THEORY OF NUCLEON-ANTINUCLEON ANNIHILATION

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The nucleon-antinucleon system has fascinated theoreticians for many years. It is the analogue of electron-positron annihilation in which all quantum numbers disappear to produce light. Here pions are the final debris. Sometimes the system does not annihilate, in which case the interrelation with baryon-baryon scattering becomes important. I will come to that subject later. First I will concentrate on the annihilation final states.

LOW ENERGY ANNIHILATION

Historically, it was Fermi who first analyzed the annihilation problem. Using the statistical model he predicted a very similar pattern to the two \( \gamma e^+e^- \) annihilation \(^1\). As it turned out, these predictions did not materialize and in spite of many modifications the dynamics of the annihilation system proved to be much more complex than what simple models based on constant matrix elements might suggest.

Hence, though the model is a good guide for order of magnitude properties of the system, it is unable to predict properly the detailed structure of the final states.

Because the matrix elements are rapidly varying and resonances populate most channels unless these are exotic, the final state interaction came to be popular \(^2\). In this model the amplitudes are allowed to resonate channel by channel and they are then added. The famous example of \( \bar{p}n \) going to 3 pions showed the inadequacy of the model whose credibility was already badly tarnished by the failure of the interference model in scattering amplitudes. Both are two aspects of the same problem : how to combine dynamically singularities of overlapping channels or their analytic continuations. We will come back to the problem.

Another model that was popular for some time is the quark rearrangement model \(^3\). I refer to the last symposium \(^4\) where its failure is well documented.

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It will be my contention that duality has been the renovating idea in the field and indeed qualitatively at least it has brought a lot of understanding to this field.

After talking to some of you I found it useful to remind you of the remarkable success of the qualitative ideas of duality in low energy hadron and non-hadron phenomena.

I will make a list of what I would call successes of phenomenological duality but I will make no effort to substantiate the claims.

1) The classical studies of averaging in meson baryon scattering.
2) The qualitative successes of \( B^* \) phenomenology.
3) Averaging and "vectorial duality" in deep inelastic scattering.
4) Zeros and suppressions of definite helicity amplitudes.
5) Mass relations between particles and values of intercepts.
6) Odorico patterns.
7) Harari-Freund's classification of cross-sections relating resonances to energy dependence and flatness to Pomeron exchange.
8) The lines of zeros in \( \pi\pi \) scattering amplitudes that follow the duality pattern.
9) The duality effects in the nucleon-antinucleon system that we will discuss.
10) The peaks that appear and disappear in mass distributions, that indeed is a contributing factor to the so-called Goldhaber effect.
11) The behaviour of slopes as a function of missing mass and many other qualitative effects that as the ratio of real to imaginary parts and the reality of some amplitudes are striking indeed. I refer to the standard Conferences and Summer Schools for further information.

We discuss now in detail the relevant points concerning annihilation. As it was first pointed out by Lovelace, one can construct a \( B^* \) function that is good for \( \pi\pi \) scattering:

\[
A^{\kappa}_{1\pi} = \beta \left[ \frac{\Gamma((1-\kappa(s))\Gamma((1-\kappa(\omega))\Gamma((1-\kappa(\beta)))}{\Gamma((1-\kappa(\omega))\alpha(\omega))\Gamma((1-\kappa(\beta))\alpha(\beta))} \right] \\
A^{\pi}_{1\pi} = \beta \left[ \frac{\Gamma((1-\kappa(\omega))\Gamma((1-\kappa(\beta)))}{\Gamma((1-\kappa(\omega))\alpha(\omega))\Gamma((1-\kappa(\beta))\alpha(\beta))} \right] \\
A^{\mu}_{1\pi} = \beta \left[ \frac{\Gamma((1-\kappa(s))\Gamma((1-\kappa(\beta)))}{\Gamma((1-\kappa(s))\alpha(s))\Gamma((1-\kappa(\beta))\alpha(\beta))} \right]
\]
It is amusing to remark that this amplitude has no diseases, from the theoretical point of view. It lacks diffraction contributions, but it is well-known that these inelastic channels themselves do not get Pomeron contributions.

