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Reply to "Comment on 'Antiproton-proton partial-wave analysis below 925 MeV/c' "

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We reply to the Comment (preceding paper) by J.-M. Richard on our partial-wave analysis of low-energy $\bar{p}p$ scattering data [Phys. Rev. C **50**, 48 (1994)], in which we determined for the first time the $\bar{p}p$ phase parameters.

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In a recent paper [1], we presented an energy-dependent partial-wave analysis (PWA) of all antiproton-proton scattering data below 925 MeV/c in which we determined for the first time the $\bar{p}p$ phase parameters (see also Refs. [2–8]). The preceding Comment [9] claims that it is "presently impossible to determine the phase parameters," and expresses furthermore "concerns about the data selection." In the following, we will demonstrate that these claims are incorrect and that they result from lack of knowledge of state-of-the-art methods of PWA and of the $\bar{p}p$ database.

Let us first address the claim that from "simple counting arguments" one can see that it is "impossible to determine the phase parameters." This claim is very easy to refute, because already in Ref. [1] we did in fact determine these phase parameters. What, then, is wrong with the reasoning leading to such an erroneous claim? The answer [8] is really quite simple: Our energy-dependent partial-wave analyses have been confused with single-energy amplitude analyses. What the Comment should have said is that from simple counting arguments one can see that it is impossible to perform amplitude analyses.

In a single-energy amplitude analysis one must determine the five complex amplitudes $a(\theta)$, $b(\theta)$, $c(\theta)$, $d(\theta)$, and $e(\theta)$, as mentioned in [9], for *every* angle θ at a fixed energy. Therefore one needs to have at least nine independent experimental quantities as a function of θ at that energy. But this many independent experiments are not available, neither in the $\bar{p}p$ case nor in the better known NN case. Consequently, amplitude analyses cannot be done and so are not done for the low-energy NN case. Obviously, then, for the low-energy $\bar{p}p$ case amplitude analyses are also impossible. This is precisely the reason why we did not try to perform amplitude analyses, but did perform energy-dependent PWA's of the low-energy NN [10–12] and $\bar{p}p$ data [1–3]. In such PWA's one must determine a finite number of phase shifts as a function of the energy. In fact, from the 3543 $\bar{p}p$ data we needed to determine only 30 parameters [1]. That this is possible should not come as a big surprise. Counting arguments in PWA's are different from "simple counting arguments" in amplitude analyses.

It is of course well known that energy-independent (single-energy) analyses can suffer from multiple solutions or so-called "phase-shift ambiguities," when not all independent experiments are available. Simple examples are the Fermi-Yang and Minami ambiguities in pion-nucleon scattering. Usually, it is easy to resolve these ambiguities from considerations about the required energy dependence of the partial waves. Not surprisingly, therefore, this problem will in general disappear in energy-dependent (multienergy) analyses, especially when theoretical constraints are imposed, such as the one-pion-exchange tail in our case. The claim that our solution is "just one among a thousand others" remains unsubstantiated and is incorrect. We claim that our energy-dependent solution is, in fact, essentially *unique*.

In the Comment, the opinion is expressed that "there are not enough data to fix the $\bar{p}p$ partial waves." Unfortunately, no mention is made of the number that will be sufficient. In reply, it is instructive to compare the present situation to the early days of NN PWA's. Let us take the, now 30-year-old, Livermore IV energy-dependent PWA of the low-energy NN data [13]. At that time (1965) the NN database below 350 MeV consisted, after selection, of 704 NN data, which were fitted with 58 parameters. We see that our PWA of the $\bar{p}p$ data uses 5 times as many data as were used in this 1965 Livermore NN PWA. In fact, the number of $\bar{p}p$ data (3543) is comparable to the number (4301) of NN data below 350 MeV used in the 1993 Nijmegen NN PWA [12]. From this, it must be obvious that the number of data cannot really be a problem.

It is implied in the Comment that the $\bar{p}p$ database is not varied enough to perform a PWA, because it contains basically only differential cross sections and polarizations, but almost no double- and triple-scattering observables ("spin data"). In this respect, however, the present situation for the $\bar{p}p$ case is quite similar to the np situation in 1965. The np database consisted at that time of 341 data, but these were practically only cross sections and analyzing-power data. At the time of the Livermore X PWA [14] this situation had not really improved. Nevertheless, it is clear that energy-dependent PWA's of the np data were feasible already at that time.

When we compare our $\bar{p}p$ PWA to the pioneering NN PWA's [13,14] of the 1960s, then we see that the present-day $\bar{p}p$ database is better than the np database at that time, but obviously not as varied as the pp database. Moreover, our method of PWA is more sophisticated than the ones used in the NN PWA's of the 1960s (and even the 1970s).

