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I am very sorry (we all are, I know, and I'm sure I speak for us all) that Dr. Gribov was unable to be here to deliver his talk. I am sure that our Soviet colleagues will convey to him our fraternal greetings, our deep disappointment, and our firm hope that we will see him at our next meeting in two years. I am glad to hear that Dr. Gribov is sending a manuscript, which will be in the proceedings of the meeting.

I will first make some general comments, and then turn to a few specific topics which have been actively discussed during the parallel sessions.

Unfortunately, the theory of strong interactions is excessively fragmented, and it is therefore very hard to see it whole.

It has become the custom at these meetings to make a virtue of confusion by showing a picture, so I have one too, intended to illustrate the connectivity properties of the different approaches which physicists have developed. The ideal figure would be different from Fig. 1; the basic theory would be in the center - and all other fields on spokes!

Figure 1 shows field theory in a central position, since (a) there is one such theory that works remarkably and puts together about all microscopic phenomena, i.e. quantum electrodynamics, and (b) field theory is the only way we have so far of systematically, order by order, constructing an S-matrix that is covariant, unitary, and appropriately analytic in all its variables. All the boxes in the picture are derived or abstracted from field theory, with the exception of the non-relativistic quark model. I have put gravitational, weak and electromagnetic interactions off-scale far away only because they are not immediately relevant to this session.

All the other boxes are relevant, or should be relevant to this session, but the two on the right have been discussed by other speakers, so I will leave them with only one comment. If nature simply used its old methods over again, we would expect to unify weak + electromagnetic phenomena with strong interactions by introducing hadronic fields, from which we could form currents, etc., and proceed as inthe previous lectures. The success of the non-relativistic quark model suggested that real quarks might correspond to these fields. However, real quarks themselves have not been seen, so that, if they exist, either

(a) they are heavy, and then why does the simple non-relativistic model work? or

(b) they are light, and then what mysterious mechanism holds them in, with a potential barrier so high that the excited states seem to be on straight trajectories? or real quarks do not exist, and the quark quantum numbers must be accounted for by

(c) something other -- which must be very different from our conventional ideas. So probably natures is proceeding quite differently than a simple-moded extrapolation from q.e.d. would indicate.

We now circle (not necessarily in historic order) the lower left closed loop. Field theory suggests Mandelstam like analyticity properties for S-matrix elements via Feynman graphs; from these one may deduce dispersion relations and Regge, or j plane analyticity (of course, axiomatic field theory was very helpful for the former, and the Schroedinger equation for both the former and the latter; in fact, in the old N over D days I once heard a theorist remark that the Schroedinger equation was a devilishly clever way of calculating the left-hand cut of a partial wave amplitude).

By S-matrix theory, I mean the attempt to use only Lorentz covariance, analyticity, and unitarity to calculate S matrix-elements. This can frequently be a matter of convenience, involving no commitment as to the underlying degrees of freedom (such as quantum fields). There is a school, led by Chew, which assumes that there are no underlying degrees of freedom, and that all S matrix-elements are uniquely determined by the above assumptions, together with Regge asymptotic behavior. That is, they would break the arrow connecting the field theory and analyticity boxes. Once this arrow is broken, the link to our world (q. e. d.) is broken. My position is neutral on this with respect to the strong interactions; however, the electromagnetic field requires a local current with which to interact, so that a pure S matrix theory will have trouble accommodating electromagnetic, as well as, probably, weak and gravitational interactions.

It should definitely be understood that the use of S matrix lagnuage, the assumption of Regge asymptotic behavior, and the study of j plane properties can be useful whether or not there are underlying degrees of freedom. There is in our particle physics community a widespread hostility to j plane language -- based on the fact that the description of some phenomena appears complicated in that language. In fact, the variable j/i is conjugate to log z (where z is proportional to the crossed channel energy) in almost the way that p is conjugate to x. One mustn't fight the x representation or the p representation; some ideas are more simply expressed in p language, some in x. More important, some requirements are mose simply seen in p space (for example, threshold dependence), some in x space (for example, cluster properties). Similarly, it pays to look in both j and log z space. For example, the very simple formula for the invariant elastic scattering amplitude at high S

$$M \to is\, f(t),$$
$$s \to \infty,\ t \text{ fixed}$$

so dear to the hearts of many of us, has, in the j plane, a fixed pole at $j=1$, and contradicts t channel unitarity; thus it is frowned upon by those who use j plane language. Its problems in (s, t) language are much harder to see, but they will be there in a complete theory possessing t and s analyticity and unitarity, and their cure will require complications in any language. This is the first of many unpleasant results of Regge theory, which Murray Gell-Mann has characterized as proving that any simple theory that agrees with experiment must be wrong. However, I believe it is not the j plane analysis that is at fault but the formula. I will, incidentally, return to the problem of diffraction scattering later.