The final state as seen in the accompanying figure is one in which overlapping resonances occur. Their contribution is multiplicative in duality, which in turn naturally forces lines of zeros when both trajectories hit half integer values and leads to the peculiar pattern that is indeed seen experimentally \(^{(18)}\). Moreover, the two-body mass distributions have a peculiar shape, the exotic channel \( \pi \pi \) doubly charged shows a resonance like behaviour for small values of missing mass. In the final state interaction model \(^{(18)}\) one is led inevitably to conclude that one has discovered exotic states, while the explanation given by formula \(^{(1)}\) is completely different: the amplitude is purely real and the reason for the rapid growth towards threshold of the mass distribution is the growth of the function with \( s \) and \( t \), the Mandelstam variables of the crossed channels. Since \( s + t + u = \) constant clearly small \( u \) values are favoured. As we will see below this dynamical enhancement of threshold values for exotic channels is a very general duality mechanism that has consequences elsewhere and it is probably responsible for the so-called Goldhaber effect \(^{(19)}\).

Unfortunately the original Lovelace explanation is quantitatively incorrect and most important theoretically inconsistent.

A reasonable fit to the Dalitz plot requires expressions of the form \(^{(20)}\):

\[
    g(s,t) = \sum c_{nm} \frac{\Gamma(n-\alpha(s)) \Gamma(n-\alpha(t))}{\Gamma(n+m-\alpha(s) + \alpha(t))}
\]

As it turns out these forms and the relative size of the coefficients \( c_{nm} \) are precisely what must come out from the mass extrapolation since the decaying system is not really a leading term of the trajectories \(^{(21)}\). This is easily understood in terms of an expansion of the \( B_5 \) in an infinite (sometimes finite) series of \( B_4 \) functions.
Several authors have tried to push these semi-quantitative results to annihilation into four particles or annihilation in flight. One faces two kinds of difficulties: either it becomes theoretically too complicated because of the many amplitudes involved and the diseases of the dual amplitudes or the phase space and symmetrizations wash out all the information to the point that almost everything you do looks alike (22), (23). The moral is the following: in the cases where the theories can be tested duality comes neatly ahead, unfortunately without detailed numerical agreement. Fitting the data, when looking for more complicated final states, teaches us very little new. Furthermore, it is worse than that. There are many papers in which statements about resonance production are made like so much $p$ and so much $f$. This is clearly a model dependent statement whose significance is very dubious. The final state interaction model is certainly unreliable since the few cases that can be analyzed do not support the addition of overlapping channels. Since these estimates have been used elsewhere one must warn about their lack of reliability.

In some cases the final state interaction model may agree, accidentally, with the duality expansion if a few poles are dominant, but it is hardly a justification to perform these detailed fits whose consequences are suspect.

The presence of the zeros in the duality pattern as seen in $\pi\pi$ scattering in $\pi$ nucleon scattering and in the work of Odorico are powerful pieces of evidence that seem to give strong confirmation to phenomenological duality.

The question is then: what can be done with the multi-particle data. I will discuss a few examples.

Qualitatively one expects zeros to exist in annihilation in flight. They do (24). It can be shown that systems that have neutral pions tend to blur the effects as the one we discussed concerning low mass exotic enhancements. Let us see how it goes.

We now study the predictions on $\bar{p}p$ annihilation into three pions. These predictions should be reliable since they follow from isospin invariance. For the charged mode the data can easily be fitted since the Dalitz plot is uniform, while the neutral case has not been measured. We will assume that these predictions are correct.
By looking at the Figs. 1b and 1c it is seen that the peaks observed in the previous case are less pronounced or almost disappearing. In the case where all the pions are neutral the distribution is almost solely phase space in spite of the strong energy dependence of each Veneziano term. The reason behind this is simple. Since in \( \bar{p}p \rightarrow \pi^{+}\pi^{-}\pi^{0} \) and \( 3\pi^{0} \) there are several terms contributing it can be seen that they have maxima at different values of the variable under study. This is because different channels contribute different terms. The reason for these terms to appear is that the different permutations are necessary to ensure crossing symmetry. However, since one wants no exotics as well, permutations that would explicitly introduce exotic variables are not allowed. This is why in the previous section, there is just one term, since otherwise one would obtain \( I=2 \) resonances. In the case of \( \bar{p}p \rightarrow \pi^{-}\pi^{+}\pi^{0} \) the presence of a neutral pion allows for all permutations.