To demonstrate explicitly that cross sections and polarization (analyzing-power) data can be sufficient for a decent PWA [7], we considered the pp case, where the spin data are available. Below 350 MeV there are in the Nijmegen 1993 NN database [12] 1787 pp data, 1381 cross sections and polarizations and 406 spin data (A_{ii}, C_{nn}, D, D_t, R, R', A, A'). When we perform a PWA using only these 1381 cross sections and polarizations, we find $\chi^2_{\rm min} = 1403$. This number should be compared with $\chi^2 = 1407$ obtained on these same data in the Nijmegen 1993 PWA [12], which was fitted to *all* 1787 pp data. The solution obtained from the 1381 nonspin data gives *only* $\chi^2 = 800$ on the 406 spin data. For the total pp database this solution has therefore a still quite acceptable $\chi^2/N_{\rm data} = 1.23$. This nice result is, according to us, due to our very computer-intensive method of PWA, containing essentially model-independent theoretical input, such as the long-range spin-dependent electromagnetic interaction and the one-pion-exchange tail. These are included exactly in our treatment.

The Comment claims that if the database contains almost no spin data, accurate information about for instance the tensor force "will remain inaccessible." We can now easily demonstrate that this claim is also incorrect. The pp PWA using only the nonspin data pins down the tensor combination of the ^3P waves at 100 MeV as $\Delta_T = -4.856°(23)$. The Nijmegen 1993 PWA, which also uses all spin data, gives $\Delta_T = -4.840°(16)$. At 215 MeV the numbers are $\Delta_T = -5.65°(12)$ without spin data and $\Delta_T = -5.50°(3)$ with spin data included. One sees that the solution remains quite stable when the spin data are included: The changes in Δ_T are within about one standard deviation. The spin data do, of course, improve the accuracy, but one can see that just the cross sections and polarizations already determine Δ_T in pp to 0.5% at 100 MeV and to about 2% at 215 MeV. This is clear evidence in favor of our claim that the presence of strong $\overline{N}N$ tensor forces has already been clearly demonstrated in our $\bar{p}p$ PWA. One should therefore look instead for experiments that will improve our PWA by providing a better determination of, for example, the singlet phases.

We can summarize this part of our Reply as follows. As claimed in Ref. [1], we have performed an energy-dependent PWA of the $\bar{p}p$ data below 925 MeV/c, using a state-of-the-art method of analysis. The accuracy of the solution should not be compared to that of NN PWA's of the 1990s, but it is comparable to that of early pp PWA's and probably better than that of early np PWA's. There remains, of course, room for improvement, for instance achievable by accurate experiments with a polarized antiproton beam.

Our method of data handling is also criticized in the Comment. We point out that in our $\bar{p}p$ PWA we used exactly the same methods as were used in the analogous NN case. In the NN field it is largely agreed upon what is the correct database and which data should not be included in this database. For example, the low-energy pp database of Arndt and collaborators at VPI&SU is practically the same as that of the Nijmegen group. Both groups are presently trying to agree on what is the database for the low-energy np data. To build the NN database use has been made of the experience of the many groups that performed NN PWA's in the past. To arrive at a satisfactory $\bar{p}p$ database [1] we had to do a lot of pioneering. Fortunately we could profit enormously from the expertise and from the software acquired by the Nijmegen group while performing PWA's of the NN data. We used only statistical criteria to decide if data sets should, or should not, be included in our database. Data selection is not "premature" at all. On the contrary, it is necessary in order to obtain a statistically correct database. Only then can the rules of statistics be fully applied. It is therefore important for further progress.

We agree with the Comment that it is "difficult for theorists to decide which data are right and which are wrong." But that is not what we have been doing. We do not judge if experiments are right or wrong. We only decide if data are statistically acceptable, yes or no. In our data selection we only apply carefully the rules of statistics, using our PWA as a tool. It is a must in a PWA to do data selection "in the same paper where the parameters are fitted." Such procedures were already followed by the theorists performing the Livermore NN PWA's.

It is unfortunate that it turns out that the elastic differential cross sections measured by the different LEAR collaborations are inconsistent among themselves. Some of the experimentalists are aware of this fact [15] as well as some of the other problems with the data. In fact, it turned out that on statistical grounds we had to omit most of these elastic differential cross sections from our database. But we fail to see in what way it can be "embarrassing" to us to "disregard some of the data obtained with the high-intensity and high-resolution $\bar{p}$ beam of the LEAR facility at CERN," as if the intensity and resolution of any beam can put the data taken with it beyond question. We point out that we did not simply "disregard" the data in question. We have first scrutinized them at length using sound statistical criteria, as discussed extensively in Refs. [1,8].

Finally, we stress that we have investigated *all* LEAR scattering data below 925 MeV/c. Apart from these problematic elastic differential cross sections almost all of the data are satisfactory to excellent. In view of this experimental progress and as a result of our energy-dependent PWA, our knowledge of the scattering amplitude is now such that we can make *reliable* predictions for all observables at any momentum below 925 MeV/c. This is not a "miracle," but the result of progress, both experimentally and theoretically, and of hard work.

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