We finally close the loop with models. Appropriately, the two parallel sessions labeled dynamics of strong interactions were subheaded (I) dual models, which originated four years ago as pure S-matrix theories, and (II) Multiperipheral models, which originated eleven years ago as a summation of a certain clan Feynman diagrams in field theory. By now, of course, it is all mixed up, including the meaning of the word multiperipheral.

I shall report a small fraction of the work discussed in those sessions, with the choice hopefully made so as to give you a feeling for where the action is now, and where it may be going; I will also add other material, not specifically submitted to this conference, which I think will be helpful in achieving perspective. This is in accord with the instructions contained in the letter of invitation to Dr. Gribov. I apologize for the ommision of important new material.

I. I discuss first the present status of dual models.

The simplest dual model for a four-point function describing scattering of spin zero particles is

$$B_4(s,t) = g^2 \;\frac{\Gamma[-\alpha(s)] \quad [-\alpha(t)]}{\Gamma[-\alpha(s)-\alpha(t)]}\,.$$

with $\alpha(s) = \alpha_0 + \alpha' s$. The properties of this function are:

1.

$$\text{For } t < 0, \ B_4 = \Sigma_n \frac{\underline{P}_n(t)}{M_n^2 - S}$$

with $\underline{P}_n$ a polynomial of order n, hence containing angular momenta $n, n-1, \cdots 0$; similarly for $s < 0$, $B_4 = \Sigma_n \dfrac{\underline{P}_n(t)}{M_n^2 - t}$.

2. Regge behavior: for fixed t, and $s \to \alpha$,

$$B_4 \to g^2 \, \Gamma[-\alpha(t)] \ [-\alpha(s)^{\alpha(t)}$$

and similarly for fixed s, $t \to \infty$.

3. For fixed $z = \cos\theta$, $s \to \infty$ (i.e. $p_\perp \to \infty$).

$$B_4 \sim \exp[-\alpha' s \, f(z)] \ ,$$

where $f(z) = \dfrac{1-z}{2} \log \dfrac{2}{1-z} + \dfrac{1+z}{2} \log \dfrac{2}{1+z}$,

thus producing an exponential fall-off at large $p_\perp$.

The formula was rapidly generalized to n point functions, which have the same spectrum, and factorize at poles. These functions may be and have been used in three ways:

1. with insertion of specific trajectories, to fit multi-particle production processes, and to check out ideas on such things as early scaling. Formulas of this type leave out a lot (I will come to this shortly) but seem to describe remarkably well some phenomena.

2. As a theoretical laboratory, to investigate, for example, the high energy Regge structure of multiparticle amplitudes.

3. Finally, the most ambitious, as a first approximation to an

exact theory. This first approximation is a strong one compared to the usual field theoretic perturbation theory, since it already contains an infinite spectrum of states. It turns out that a Feynman like interaction scheme exists, so that given a "correct" first approximation and weak coupling (which is really the basis of the whole scheme the weak coupling, that is) one can systematically go on order by order.

Let me list for you know the standard problems of the 1st order dual model, viewed as a first approximation to the real world. There are:

1. Ghosts: Since one has n particle amplitudes, one can use factorization to study the degeneracy and coupling constants of each level. In general, some will have negative g^2, i.e. ghosts. Brower, and independently Goddard and Thorn, have shown that if the leading trajectory passes through $\alpha = 1$ at $t = 0$, the ghosts decouple. However, one would then have a tachyon at $M^2 = -1$, and a zero mass vector.

2. The tachyon: Neveu and Schwarz have introduced a new model in which one can eliminate the tachyon. However, (3) + (4), a correct spectrum, including spin 1/2, does not come close to emerging.

