CERN-TH.5522/89

REVIEW OF ELASTIC SCATTERING

André Martin
CERN -- Geneva

ABSTRACT

In this partly historical review we set the stage for the new results presented at the 3rd Blois Meeting in Evanston and at Fermilab. We also suggest experiments with existing or future machines (like UNK) which should shed light on the problem of the "odderon" and on the question whether total cross-sections are dominated by soft or hard processes.

CERN-TH.5522/89

September 1989

When I saw on the programme that I was supposed to give a review talk on elastic scattering and total cross-sections, and that this was the first talk of the Conference, I asked myself: review of what? It cannot be a review of what is going to be said in this room. It cannot be an anticipatory summary talk. I know of only one case when the summary talk took place before the conference - some of you are old enough to remember it - it was Murray Gell-Mann's summary talk at the high-energy conference in Berkeley in 1966¹. It was on this occasion that Murray said that weak interactions were in good shape, thanks to the introduction of what he called "this curious angle"².

Here, not being a genius like Murray, I am not able to review the present, so I shall try to review the past, in order to set the stage for the talks which will follow.

Proton-proton scattering is one of the oldest experiments, probably as old as particle accelerators. Its original goal was to test the nucleon-nucleon potential. In fact, my first work in physics, with L. Verlet, was to calculate phase shifts from the Lévy potential and confront them with experiment³. This kind of activity is still going on at relatively low energies up to, say, 300 to 500 MeV lab energy, with great success if one uses, for instance, the "Paris potential" of Vinh Mau and collaborators⁴, based on the dispersive approach initiated long ago by Fubini and Charap⁵ combined with some assumptions about the very short range part.

Before leaving low energies, let me indicate that a still more topical question is that of the proton-antiproton potential. The real part of the potential is supposed to be known from the G parity rule for one- and two-pion exchange, but except for some very general constraints which are not terribly useful⁶, the imaginary part is not known. The various models proposed, like the $\bar{p}$ Paris potential⁷ or the Richard-Dover potential⁸, are being tested at LEAR, where spin effects and dual parts of the scattering amplitude are being measured. One of the results of LEAR is a measurement of the proton-

antiproton scattering length which enters into sum rules, which will be discussed at this meeting by T.N. Truong.

However, beyond a certain energy, say 1 GeV lab energy, it was clear that the potential description was no longer valid. During a few years the prevailing view was a very simple-minded picture in which the nucleon was a finite size object. The scattering amplitude then took the simple form $F = s f(t)$, s being the square of the centre-of-mass energy and t the square of the momentum transfer, the total cross-section approaching a constant at very high energies.

This picture, though, was theoretically killed in 1961 by Gribov⁹, who showed that it was incompatible with the requirements of analyticity and unitarity, and also experimentally killed when the shrinking of the proton-proton diffraction peak was discovered in 1962¹⁰. All this seemed to agree with the freshly-formulated Regge pole model, in which the scattering amplitude is dominated by the Pomeron:

$$F = s^{\alpha(t)} f(t)$$

with $\alpha(t) = 1 + \alpha'(0)t + \dots$, $\alpha'(0) > 0$. Thus the width of the diffraction peak is of the order of $(\alpha'(0)\log s)^{-1}$, but the cross-section is essentially constant, and indeed, with the eyes of the fixed target machines, with maximum energy of about 30 GeV lab energy, it looked constant, around 40 millibarns. There were some problems with πp scattering, which refused to shrink, but the blame was put on more complicated animals called Regge cuts. So, up to that point, the dogma of the constancy of high-energy total cross-sections was untouched.

Yet theoreticians, or at least some of them, were prepared for the possibility of having rising cross-sections. First of all there was the Froissart bound¹¹, proved by using Mandelstam's representation and later from local field theory results¹², which says that the cross-section cannot grow faster than $(\log s)^2$. This does not exclude constant cross-sections, but the same mechanism which enters into the Froissart bound, the fact that the cross-section would like to grow like a power of the energy but is prevented from doing so by unitarity, is also in a way present in models of high-energy scattering. The $(\log s)^2$ dependence is the result of this compromise between the tendency to grow and the unitarity bound on individual partial wave amplitudes. To get a slower growth than $(\log s)^2$, you have to have an exceptional cancellation. Though nobody has been able to exploit the full content of unitarity, as pointed out in particular by Alan White, unitarity limited to its most rudimentary version in all three channels related by crossing of a two-body to two-body scattering amplitude does not lead to a qualitative improvement of the

Froissart bound, as shown by Kupsch¹³.

