C_1 as for the s -waves i.e. $C_1 = -1.0$. The corresponding curves are shown in Fig. 8. $f_{d^{1/2}}^{(1/2)} - f_{d^{3/2}}^{(3/2)}$ is almost zero experimentally and any attempt to fit the data would depend on a good evaluation of the nucleon term in this energy region.

(d) f -waves: Experimentally there seems to be evidence for the resonance around 900 MeV to be in the $f_{s^{1/2}}^{1/2}$ wave and although there are no f wave

phase shifts with which to compare our theory we might hope to be able to show that the theory gives the right conditions for a resonance to occur in that state. This is indeed so.

The pion-pion interaction gives its largest contribution to $f_{s^{1/2}}^{1/2}$ and this contribution is positive. This is known to be an indication that a resonance should develop in that state.

LIST OF REFERENCES

Editor's Note: These are general references considered by the authors to be pertinent to this article.

- Chew, G. and Mandelstam, S. Phys. Rev. **119**, p. 467 (1960).
 Frazer, W. and Fulco, J. Phys. Rev. Letters **2**, p. 365 (1959).
 Frazer, W. and Fulco, J. Phys. Rev. **117**, p. 1609 (1960).
 Cini, M. and Fubini, S. Annals of Physics **3**, p. 352 (1960).
 Amati, Leader and Vitale. Nuovo Cimento **17**, p. 68 (1960).
 Amati, Leader and Vitale. (To be published in Nuovo Cimento.)
 Gourdin, M. and Martin, A. Nuovo Cimento **16**, p. 78 (1960).
 Gourdin, M. and Martin, A. (To be published in Nuovo Cimento.)
 Gourdin, Lurié and Martin. (To be published in Nuovo Cimento.)
 Ferrari, De Tollis, Munczek. (To be published in Nuovo Cimento.)
 Bowcock, Cottingham and Lurié. Nuovo Cimento **16**, p. 918 (1960).

THEORY OF LOW ENERGY NUCLEON-NUCLEON SCATTERING

D. Amati, E. Leader and B. Vitale

CERN, Geneva, Switzerland

(presented by D. Amati)

A dispersion theoretical approach is used to study nucleon-nucleon scattering up to energies for which the inelastic processes are rather weak (i.e. $E \lesssim 450$ MeV.).

The nucleon spin and isospin are taken into account by developing the scattering amplitude in invariants (that depend on γ and τ matrices) so that their coefficients depend only on the kinematical variables w , t and $\bar{t}$ (physically, on the energy and scattering angle). The choice of the appropriate

set of invariants is based on perturbative arguments, which for the moment are the essential basis of the whole Mandelstam representation approach. It is in fact shown that if one chooses the 5 usual Fermi invariants (or linear combinations of them) the invariant amplitudes satisfy Mandelstam representation up to fourth order. In order to have also simple crossing properties ($t \leftrightarrow \bar{t}$) we choose then a particular set of linear combinations of Fermi invariants. The

utilization of such invariants was subsequently justified in another way by Goldberger, Grisaru, MacDowell and Wong.

We are left then with 10 scalar amplitudes, functions of w , t and $\bar{t}$, for which we suppose that a Mandelstam representation is valid. We begin by writing a dispersion relation for them for fixed energy. The fact that this is the appropriate manner of introducing the 2π contribution was pointed out one year ago by Cini and Fubini. Essentially

$$\operatorname{Re} C_j(\omega, t, \bar{t}) = \frac{g^2}{\mu^2 - t} + \frac{1}{\pi} \int_{4\mu^2}^{\infty} \frac{\rho_j(\omega, t')}{t' - t} dt' \pm (t \rightarrow \bar{t}) \quad (1)$$

in which the singularities in t and $\bar{t}$ (or in $\cos \theta$ if we want) appear explicitly. Let us distinguish the various contributions to such a dispersion relation. (They are sketched in Fig. 1 in terms of $\cos \theta = \frac{t - \bar{t}}{4k^2}$.)

(a) The pole term: it appears at $t = \mu^2$ ($\bar{t} = \mu^2$). This has been known for a long time.

(b) The two-pion contribution: this begins at $t = 4\mu^2$ ($\bar{t} = 4\mu^2$) and shall be calculated in terms of the amplitudes for the process $N\bar{N} \rightarrow \pi\pi$ by making use of unitarity.

(c) Higher contributions. These start at $t = 9\mu^2$ ($\bar{t} = 9\mu^2$) and come from intermediate states with masses higher than 3μ . We have no way, for the moment, to calculate explicitly such contributions. They contribute, however, for values of t' in the integral of Eq. (1) rather far from the physical region of t , so that they shall give rise to terms that are weakly dependent on t and $\bar{t}$ (or $\cos \theta$). Let us say schematically: in a Legendre polynomial expansion higher contri-

butions $= \sum c_n(\omega) P_n(\cos \theta)$, and the number of c_n needed shall be small.