Our purpose is to infer qualitative predictions on many pions processes. Consider four-pion final states. In \( \bar{p}n \) at rest into \( \pi^{+}\pi^{-}\pi^{0}\pi^{0} \) we have only one neutral external leg, including the decaying state, and hence the number of permutations is rather limited. Dynamically, each term is replaced by a \( B_{5} \) five-point function. However, their main qualitative features are the same: these lack dependence on exotic variables and they grow as a function of the variables dual to an exotic one. However, because of the integration that is not present in the \( B_{4} \) the shape and position of the peaks is expected to be somewhat changed. Despite this, it is reasonable to expect that exotic peaks and resonance-like ones will persist. In the case of \( \bar{p}p \) decay into \( \pi^{+}\pi^{+}\pi^{-}\pi^{-} \) one expects very similar results to the previous case. There are some dynamical spin differences but these are expected to be small as discussed further on. Adding two neutral pions to \( \bar{p}p \) or three to \( \bar{p}n \) should make the distributions of the annihilation into four-pions completely phase space-like.

The striking effects start to appear with five final state pions. In \( \bar{p}n \) annihilation there is a fully charged mode. This one is a pure isospin state and allows for few permutations. One expects strong peaks. The \( \bar{p}p \) state must have two neutral legs: the decaying one and the final neutral pion. Hence there should be suppression of the peaks relative to the \( \bar{p}n \) case. Adding neutral pions should always neutralize the effects and make distributions phase space-like.
And so on... By adding neutral pions or charged ones whenever possible the \( \pi n \) can have always configurations with one neutral leg at most while \( \pi p \) is sometimes forced to have two at least.

As seen in the accompanying figure, this duality prediction is strikingly satisfied \(^{25}\). The generality of these properties has been tested elsewhere with success \(^{26}\).

By introducing further assumptions one can enlarge the predictive power of the scheme. It is well known that up to the highest known energies the exchange of the pion trajectory is important in channels like \( pp \to pp, pp \to p\Delta, \pi p \to \text{vector meson } p \), and so on.

Because of duality one expects that an infinite sum of s-channel amplitude resonances should be enough to describe the full system. The s-channel nucleon-antinucleon system can couple to all known trajectories and by crossing one expects the pion trajectory to be rather important \(^{27}\). If this were the case several interesting things would happen:

a) since \( J = \sigma \)'s, at threshold one expects a spin three-state,

b) recurrences of high spin may be detectable,

c) the negative \( G \) parity final states should be preponderant,

d) the energy dependence of even and odd \( G \) parity states should be different,

e) the differences should disappear once kaons are present.

There is some scanty evidence in favour of these predictions \(^{28}\). I will come back to this point later.

**THEORETICAL DUALITY**

I think that all of you are aware that dual resonance models have not been able to live up to expectations. Though much progress has been achieved \(^{29}\), it is far from clear if we are near a reasonable theory. However, some of the demanding questions like the self-consistency of the duality models as embodied for example in the duality diagrams are still with us. The existence of Rosner states is a good example. Since these states have been discussed in the previous meeting I can only insist on the importance of the question. To conclude the discussion of the low energy annihilation I would like to summarize my views.
Much like in nuclear physics I feel that you must concentrate in limited goal experiments. General surveys of multibody final states without a detailed theory behind the search is not very rewarding. It is not very probable that one will make fast progress this way. The staggering number of variables that increases rapidly with the number of final state particles makes the difficulties insurmountable. Hence one must ask the appropriate consequential questions and I believe that these perhaps difficult but very relevant experiments should be considered. In particular:

a) are there peculiar Rosner or other states?
b) are there low lying high spin objects?
c) are there dynamical differences between even and odd final states?
d) are there $G$ parity unbalances?
e) what is the limiting value of the low energy cross-section? This particular point has astrophysical consequences as well.
f) are there interesting properties of the ratio of neutral to charged energy? This point could give at rest some interesting handle to related problems.