The case where σ_{total} goes to infinity was considered by Khuri and Kinoshita¹⁴, who proved that if $\sigma_{\text{tot}} \rightarrow \infty$, $\text{Re } F_+/\text{Im } F_+ > 0$, for s large enough, where F_+ is the even signature amplitude.

Also worth mentioning is the Eden-Kinoshita generalisation of the Pomereanchuk theorem¹⁵ which in its most general version due to Truong et al¹⁶ states that if $\sigma_{\text{total}}(pp)$ goes to infinity, the ratio $\sigma_{\text{total}}(pp)/\sigma_{\text{total}}(p\bar{p})$ approaches unity. Another theorem, due to Cornille and myself, states that in the diffraction peak the ratio of the pp and $p\bar{p}$ differential cross-sections approaches unity when energy goes to infinity¹⁷.

Finally, let me point out that as early as 1970 there was a model by Cheng and Wu predicting that the cross-sections were rising like $(\log s)^2$ ¹⁸. Yet it was a surprise when rising cross-sections were discovered at the ISR in 1972¹⁹. (Similarly it was a surprise in 1975 when the J/ψ was discovered, in spite of the prediction of Glashow, Iliopoulos and Maiani in 1969!)

Now, once you know that $\sigma_{\text{tot}}(pp)$ is rising, you predict that the real part of the scattering amplitude will become positive at high energies if you make the reasonable assumption that the odd signature amplitude is small, as suggested by fixed-target experiments at lower energies. Indeed, it was observed²⁰ that $\text{Re } F$ was positive above 30 GeV centre-of-mass energy. Later on, the same appeared to be true for $p\bar{p}$ scattering at the ISR²¹.

The combined measurements of total cross-sections and real parts at the ISR led to constrained fits of the proton-proton amplitude, satisfying the requirement of analyticity or, if you prefer, dispersion relations. One such fit is the so-called Amaldi fit²¹, but there are many others^{22,23}. What is expected is that the cross-section is 65 ± 5 millibarns at 546 GeV.

Another product of the ISR era is the belief (which turned out to be wrong) that, starting at 20 GeV centre-of-mass energy, the scattering amplitude satisfies geometrical scaling²⁴, i.e.,

$$\frac{d\sigma}{dt} = \frac{d\sigma}{dt}(0) F(t\alpha_t)$$

This predicts

- the motion of the dip from 23 to 62 GeV;
- the constancy of $\sigma_{\text{el}}/\sigma_{\text{total}}$ around 0.18;
- the fact that the dip is deepest at 30 GeV because of the vanishing of the real part.

If this had continued at higher energies, it would have fit with the asymp-

otic regime implied by the saturation of the Froissart bound²⁵ but, as we shall see, it was not the case.

We now turn to the SpS era, which was a mixture of pleasure, disappointments and surprises. The greatest pleasure was perhaps that the measured cross-section at 546 GeV centre-of-mass energy was in agreement with expectations²⁶:

$$(14p^2)\sigma_{\text{total}} = 63.3 \pm 1.5 \text{ mb.}$$

At that point, we see that the total cross-section has increased by 50% from its low-energy value.

A disappointment for some people, like P. Kroll, J. Dias de Deus and myself, was that geometrical scaling was ruled out, since $\sigma_{\text{el}}/\sigma_{\text{total}} = 0.21$ at the SpS. However, the inconsistency of this ratio agrees with many models, in particular the Chou-Yang model, in which there is a one-to-one correspondence between the total cross-section and the ratio $\sigma_{\text{el}}/\sigma_{\text{tot}}$ ²⁷ and the Bourrely-Soffer-Wu model²⁸.

The great surprise was that UA4 found a very large real part at 546 GeV²⁹:

$$\rho = 0.24 \pm 0.04,$$

while naive fits of lower energy data plus the total cross-section at 546 GeV predict ρ between 0.10 and 0.15.

If one excludes an experimental error or a large statistical fluctuation, one is left with the following "explanations".