To compare with preceeding information, let us note that in the development in partial waves of the contributions $a+b+c$ (i.e. the total scattering amplitude) we need a lot of waves: up to $l \sim 6$ for 310 MeV. If one considers separately the pole contributions (a) and develops only $b+c$ in partial waves, at 310 MeV, it is sufficient to consider up to $l \sim 3$ as has been shown by MacGregor, Moravcsik and Stapp. By considering explicitly $a+b$ and developing c alone it is reasonable to expect that the consideration of s - and p -waves shall be sufficient.

If there is a strong resonance in the 3π state, as proposed by Chew, it would be important, of course, to consider its influence in our problem. This effect can be taken into account by adding phenomenologically an extra "pole" term whose parameters can be obtained from the analysis of the isoscalar nucleon form factor and pion photoproduction.

The contribution b , as said before, is given in terms of the amplitudes for $N\bar{N} \rightarrow 2\pi$, for which we take the representation given by Bowcock, Cottingham and Lurié that Cini has already reported and which applies equally well to $N\bar{N} \rightarrow \pi\pi$ as to πN scattering. It contains mainly three contributions:

- (1) the pole term,
- (2) the πN rescattering corrections (mainly (3,3) resonance),
- (3) the term coming from $\pi\pi$ interaction.

As you have heard, such a representation fits very well πN scattering experiments up to 350 MeV. The fit allows, by the way, to obtain the parameters for

Fig. 1 Cuts and poles for the variables t and $\bar{t}$.

the $\pi\pi$ interaction (that is supposed to be in p -wave). As discussed by Cini those parameters also give good results for the isovector nucleon form factor and for photoproduction.

By using the BCL results, the contribution b (2π contribution) is then explicitly calculated. It is interesting to note from the results that the imaginary part coming from b coincides with the fourth order perturbative result with renormalized coupling constant. It is amusing to verify that that dispersion theory gives a precise meaning not only to second order but also to the fourth.

We obtain the partial wave amplitudes by combining our invariant amplitudes so as to have the usual singlet-triplet representation, and by projecting them over the corresponding Legendre associated functions. What we discussed before allows us immediately to distinguish the partial wave amplitudes in three categories.

- (1) The super high waves. These are essentially given by the pole. They are practically real, and are given in the work of Cziffra, MacGregor, Moravcsik, and Stapp. At 310 MeV these correspond to $l \gtrsim 4$.
- (2) The high waves. These come essentially from contributions a and b (pole and 2π terms). Their imaginary part is practically given by the fourth order perturbative calculation. At 310 MeV these correspond to $l \gtrsim 2$.
- (3) Low waves, for which the contribution c (higher singularities) is important. These are essentially s - and p -waves at 310 MeV.

By projecting over partial waves our previous results we have then for the high waves a set of explicit formulae without extra parameters besides the

aforementioned ones that are determined from πN scattering.

For the low waves, the procedure is more complicated. Our representation allows us to write for the partial wave amplitudes (multiplied by specific coefficients so as to avoid some kinematical singularities) dispersion relations with the energy (or k^2) as variable.

The contribution of the left hand cut ($k^2 < 0$) is calculated in terms of the pole and the 2π contribution already discussed; what remains of it shall contribute essentially with a constant (scattering length) or a low polynomial in the energy. By virtue of unitarity the right cut is given in terms of the amplitude itself: this sort of integral equations has been studied recently to some extent by Noyes with the N/D method. Their solution can be obtained in terms of the left cut residues that we have here explicitly. Then the energy dependence of the low partial wave amplitudes can be obtained in terms of known parameters.

We have no hope to obtain from the theory the s -wave scattering length but we expect to be able to get out the effective range. Cini, Fubini and Stanghellini, and Noyes and Wong showed more than a year ago that the pole term was sufficient to understand the shape dependent parameter of s -wave p - p scattering; the calculation of the next singularities must therefore give us information also on the effective range.

The numerical calculation of high waves is in progress as well as the solution of the integral equations for low waves (singlet s for the moment). The expressions are rather complicated, so it takes some time to have the computing program working. We expect soon, however, to give curves for the partial waves in terms of the energy. Thus we hope to be able to fit the experimental results with only a few scattering lengths as parameters.

DISCUSSION

BRUECKNER: Is MacDowell still here? One of the old fashioned reasons for introducing a potential was that one can go from knowledge of the two body scattering to the properties of the triton. Is there any way that you can see of doing this using dispersion techniques?

MACDOWELL: I do not know of any very reliable way of dealing with problems involving three bodies with dispersion relations.

