

At this point we like to mention that we disagree with Li and Machleidt that the $^2$ of the coordinate-space Bonn potentials is of no interest. In a footnote they write that these potentials were merely constructed "to point out the deficiencies of such simple models" [2]. But when that is true, why at all bother to present a coordinate-space version of the one-boson-exchange version of the Bonn potential and then two years later again present [3] coordinate-space versions of both the Bonn A and Bonn B potentials? Moreover, as can be seen in Table II of our comparison [1], there exist other similar coordinate-space potential models which do give a reasonable description of the scattering data. We think that the users of the coordinate-space Bonn potentials should be warned, that these potentials do not give a reasonable description of the pp scattering data and, therefore, these potentials should not be used to calculate, e.g., pp bremsstrahlung or the binding energy of the triton.

In their footnote 10, Li and Machleidt had even fault with the energy range used by us. When these authors [5] fitted the Bonn potentials to the pp data they use the Nijmegen partial-wave analysis. Then in their Comment these authors proceed to point out, as if we did not already know, "that the pp data carry a very small error at low energies" [2] and, when included, add disproportionally to the total $^2$. Of course, we are well aware of these facts. This was precisely the reason why in our comparison [1] we also explicitly presented the results for the case where we removed these low-energy data. There is lack of knowledge about the NN data is shown when, without any supporting evidence, they state that the
real potential concept is strictly speaking wrong above 280(290 M eV). It may be o.k. to stretch the limit by a few M eV up to 300 M eV, but not beyond that\[3\]. This is absolutely incorrect. Because of bremsstrahlung, the real potential concept is strictly speaking wrong at any energy $E > 0$. When we include in our partial-wave analysis of the $0(350$ M eV $pp$ data the inelasticities as determined in a $0(500$ M eV $pp$ partial-wave analysis $[14]$), then we see a drop in $^2N_{\text{data}}$ of about 0.0006, which is truly negligible. This shows that the real potential concept is valid at least 350 M eV and definitely a fair distance beyond that energy as well. Another way to see this is to look at Table II of Ref. $[1]$. There it can be seen that most potentials $tt$ the $pp$ data in the interval 290(350 M eV better than over the whole energy range 2(300 M eV. A notable exception, however, is the full Bonn pp potential $[11]$, which in this 290(350 M eV region has a $^2N_{\text{data}}$ which is worse than average. When a potential does not $tt$ the data in this energy interval, then it is the potential which is at fault, and not the potential concept.

Let us conclude with some additional remarks on why we only made a comparison with the $np$ data. It is not surprising that the more recent potentials (e.g., the Argonne $[6]$ and Bonn $[5]$ potentials) were $tt$ only to the $np$ data. It is much easier to $tt$ a potential to the $np$ scattering data than it is to the $pp$ data, because in that case it is not necessary to go through all the difficulties of making all kinds of electromagnetic corrections. And as already stressed before, it is often assumed that with only a minor adjustment in one of the parameters of the model, that model will also give a good description of the $pp$ data. By explicitly confronting some of these $np$ potentials with the $pp$ data, we showed $[1]$ that this assumption is not always valid. Even re-adjusting the $^1S_0$ phase shift is often not good enough. So it is definitely not true to state that "a potential that $ts$ the $np$ phase shifts well, will automatically $tt$ the $pp$ phase shifts well", and that "there is no need to bring the $pp$ data into play" $[7]$.

Moreover, one has to bear in mind that the older potentials were $tt$ using old databases. For example, the Nijm 78 and Paris80 potentials $[12,13]$ were $tt$ to the 1969 Livermore database $[14]$. At that time the $pp$ data were already accurate enough to pin down the $I = 1$ partial waves. So well even that at the present time these Nijm 78 and Paris80 potentials still give a good description of the $pp$ data, including the more accurate recent data. On the other hand, the quality of the $np$ data was very poor at that time. In the last two decades the quality of the $np$ data has improved enormously, especially in providing more accurate bounds on the $I = 0$ partial waves. Hence, it is not so surprising that the old Nijm 78 and Paris80 potentials give a rather poor description of the present $np$ data, whereas the full Bonn87 potential is claimed to $ts$ these recent $np$ data much better. It should be noted, however, that by simply re-setting the parameters, the Nijm 78 and Paris80 potentials are likely to describe the $np$ data equally well as the $pp$ data. As a matter of fact, an updated $[15]$ version, Nijm 92, of the Nijm egen potential describes all $NN$ scattering data ($pp$ and $np$) with $^2N_{\text{data}} = 1.9$.

Therefore, it is incorrect to compare the $I = 0$ partial waves of the Nijm 78, Paris80, and Bonn87 potentials, and then (because of the large differences) come to the conclusion that $[6]$ "The more seriously and consistently meson theory is pursued, the better the results." The $2$ is the product of the number and kind of free parameters in the model and the extent one wants to invest in $tt$ing the data. The increase in computing power over the last decade, together with the ease of access to scattering data, to partial wave analyses, and
their improved accuracy, has made it possible to construct easily much better potentials \[15\] than the Nijm 78, Paris80, and Bonn87 potentials. This makes a farce of the incorrect claim by Macleid et al. that their better results indicate, that they apply meson theory more seriously and consistently" \[9\].

In summary, our earlier paper \[1\] gives a fair comparison of the quality of a number of NN potential models with respect to the pp scattering data. It clearly demonstrates the fact that potentials which have only been fitted to the np data do not automatically give a good description of the pp data, even after adjusting the \(^1S_0\) partial wave. In fact, most potential models do not. Therefore, our paper \[2\] perfectly serves its purpose: namely, to point out this misconception, and to warn other physicists to administer some care in using these models in practical calculations.
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TABLE I. $^2\text{N}$ data results in the 2(350 M eV) range for the coordinate-space OBEPR, Bonn A and Bonn B potentials. In the second line the $^1\text{S}_0$ of the potential is replaced by the $^1\text{S}_0$ of the Nijmegen analysis.

| $^3\text{S}_0$ | OBEPR | Bonn A | Bonn B |
|----------------|-------|--------|--------|
| potential      | 13.4  | 10.4   | 9.2    |
| analysis       | 8.1   | 6.8    | 5.5    |