1) The fit is too naive:

$$F = (\rho + i)s \exp(Bt/2) + \text{Coulomb term.}$$

This is advocated, for instance, by Glauber and Velasco³⁰. My observation is that there is no indication of deviations of $\log(d\sigma/dt)$ from linearity. The fit of $\log(d\sigma/dt)$ by a straight line is better than that of Glauber and Velasco in the region where $d\sigma/dt$ is measured. This hints, without proving it, that this might be the case also for very small t . Concerning the effect of taking the same slope for the real and imaginary parts, it can be estimated and it is small (replacing 0.24 by 0.23).

2) The assumption that the odd signature amplitude is small might be wrong because of the presence of an odderon, as advocated by Nicolescu and his friends³¹. There is no other evidence in favour of the odderon, but it is

true that it cannot be excluded. One hope of ruling it out (I may be criticized for the word "hope") was to write the analogue of the Goldberger-Myazawa-Oehme sum rule for pp and $\bar{p}\bar{p}$ scattering, as was done by Kroll and Igi³². Though it is very gratifying that they get the right value for the pp- $\bar{p}\bar{p}$ scattering length difference, they cannot exclude a small odderon. Thus the rule should be treated as a finite-energy sum rule, as suggested long ago by Truong, who will describe this to us later.

An unpleasant thing with the odderon is that the pp- $\bar{p}\bar{p}$ cross-section difference must change sign around the maximum ISR energy. However, once again, this cannot be excluded on rigorous grounds.

3) Finally, the large value of ρ at 546 GeV might be due to an anomalous increase of σ_{tot} above 546 GeV energy. Anomalous means more brutal than a behaviour $(A+B(\log s)^2)(1+C(\log s)^2)^{-1}$. This increase would not affect the values of ρ measured at the ISR. The domain of energies where this increase takes places is unfortunately not known. The closer it is to the energy at which ρ is actually measured, the smaller it needs to be; the further away it is, the bigger it must be. This means that mathematically it will never be ruled out, but experiments with colliders or cosmic rays could make it more and more unlikely. So experiment will eventually decide. If indeed there is an anomalous increase of the total cross-section - what I called in a rather misleading way a "new threshold" at the last Blois meeting - we have to know its origin. It cannot be a new resonance or particle which has a given angular momentum and would have its production bounded by unitarity. The best candidate today is the minijet, about which you will hear in Bernie Margolis' talk later in this Conference.

I would like to conclude by saying that none of the three possibilities I mentioned above can be excluded and that the situation should be clarified, both experimentally and theoretically. Experiments are presently being carried out at Fermilab which will be very informative, and we will hear the status report of Roy Rubinstein about this. I am not sure that this will be enough. I think that at CERN also more refined experiments on elastic scattering should be done, in particular a measurement of ρ using smaller values of t .

I also think that different experiments should be done to try to see if possibility number 2), the "odderon", is ruled out or not. The ideal thing would be to compare pp and $\bar{p}\bar{p}$ collisions in future colliders like the SSC or the LHC. Though some machine physicists say that this would be easy to do at relatively little cost in a machine primarily designed for proton-proton

collisions, it is not obvious that the majority of physicists and the deciding bodies will be easily convinced that this is needed.

What seems easier is, first of all, to do systematic fixed-target pp, $\bar{p}\bar{p}$, $\pi^+\pi^-$, $\pi^+\pi^-$, $K^+\pi^-$, $K^-\pi^+$ (regeneration) collisions, as well as hyperon-nucleon collisions at Fermilab, and see if the trend observed at lower energies continues, and if the difference between cross-sections continues to decrease like $s^{-1/2}$. Much more informative experiments of this kind could be carried out at UNK where, with 3 TeV lab energy one would reach centre-of-mass energies of 77 GeV.

In fact, the measurement of the individual total cross-sections at these energies would be as important as the measurement of the difference between AB and $\bar{A}\bar{B}$ cross-sections. One question which will be debated at this Conference will be whether the total cross-section is a "soft" or a "hard" quantity. If it is a soft quantity, we expect that the description of hadrons in terms of three or two constituent quarks or antiquarks still holds at high energy, and that the predictions of the additive quark model or total cross-sections, such as $\sigma_{pp} = (3/2)\sigma_{\pi p}$, and also the more sophisticated predictions on hyperon-nucleon cross-sections, should still be valid. Up to now these properties have only been checked in the region where cross-sections are approximately constant. If total cross-sections are due to primarily hard or semihard processes at high energies, I do not see why the naive rules of the additive quark model should apply.