CUTKOSKY: I should like to comment that I think I see an approach. Of course there are many

complications that I am worried about but I think I see a clear way toward a dispersion-theoretic treatment of the three-body problem and the four-body problem.

LÉVY: I wanted to make this comment on whether one can do without the potential mostly to stimulate discussion. I think the dispersion relations enthusiasts have been right in criticizing the concept of potential. We have been—I mean lots of people have been relying too much on this. Saying that you do not need a potential at all, that you just calculate a scattering matrix, is probably going a little bit too far, at least for the time being. I think that for the time being the concept of potential is still a very useful tool to correlate data, or, for example, as Brueckner points out, to go from the two-body problem to the nuclear matter problem.

Also I think it will be a long time before dispersion relations calculate photodisintegration of the deuteron including final state interactions, and other things—I mean detailed properties of bound states. Now the thing I think I have understood in the talks of MacDowell and Amati is that the potential or the force between two particles is very closely related to the discontinuity in the left hand cut of the partial waves and we see, in fact, in the last analysis of Amati, which is very clear, how this will come about. Clearly, according to this approach, something like the spin-orbit interaction or the hard core, which are needed to fix the data, will have to come from the last term and obviously something must be done about it. I am not sure that just putting one or two poles into the cut will do the trick. Perhaps I would rather like the idea of introducing a three-pion resonance because this will be something like a vector meson and perhaps will represent the combination of the core and the singular spin-orbit potential—both of them will probably come in just from this contribution.

AMATI: Concerning Lévy's feeling, I would like to explain briefly how we are constructing a nucleon-nucleon potential from the theory I have reported. The potential comes out very clearly just generalizing the approach that Charap and Fubini proposed a year ago. Our potential will have the general form

$$V = V_c(r) + V_{so}(r)(\mathbf{L} \cdot \mathbf{S}) + V_{sos}(r)(\mathbf{L} \cdot \mathbf{S})^2 + V_T(r)\tau_{12} + V_{ss}(r)\boldsymbol{\sigma}_1 \cdot \boldsymbol{\sigma}_2 \quad (1)$$

and is defined so as to reproduce the scattering amplitude that we have found in the dispersion-theoretical approach.

We can then show that every $V_i(r)$ will be given by

$$V_i(r) = a_i \frac{e^{-\mu r}}{r} + \int_{\mu}^{\infty} \mathcal{H}_i(\sigma) \frac{e^{-\sigma r}}{r} \quad (2)$$

where the a_i are fixed numerical coefficients. The functions $\mathcal{H}_i(\sigma)$ are then obtained as linear combinations of

$$\rho_j - \rho_j^{\text{It. Yuk.}}$$

where the ρ_j are the weight functions mentioned in the report and explicitly calculated. The $\rho_j^{\text{It. Yuk.}}$ are the weight functions of the iterated Yukawa potential written in a parametric representation and duly developed in terms of invariants. In ρ_j we separate several contributions: The fourth order contribution, $\rho^{(4)}$, coming from Fig. 1; the contribution from the crossed diagram (Fig. 2) which gives ρ^{cr} ; the pion nucleon rescattering contributions, $\rho^{\pi N}$; and the $\rho^{\pi\pi}$ contribution coming from π - π interaction. Thus our $\mathcal{H}_i$ should be proportional to

$$\rho_j^{(4)} - \rho_j^{\text{It. Yuk.}} + \rho_j^{(4)\text{cr.}} + \rho_j^{\pi N} + \rho^{\pi\pi}. \quad (3)$$

It is essential, and easy to show, that the $V_i(r)$ so obtained are effectively only weakly energy dependent

Fig. 1 Uncrossed fourth order N - N scattering diagram contributing to $\rho^{(4)}$.

Fig. 2 Crossed fourth order $N-N$ scattering diagram contributing to g^{cr} .

(below the inelastic threshold) so they can be defined in the adiabatic limit $k^2 \rightarrow 0$. In fact the strong energy dependence of the $\rho^{(4)}$ is just canceled out by $\rho^{\text{It. Yuk.}}$ as can be easily seen from general arguments and as has been shown explicitly by Charap and Tausner in a recent paper. In fact $\rho^{(4)}$ and $\rho^{\text{It. Yuk.}}$ differ only by kinematical coefficients in the parametric integrals defining them. The rest of the ρ_j of Eq. (3) do not give rise by themselves to a strong energy dependence. It is physically clear why in Eq. (3) the $\rho^{\text{It. Yuk.}}$ appears. In fact, the Yukawa term contained in Eq. (2) gets automatically iterated when inserted in the Schrödinger equation, so that the iteration must be subtracted from the rest of the contribution so as to avoid counting it twice. We have then from Eq. (2) and Eq. (3) an explicit expression for the potential in terms of the parameters we discussed in the dispersion approach and which are known from the πN scattering analysis. It is amusing to see how the (3,3) resonance, or the $\pi-\pi$ resonance, generates the corresponding potential term. We have not spoken about the exchange potentials; but they come in from the exchange terms ($t \rightarrow \bar{t}$) of Eq. (1) in my report in a way analogous to that of the ordinary potential. It is clear, however, that the potential obtained in this way is not valid at small distances. In fact, for ρ we have substituted the two-pion contribution (the b of the report) and neglected the lighter contributions (c). For instance we shall

certainly not get any indication on the hard core. It influences the scattering lengths and we knew from the beginning that it was not our aim to explain them. We have therefore from our dispersion theory a clear relation between our contributions a , b and c : the usual tails, intermediate, and inner potentials.