5. Absence of exotics of course implies absence of a vacuum pole--but this will be cured by dual loops, it is assumed, once the "correct" 1st order model is found.

6. The most obvious problem is, of cource, unitarity, since the

resonance poles are on the real axis, corresponding to the straight, real trajectories. A sum over dual loops (an infinite number, to be sure) is the suggested solution. This is analogous to the situation in ordinary field theory, where an unstable particle is introduced with a real mass, and an infinite order damping (a la Wigner-Weisshopf) calculation is required to give it its imaginary part.

7. Finally, the problem of introducing local currents has successfully resisted all attacks.

I turn now to the higher order corrections, i.e., the dual loops. Again, these may be viewed as

1. guides to phenomenology
2. theoretical laboratories
3. the "true" theory.

Use (3) of course requires a correct 1st order. However, (1) and (2) do not--that is, one can hope to distinguish correct general features from specific calculations, even though the underlying theory is wrong (remember perturbation theory teaching us analyticity).

What are the new features introduced by the loops?

1. Iteration of poles (in j or in s or t) of course produce cuts (threshold in s,t + Regge in j). The Regge cuts have slope $\alpha'/2$, as expected.

2. A new j plane singularity, with vacuum quantum numbers, emerges,

but <u>not</u> as a Regge-Regge cut. One would like to interpret this singularity as the Pomeron; however, it has unacceptable t analyticity except in a space of d = 26 dimensions (25 space and 1 time) where it is a new linear trajectory with $\alpha' = 1/2$ and $\alpha_0 = 2$ (in the Neveu-Schwarz model, d goes down to 10 dimensions). Again, presumably in the correct theory it will go down to four.

In spite of these problems, Lovelace, in a contributed paper, has described how one uses these results as a model to study various Pomeron properties, such as t or s channel helicity conservation, the sing of the p pomeron-pomeron cut (to which subject I will return).

Another interesting application is suggested by Ellis and Freund who observe that in each order, the leading elastic scattering at fixed z is given by Veneziano's formula

$$\exp - \alpha'_n f(z) s,$$

where α'_n is the slope of the highest singularity (they also give somewhat more general arguments for this form). Since $\alpha'_n = \alpha'/n$, we have a succession of decreasing $p_\perp$ dependencies, whose true asymptotic limit is a power, the power dependency on the details of coupling as $n \to \infty$.

Figure 3 is a list of difficulties of the dual loops, viewed as a "true" theory.

In this figure, $26 \to 10$ for the Neveu-Schwarz model. Presumably, the contradiction between (1) and (2) will be resolved by the correction first order theory, for which d will also equal 4. Thus, the dimensionality of space will be boot-strapped!

If this program works, we will have a complete S matrix theory, with, nowever, one free parameter (the coupling constant). Evidently, one is still quite far from a correct beginning, but the number of sensible properties is impressive.

A very interesting new development, based on independent Susskind, work by Mansouri and Nambu, and Goldstone, was reported in the parallel session, and promises a great simplication of what is at present an exceedingly complex formalism, and thus, may make the introduction of the correct initial theory much simpler. This is a Lagrangian for the uncoupled dual system, based on the excitations of a relativistic string.

II. Multiperipheral models: The so-called multiperipheral (I say "so-called" because the work multiperipheral has taken on many different meanings) session was devoted almost exclusively to high-energy processes. Here the most interesting new techniques are based on the generalized optical theorem of Mueller. This has let to impressive empirical success, but has also raised new problems.

Slide 4 shows the conventional optical theorem for elastic scattering: unitarity requires that the total cross-section σ_{ab} be proportional to the imagninary part of the forward scattering ampltiude, Im fab, with known kinematic factors. The imaginary part is in turn given by the discontinuity across the s cut. The usefulness of this theorem is well known. Nevertheless, let me point out one major theoretical beauty: it replaces the calculation of an infinite, non-linear functional of all production amplitude (σ_{ab}) with linear functional of the elastic amplitude (disc fab). Thus, for example, Regge behavior $s^{\alpha(t)}$ in an elastic amplitude implies $s^{\alpha(0)-1}$ in the total cross-section.