Finally, I should like to make a very down-to-earth remark. You can ask "Why measure total cross-sections which nobody will be able to calculate from first principles?" Why do you measure the boiling points of liquids, though no molecular physicist will be able to calculate them because it is a too complex problem? Just because you need it. For total cross-sections, it is the same thing! Recently I met two physicists, one from UAL and one from UA2. They wanted to know the most likely value of ρ in order to get the most likely value of the total cross-section to normalize their W and Z cross-sections!

REFERENCES

1. M. Gell-Mann, in: Proceedings of the XIIIth International Conference on High-Energy Physics, Berkeley, California, eds. M. Alston Garnjost and C.E. Mark, published by the Regents of the University of California (1967), p. 3.
2. M. Gell-Mann and M. Lévy, Nuovo Cimento 16 (1960) 705.
N. Cabibbo, Phys. Rev. Lett. 10 (1963) 531.

3. A. Martin and L. Veriet, Phys. Rev. 89 (1953) 519; Nuovo Cimento 12 (1954) 483.
4. M. Lacombe et al., Phys. Rev. C21 (1980) 861.
5. J. Charap and S. Fubini, Nuovo Cimento 14 (1959) 540.
6. A. Martin, Phys. Rev. 124 (1961) 641.
7. J. Coté et al., Phys. Rev. Lett. 48 (1982) 1319;
M. Lacombe et al., Phys. Rev. C29 (1984) 1800.
8. C.B. Dover and J.M. Richard, Ann. Phys. 121 (1979) 47.
9. V.N. Gribov, in: Proceedings of the 1960 International Conference on High-Energy Physics, Rochester 1960, eds. E.C.G. Sudarshan, J.H. Tinlot and A.C. Melissos, published by the University of Rochester, p. 340.
10. G. Coconì, in: Proceedings of the 1962 International Conference on High-Energy Physics at CERN, Geneva, ed. J. Prentki, published by the CERN Scientific Information Service, p. 883.
11. M. Froissart, Phys. Rev. 123 (1961) 1053.
12. A. Martin, Nuovo Cimento 42 (1966) 930.
13. J. Kupsch, Nuovo Cimento 708 (1982) 85.
14. N.N. Khuri and T. Kinoshita, Phys. Rev. 137 (1965) 720.
15. R.J. Eden, Phys. Rev. Lett. 16 (1966) 39;
T. Kinoshita, in: Perspectives in Modern Physics, ed. R.E. Marshak, (New York, 1966) p. 211.
16. G. Grunberg and T.N. Truong, Phys. Rev. Lett. 31 (1973) 63.
17. H. Cornille and A. Martin, Phys. Lett. 40B (1972) 671.
18. H. Cheng and T.T. Wu, Phys. Rev. Lett. 24 (1970) 1456.
19. See, for instance: "A Discussion on Proton-Proton Scattering at Very High Energies", organized by J.M. Cassels and A.M. Wetherell, Proc. Roy. Soc. London A335 (1973) 409-507.
20. U. Amaidi et al., Phys. Lett. 66B (1977) 390.
21. A. Amos et al., Nucl. Phys. B262 (1985) 689.
22. C. Bourrely and J. Fischer, Nucl. Phys. B61 (1973) 513.
23. M.M. Block and R. Cahn, Rev. Mod. Phys. 57 (1985) 563.
24. J. Dias de Deus and P. Kroll, Acta Polonica B9 (1978) 157; Phys. Lett. B171 (1986) 142.
25. G. Auberson, T. Kinoshita and A. Martin, Phys. Rev. D3 (1971) 3185.
26. UA4 Collaboration, M. Bozzo et al., Phys. Lett. 147B (1984) 392.
27. T.T. Chan and C.N. Yang, Phys. Rev. D19 (1979) 3268.

28. C. Bourrely, J. Soffer and T.T. Wu, Phys. Rev. Lett. 54 (1985) 757.
29. UA4 Collaboration, D. Bernard et al., Phys. Lett. 8186 (1987) 227.
30. R.J. Glauber and J. Velasco, in: Proceedings of the Second International Conference on Elastic and Diffractive Scattering, ed. K. Gouleanos, The Rockefeller University, New York (1987), Editions Frontières, p. 219.
31. B. Nicolescu, in Ref. 30, p. 133;
P. Gauron, communication to this Conference.
32. P. Kroll and K. Igi, Phys. Lett. B218 (1989) 95.