**HIGH ENERGY ANNIHILATION**

I will not discuss the high energy annihilation problem much since the subject will be covered by Miettinen. Nevertheless, I will make some comments.

We all believe that asymptotically

$$\sigma_{pp}(s) \rightarrow \sigma_{pp}(\infty)$$

It has been conjectured that

$$\sigma_{pp}(s) = \sigma_{pp}(\infty) - \sigma_{pp}(\infty)_{\text{annihilation}}$$

holds. Since the difference has the right quantum numbers and energy dependence of $\omega$ exchange it has been assumed that this is the case.

Using duality one can analyze the different contributions to annihilation. Unfortunately, annihilation is really a non-leading phenomenon and because of that a model dependent analysis is necessary.
Several conclusions are, however, easy to reach:

1) unless extraordinary things happen the two non-annihilation cross-sections for baryon baryon and antibaryon baryon cannot be equal,
2) in some classes of models the leading contributions to the annihilation look Pomeron-like but with an energy dependence. These graphs lead, by the counting arguments of H. Lee and Veneziano, to an appealing relation between intercepts \(^3\). Other predictions come out but they are not very much of a constraint.

It seems to me that the problem is a hard one indeed.

**CONCLUSIONS**

1) It seems that there is a possibility that important properties of the hadron spectrum can be determined from low energy annihilation. In particular it might be possible to look for spin three- or higher pion recurrences.

2) The structure of zeros in multipion systems may be a place for looking for general properties of dual models.

3) Neutral energy and the G parity of the final states is another interesting quantity. At rest it might help, besides its own interest, to compare with the hadronic annihilation of e\(^+\)e\(^-\) at the same energy.

4) Fits to complicated modes of annihilation with models that are wrong in well-known channels is not very rewarding even if miraculous cancellations make them more reliable in complicated channels. In particular statements about how much of that resonance and how much of the other are produced are meaningless in the presence of overlapping channels. These mass distributions reflect the overall dynamics and are very sensitive to the theory assumed.

5) The sacred cows

\[
\sigma_{p\bar{p}} = \sigma_{pp} + \sigma_{\text{annihilation}}
\]

and

\[
\sigma_{\text{annihilation}} \propto s^{-1/2} \text{ (as exchange)}
\]

are no longer with us. High energy annihilation is a complicated problem. You might find some of my remarks disappointing. I think
that the annihilation is the first really many-body problem of high
energy physics from the start. The theories of these processes are
still at the beginning of a long road and one is still looking for
the qualitative guiding aspects of the data.

Let me finish by reminding you of what J.L. Borges said at
the funeral of one of his friends: he lived at difficult times, like
all men.

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Phys.Rev. 176, 1904 (1968);
M. Bishari, H.R. Rubinstein, A. Schwimmer and G. Veneziano,


13) These were first pointed out by C. Lovelace, Phys. Letters 28B, 265 (1968).


16) D. Horn, Lectures at the Schladmig Summer School, M. Kugler Schladmig Lecture, 1971.
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17) C. Lovelace, Ref. 13), also
Y. Shapiro and Y. Yellin, unpublished.


22) For the theory of these functions, see H.R. Rubinstein, Springer Tracts of Modern Physics 57, 191 (1971).


25) The references for the data can be found in Ref. 14).

26) New data seem to support the model quite nicely. Grammatikakis, private communication.


28) The statements to the contrary in the previous Symposium are not correct.
29) See, for example, the series of Physics Reports by E. Amati, V. Alessandri, M. Le Bellac and G. Veneziano amongst the least technical ones.

