Dear Michael

We were most interested to receive your output, especially your solution 2. We believe that our procedures (as they now stand) would not allow us to reach this minimum in a systematic way. We could reach it only following a lucky guess for starting point. We can see the reason why this is so, and I shall enclose some notes discussing this. Whether solution 2 gives the lowest of all minima remains an unsettled question, I think.

The trouble with our procedure does not lie in the question of analyticity, we think, because we do not make any use of analyticity in our minimization program. Does your minimization program involve calculating gradients from an analytical formula which you have given to the computer?

Our situation is well illustrated by the enclosed Fig. 1. When the starting value of \( a \) corresponds to being in the upper parabola there, to the right of the cross-over point, our program always makes the point run downhill to the corresponding minimum, which is of course a good local minimum. We can get out of this minimum only by taking a sufficiently large random step. The program does try this but doesn't manage to get us away from this local minimum. A random starting point could get us into the lower parabola, and to the lower minimum, but with 11 variables it is clear that we cannot explore all regions of this 11-dimensional space in this way.

The only systematic procedure which I can see is to carry out the minimization for all possible assignments for the partial waves where resonances are known. Looking over the present printout, and the partial waves where is freedom of assignment, I find that (working back from the end of the printout of states) we would need \( 4 \times 6 \times 3 \times 3 \times 2 \times 2 \times 2 = 5184 \) separate minimization runs to test all possibilities. Of course, most of these would give enormously high-lying minima. This is not a practical proposition, anyway, although fixed assignments could be tried for a small number of parameters, where there could conceivably be ambiguity in the assignment, as you have done for the uppermost \( AP05 \) state. In the present output, the only places where a different assignment could be considered are \( AP03 \) where 1873 MeV could be identified with the lowest level (at present at 1793 MeV, 80 MeV away), and \( AD03 \) where 1689 could be identified with the uppermost level (at 1785 MeV now, so 96 MeV away). The latter possibility appears most unlikely, but the former might be considered, just to see what happens.

The Soln. 1 and Soln. 2 parameters differ greatly only for the spin-orbit parameters, and there the change is quite significant. It is interesting to compare the two sets of calculated levels. The fit to the input levels is excellent for both cases - the difference between soln. 1 and soln. 2 is generally less than the empirical uncertainty in the level position. The differences which exist are in the levels for which there is no empirical data, as is only to be expected. Consider the \( E^* \) states:
(i) $E^*(1940)$ is generally identified as $\Sigma 15$ and that is quite natural here. Soln.1 gives 1914 MeV, Soln.2 gives 1909, for this level, which is not so good.

(ii) $E^*(1820)$ is usually identified as $\Sigma 13$. Here solns. 1 and 2 give low values, 1771 and 1791 MeV respectively. There are many other calculated levels which could contribute to this bump.

(iii) $E^*(2030)$ and $E^*(2330)$ both have strong. What spin-parities? I suppose that $\Sigma 15$ and $\Sigma 17$ are the most likely possibilities. Soln.1 gives 1991 and 2126 MeV for these levels; soln.2 gives 1989 and 2124 MeV. The agreement between the soln.1 and soln.2 predictions is remarkable, since it does involve an extrapolation.

As pointed out in my talk at the Triangle Meeting in Czechoslovakia (1973), certain combinations of the spin-orbit potentials may be well-determined by the data, while others are poorly determined. For example,

$$R = \frac{15}{35} \cdot \frac{1}{3} (15, 8, 3) + \frac{15}{189} (8, 3)_s$$

is determined by the $N^4$ data. For soln.1, $R$ takes the value $+27.7$ MeV; for soln.2, it takes the value $-22.4$ MeV. However this is not the only difference between the spin-orbit terms for soln.1 and 2; for example, the difference $\frac{15}{35} (15, 8, 3) - \frac{15}{189} (8, 3)_s$ is substantial and of reverse sign, from $-52$ to $+31.5$ MeV (concerning what is or is not closely established) in going from soln.1 to soln.2. This question deserves further thought.

I think we should keep to the same input data until we understand completely what the program is doing, and we are convinced that we do have the lowest minimum. However, we might think about other possible levels. For example, the $D13(1590)$ and $D13(1660)$. These don't fit too naturally with our present output 1629 and 1704 for soln.1, and 1615 and 1713 for soln.2, but it is probably possible to accommodate without too great an increase in $\chi^2$. Of course the $D13(1590)$ assignment is very uncertain. The BNL data on $\bar{K}p$ shows a strong peak and I don't think that will go away, but there is only slender evidence for the assignment $D13$. $D13(1660)$ is in much better condition. If we accept only $D13(1660)$, then we have the same ambiguity situation (whether it is the bottom or the middle level) as you have pointed out for the $F03$ state. This should be considered further, at some time.

Where did your starting parameters for soln.1 come from? Probably they were the location of the minimum you found first, starting with our parameter set.

Could you please send us two reprints of your article in Nuovo Cimento. We do not have this journal right at hand here, and we would both find a copy useful to have.

Yours sincerely

[Signatures]

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