Comments

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Comment on "Charge dependence of the nucleon-nucleon interaction due to the pion-mass difference"

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(Received 17 April 1987)

We analyze the treatment of Cheung and Machleidt and show that the essential physics is the same as in our previous approach. Thus their work using a detailed model confirms the essence of our findings.

In a recent paper, Cheung and Machleidt¹ (CM) made an elaborate evaluation of the effects of the $\pi^+-\pi^0$ mass difference on the 1S_0 NN scattering lengths. While we do not disagree with their numerical results, they did not appreciate a number of general clarifying features noticed in our earlier work.² Instead they criticize our approach on several points although the correct conclusion should be that their evaluation confirms our results.

There are two central issues which arise in any such calculation: formalism and dynamics. Let us first take up the question of formalism. CM start from a set of contributions, δV_{CIB} , to the charge-independent breaking (CIB) potential. They compute a set of corresponding contributions δa to the 1S_0 scattering length a . They arrange their calculations so that "the magnitudes of the δa 's. . . depend on the order in which the corresponding CIB mechanisms are introduced" and that "this clearly indicates that higher orders in δV_{CIB} are not negligible."

The formalism of Ref. 2 was explicitly constructed so as to avoid these undesirable features. Our starting point was the exact relation between scattering lengths (a, a') and their corresponding potentials (V, V'), as cited as Eq. (6) by CM in Ref. 1:

$$1/a' - 1/a = M \int_0^\infty dr (V - V') uu', \quad (1)$$

where u and u' are 1S_0 wave functions normalized so that $u(r) \rightarrow (1-r/a)$ at infinity. It is well known³ that such integrals in Eq. (1) are insensitive to the wave functions u and u' as long as the scattering lengths are much larger in magnitude than the range of the potentials. We replaced uu' by u^2 , where u is obtained from the Reid or Paris potentials. This introduces an overestimate of only about 2% of the total Δa even though the scattering lengths differ by nearly 30%. This precision is quite adequate for present purposes.

Within this approximation, the contributions of the different mechanisms for δV_{CIB} to the accurately known experimental difference, $(\bar{a}_{\text{exp}} - \sigma_{\text{exp}}^{\text{np}}) = [-18.2 - (-23.748)] = 5.5 \text{ fm}$, can be written as²

$$\Delta a = \bar{a}_{\text{exp}} a_{\text{exp}}^{\text{np}} \int_0^\infty dr \Delta V(r) u^2(r). \quad (2)$$

The result is an expression in which the various contributions are additive and treated on an equal footing. Therefore if the total perturbation potential ΔV is to result in the experimental value of Δa , the use of $\bar{a}_{\text{exp}} a_{\text{exp}}^{\text{np}}$ is a consistent procedure. When we included our estimate of the $\gamma\pi$ exchange term, $\Delta a_{\gamma\pi} = (1.1 \pm 0.4) \text{ fm}$, the full experimental value of Δa [$(5.5 \pm 0.3) \text{ fm}$] was reproduced. This confirmed the consistency of our analysis. However, even if one does not account for the full value of Δa , it is still correct to use our procedure. Another way to look at Eq. (2) is as follows: it is equivalent to using Eq. (1) to compute the difference in inverse scattering lengths. Experimentally this difference is 0.128 fm^{-1} . For example, our calculation of the one-pion exchange potential (OPEP) contribution to the integral on the right-hand side of Eq. (1) yields 0.081 fm^{-1} or 63% of the answer. This is the same percentage as results in comparing Δa of Eq. (2) to the total answer.

However, our use of Eq. (2) means that the individual contributions to Δa cannot be immediately compared with those of CM (δa). As an example, consider the CM calculation of the OPEP term. CM compute that $\delta a_{\text{OPEP}} = 2.80 \text{ fm}$ which they find "substantially smaller" than our result of $\Delta a_{\text{OPEP}} = 3.5 \text{ fm}$. However, a comparison of the integrals

$$M \int dr \Delta V(r)^{\text{OPEP}} uu' = \Delta(1/a)$$

reveals that they obtain 0.00733 fm^{-1} versus our value

of 0.00810 fm^{-1} and the Coon-Scadron value⁴ of 0.00797 fm^{-1} . This $\sim 10\%$ difference between the CM result and the other two evidently arises from the noncovariant perturbation theory treatment typical of the Bonn potential even on the OPEP level. Indeed, their version of OPEP is nonlocal and energy dependent.

Let us now discuss the two-pion exchange (TPE) contributions. To understand the charge dependence of the singlet scattering length one needs only to consider the $I=1$ exchanges between nucleons: charge dependence corresponds to lack of rotational invariance in isospin space. Thus the $I=0$ exchange contributions are irrelevant even though very large.

In the case at hand, the charge dependence arises from the $\pi^\pm - \pi^0$ mass difference in the TPE. One is concerned with obtaining a good description of the medium- and long-range potentials. These are dominated by low-mass exchanges for which the pion mass difference is particularly significant.

Which are the general criteria which must be fulfilled to control the computation of the TPE term?

1. The medium- and long-range parts of the $\tau_1 \cdot \tau_2$ potential must be reasonably consistent with observations.
2. The $I=1$ exchange potential is not sensitive to effects of chiral symmetry breaking that require pair suppression.
3. More technically, the nucleons should not be treated in the static approximation.

These criteria were the basis for our selection of the Partovi-Lomon (PL) approach to TPE. It is in reasonably good agreement with the medium- and long-range parts of the S -wave phenomenological potentials. Since only $I=1$ exchanges contribute to CIB, the failure of the

PL model to provide pair suppression in the $I=0$ exchange channel is irrelevant in the present context. The PL potential was explicitly constructed to avoid the static limit. While this potential is not complete, it satisfies the stated criteria reasonably well. As a consequence our evaluation of the PL TPE contribution to Δa ($=0.88 \text{ fm}$) provided the first realistic estimate of that significant but not dominant term.

CM based their calculation on the full Bonn potential. This guarantees consistency with scattering data. In this respect their work is far superior to ours. Note that in their approach, intermediate Δ 's provide the dominant TPE terms. Thus the dynamics is very different from those of PL. Chiral symmetry is implicitly satisfied in the Bonn potential, since Z graphs are totally ignored. CM find the total TPE contribution to be $\delta a = 0.85 \text{ fm}$, as compared to our $\Delta a = 0.88 \text{ fm}$. If we compare integrals of $M \int dr \Delta V^{\text{TPE}}_{uu'}$, their value is 0.00185 fm^{-1} as compared to our value of 0.00204 fm^{-1} , which is similar to the relative difference between the OPE terms. In any case, the results are essentially identical.

This agreement is no accident. Any nonpathological TPE potential satisfying the stated criteria gives approximately the same contribution to Δa . It is gratifying to see that our result based on general considerations gives the same result as that of a model that is consistent with a vast array of data.

It is true that not all of the effects have been computed with the highest possible precision. However, the CM calculation does substantiate our claim that most of the explanation for the charge dependence of the 1S_0 scattering lengths has been found.

¹C. Y. Cheung and R. Machleidt, Phys. Rev. C **34**, 1181 (1986).

²T. E. O. Ericson and G. A. Miller, Phys. Lett. **132B**, 32 (1983).

³E. M. Henley, in *Isospin in Nuclear Physics*, edited by D. H. Wilkinson (North-Holland, Amsterdam, 1969), p. 15.

⁴S. A. Coon and M. D. Scadron, Phys. Rev. C **26**, 2402 (1982).