FRAZER: I would just like to briefly clarify the difference which Cini brought out between the analysis they did of the nucleon structure and the analysis that Fulco and I did. I think the basic point is that they were more conservative and did not try to calculate the anomalous magnetic moment. We assumed that the two pion contribution would dominate the anomalous magnetic moment. Now it may be, if there are unpleasant experimental facts to face, such as if Frautschi's discrepancy persists—it may be that we would have to go to the more conservative approach. I think that is the point of difference.

BLANKENBECLER: I would like to comment about Brueckner's question about bound states. There is a way of discussing bound states by discussing vertex functions which avoids going to the second sheet. This was discussed by Roy Cook and myself (Phys. Rev., in press). In essence one discusses a three-particle vertex in which the heaviest particle around is the bound state you are interested in. By dispersing this you can reproduce an equation equivalent to the Bethe-Salpeter equation. It is interesting that in the non-relativistic limit this will reduce to the Schrödinger equation if there are anomalous thresholds around to "drive" the thing; otherwise you can not reduce it.

OPPENHEIMER: The early hope of the Yukawa theory was, of course, that one could treat it in analogy with electrodynamics, but that evaporated very fast. The next hope was that one could use the smallness of the mass ratio to produce a kind of quasi-static approximation. The ghost of this lies in the classification of the regions; the outer regions where the quasi-static approximation works, the middle region where it is hopeful but does not work, and the inner region where something entirely different has to be put in, in the form of phenomenology. I think that the basic hope (though I do not want to speak for everyone) of both the Berkeley and Geneva-Rome programs is that this ratio of masses in the form of distinction between high and low thresholds can be

maintained in dispersion theory, and we have heard a good deal today that casts some doubt on this; at least in the pion-pion problem it does not work out straightforwardly. We see in Amati's report a revival of the hope in the nucleon-nucleon case, and it will be most interesting to know whether that looks like the very odd results of scattering experiments. But I think there is more connection to the question of the usefulness of potentials and the usefulness, not of the Mandelstam representation, but of its practical application in these programs, than one would suppose from Brueckner's question.

LÉVY (to MacDowell) : Have you proved or inferred a Mandelstam representation for your choice of amplitudes?

MACDOWELL : We have inferred it. If the Mandelstam assumption is correct these amplitudes will have a Mandelstam representation.

WONG : I would like to make a remark regarding the choice of the invariant amplitudes in the nucleon-nucleon problem. It is actually proven to all orders in perturbation theory that this particular choice (given by MacDowell) has only the Mandelstam singularities, provided the scalar problem has only the Mandelstam singularities.

BRUECKNER : You said you thought that possibly you could predict the effective ranges, putting in the scattering lengths as subtraction constants. It is well known that the effective range depends very much on the short-range repulsion which is present in the potential description. Do you see any indication from the dispersion relations that you will find such effective repulsions?

MACDOWELL : I think this is a very interesting question, but one which cannot be answered until we solve the equation. In fact, our hope is that the short-range forces, at least in part, are included in the subtraction constant. When you make a subtraction in the dispersion equation, convergence factors are introduced that cut down the effect of short-range forces, and these are absorbed in the subtraction constant. Now whether one subtraction is sufficient to describe this effect we do not know.

OPPENHEIMER : The effects of the constant are not ambiguous in dispersion theory; the hardness of the core is very ambiguous in potential theory.

MACDOWELL : Yes. In potential theory you want a potential that does not depend on energy so you do not know how to absorb all these short-range forces into something. But here in the dispersion approach you know exactly how you can avoid or at least decrease the effect of the short-range force in a very definite way.

LÉVY : The work of the Berkeley group to which Amati referred would be the work in which one takes $b+c$ from phenomenology and one takes the pole terms exactly. This now gives, I believe, a unique set of phase shifts. Independently both Berkeley and Yale got that result. So now there is something we can really believe in up to 310 MeV. But I think the inclusion of the two-pion contribution is going to be extremely crucial. If the agreement is not improved, or gets worse, then I think it will reflect not on the Mandelstam representation but on its practical use and in particular on the way that the pion-pion interaction has been treated.