30) Clearly, there is no relation to $e^+e^-$. See, H. Rubinstein, Rencontre de Moriond, 1974.

At rest, however, even if different, the comparison may prove useful.

31) G. Veneziano, private communication;

Y. Eylon and H. Harari, Nuclear Phys., to be published.

32) H. Lee, Phys.Rev.Letters 30, 719 (1973);


FIGURE CAPTIONS

Fig. 1

Top: $\pi^-$ spectrum in $\bar{p}n \to \pi^-$;
Centre: $\pi^-$ same reaction;
Bottom: a) $\pi^{00}$ spectrum, in $\bar{p}p \to \pi^{000}$,
b) $\pi^{+}$ spectrum, in $\bar{p}p \ I = 1 \to \pi^{+0}$,
c) $\pi^{+0}$ spectrum, idem. See Ref. 20).

Fig. 2

a) $\pi^+$ mass in $\bar{p}p \to \pi^{+++}$,
b) $\pi^-$ mass in $\bar{p}p \to \pi^{+++}$,
c) $\pi^-$ mass in $\bar{p}n \to \pi^{++0}$,
d) $\pi^-$ mass in $\bar{p}n \to \pi^{++++}$,
e) $\pi^-$ mass in $\bar{p}p \to \pi^{+++0}$,
f) $\pi^+$ mass in $\bar{p}p \to \pi^{++0}$,
g) $\pi^+$ and $\pi^-$ mass in $\bar{p}p \to \pi^{++++}$,
h) $\pi^-$ mass in $\bar{p}p \to \pi^{++++}$,
i) $\pi^-$ mass in $\bar{p}p \to \pi^{++++0}$,
j) $\pi^+$ and $\pi^-$ mass in $\bar{p}p \to \pi^{++++0}$,
k) $\pi^+$ and $\pi^-$ mass in $\bar{p}p \to \pi^{+++++++}$,
l) $\pi^+$ mass in $\bar{p}p \to \pi^{+++}$;
m) $\pi^+$ mass (full line) and $\pi^+ + \pi^-$ mass (dotted line) in $\bar{p}p \to \pi^{+++++++}$,
n) $\pi^+$ and $\pi^-$ mass in $f \to \pi^{++}$,
o) $\pi^-$ mass in $f \to \pi^{++}$, see Ref. 14);

$\pi^{n+m}$ means $\pi^+ \cdots \pi^+ \pi^- n, m$ times.
Fig. 1
- Pišút:
  Are there any data about the charge transfer in e⁺e⁻ annihilation?

- Rubinstein:
  As far as I know there is no charge transfer. The distribution is absolutely flat except for forward and backward peaks. In ðr there are cases when π⁺ remembers the direction of the proton and π⁻ the direction of ðr.

- Šimák:
  But there is no charge asymmetry; is that what you mean?

- Rubinstein:
  No charge asymmetry, no, but the errors are large.

- Smith:
  At SLAC they do not look at the forward direction, their detectors are at 45°.

- Rubinstein:
  That does not matter, you can put the limits for the Legendre polynomial coefficients anyway.

- Schultz:
  For example, in ðr, leading particle effects do not show up in low multiplicity events.

- Rubinstein:
  That is a different situation.

- Schultz:
  What about the scaling?

- Rubinstein:
  The bulk of the data does not scale.

- Schultz:
  You are looking into the central region.

- Rubinstein:
  This does not matter, either you have the scaling and the cross-section behaves as 1/s or you do not have it. It is a different question whether a part of the data, say those at large X, scale; the data there are slim but it seems that for large X there is scaling.

- Kalogeropoulos:
  For a long time we studied the ðn → π⁺π⁻π⁻ and ðp → 3π⁰, and I was impressed by the quality of the fits based on duality, which worked much better than the fits based on the final-state interaction. In the Dalitz plot for ðn → π⁺π⁻π⁻ the important thing is not only the hole in the middle but also the enhancement above that, and this is just the result of a delicate interference which is not trivial in final-states models; the π⁻π⁻ enhancement at low effective masses is, of course, the result of a projection of this enhancement. In 3π⁰ there is again
the same enhancement and again the dual theory works. Still I do not believe that all enhancements at threshold for $\pi^-\pi^-$, $\pi^+\pi^-$, etc., are a result of similar interferences of $f^1$ with itself.

- **Rubinstein:**

  Sorry, I did not say it is a general feature. I just said that in this particular case the $\pi^-\pi^-$ enhancement is not the result of the Bose–Einstein statistics, but a consequence of the dual theory.

- **Šimák:**

  There are some people who believe that you know the six-point function and that combining it with the Mueller–Regge approach one could get everything about the inclusive reactions. Do you think that this will work?

- **Rubinstein:**

  No, dual theory is not complete -- at least at present -- and it is only a theory of soft processes. For instance, the Veneziano formula gives wrong predictions for large $p_T$ behaviour of elastic amplitudes. I would not use the dual theory as it stands now for quantitative phenomenology.

- **Liljestrål:**

  In the $4\pi$ data which were presented by Žáček I got the impression that there were no Goldhaber effects. Is that right?

- **Rubinstein:**

  If we had a perfect theory we could predict everything. The trouble is that we do not have it. In fact I think it is not necessarily a contradiction.

- **Pišút:**

  This morning Žáček presented data on $\bar{p}p \rightarrow 2\pi^+2\pi^-$ at 5.7 GeV/c with a deep dip in $\pi^+\pi^-$ mass distribution at about 1 GeV. Has anybody looked into the generalization of Odorico zeroes for the five-point dual amplitude?

- **Rubinstein:**

  I have a graduate student looking at that. We found a family of zeroes but I cannot guarantee we found them all. It is an interesting problem.

- **Schlesinger:**

  Rosner's suggestions to look for exotic states are unfortunately very hard to apply experimentally.

- **Rubinstein:**

  It is exactly my point. One $\Omega^-$ was worth a lot of other experiments. It is much more important for the progress in physics to make an effort on some difficult point. Nature is tricky with us. And SU(3) is not even a dynamical concept. As Lipkin says, we need neither exclusive nor inclusive but conclusive experiments.

- **Kalogeropoulos:**

  If I understand correctly, the dual explanation of $\bar{p}n \rightarrow \pi^+\pi^-\pi^-$ says that $\pi^-\pi^-$ are pushed together by the two resonance pairs $\pi_1^+\pi^-$ and $\pi_2^+\pi^-$. If this is the case, at higher energies the Goldhaber effect should disappear.

- **Rubinstein:**

  One has to be careful; at higher energies the analysis of $\bar{p}n \rightarrow \pi^+\pi^-\pi^-$ at rest cannot be applied.
- Lillestol:
  I think that you see the same effect also for Kπ system.

- Smith:
  You mentioned a possibility of making the Lovelace type of analysis in flight.

- Rubinstein:
  I tried to do some myself, but finally I got embarrassed. The business is complicated. I do not advise people to make quantitative dual fits; the work by Chan and Törnquist of πN → πKΛ was very nice, but it probably cannot be pushed much further right now.

- Montanet:
  Do you assume that the daughters should appear?

- Rubinstein:
  The daughter situation is a very model-dependent aspect of the theory. I do believe that there are some daughters, otherwise nature would stop; on the other hand, the structure of daughters, which particles have daughters and how many, is not clear; experimentally they seem to be very broad.

- Lundby:
  What is the importance of cuts in the game?

- Rubinstein:
  The optics is in fact cuts. I think that Miettinen should answer the question.

- Miettinen:
  The importance of the cuts can be seen in the comparison of p̅p and pp differential cross-sections, in particular from the cross-over. They are important.

- Šimák:
  Can you say something, on the basis of the dual model, about the relative importance of I=0 and I=1 states in annihilation?

- Rubinstein:
  No.

- Kalogeropoulos:
  What do you consider to be a crucial experiment?

- Rubinstein:
  Well, for instance establishing the existence of low-lying states in NN; it would be nice to know whether the cross-section rises near threshold faster than partial waves.