The Mueller theorem has the same advantage: if we have a model for $3 \to 3$ amplitudes, we immediately get, by a linear operation, a model for an inclusive cross-section. (Note the difference here from the two body case, that the Mueller relation does not connect observables, since our experimental colleagues have not yet been clever enough to devise methods of measureing $3 \to 3$ amplitudes.)

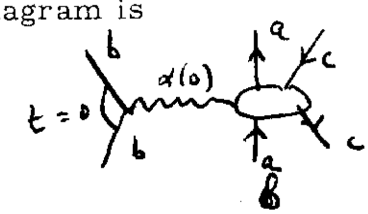
Now Regge theory does make predictions for many particle amplitudes is some regions of phase space. These were systematically studied many years ago by Toller, and generalized more recently by Misheloff and others. Thus Toller plus Mueller puts Regge analysis puts inclusive reactions on almost the same footing as that of two body cross-sections, with with two very important caveats.

(i) As I said before $M_{3\to 3}$ is not measureable, so there is no measurable analogue here to the differential corss-section.

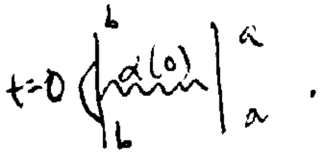
(ii) The connectivity and analyticity properties of multiparticle amplitudes are much more complicated than those of two particle ones; thus, probebility of error is greatly increased, and most cond usions in this field should be considered with that in mind.

(In what follows, I will take the point of view that at high-energy, amplitudes can be described by factorizable Regge poles, the leading one, $\alpha_p(t)$, passing near $\alpha = 1$ at $t = 0$. I shall come later to some of the problems connected with $\alpha_p(0) = 1$; for the moment, in order to avoid them, let me assume $\alpha_p(0) = 1 - \epsilon$, where ϵ is small but finite.

To continue with Mueller: Mueller applied his relation to the $a \to c$ fragmentation region, for which the Regge diagram is

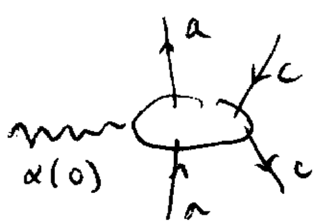


and thus showed that limiting fragmentation or Feynman scaling holds in Regge theory; the dependence drop out by factorization after dividing by σ_{ab}, for which the Regge diagram is

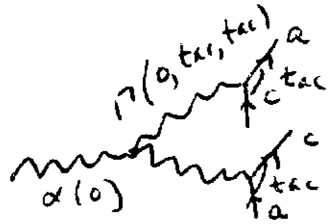


The approach to the limit should come via the next trajectory, and thus be $\sim s^{\alpha_M(0) - \alpha_p(0)}$ or $\sim s^{-1/2}$. Here α_M is an average meson trajectory. We have heard that all this fits quite well, with the possibile exception of the processes $p \xrightarrow{\pi^+} \pi^+$ vs $p \xrightarrow{p} \pi^+$.

In the triple Regge limit, $s/M^2 \to \infty$, or $1-x$ becomes small (but not too small overall); the blob

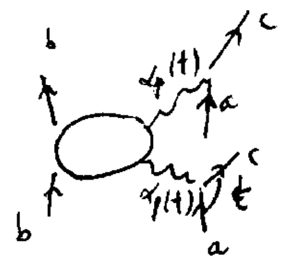


is opened up to



and the triple-Regge coupling Γ is introduced.

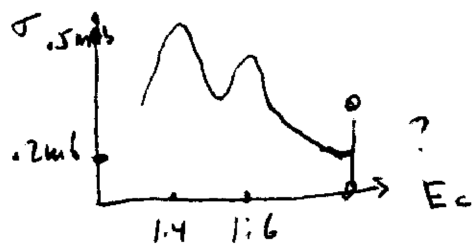
I mention here a related report by Ter-Martirosian on a deduction from experiment of a different quantity, the pomeron-particle total cross-section, $\sigma_{pb}(M^2, t)$ deduced from the inclusive process



$$\frac{1}{\sigma_{ac}} E\alpha \frac{d\sigma_{ab}}{dp_c} \sim \left(\frac{s}{M^2}\right)^{2\alpha_p(t)-1} \sigma_{\underline{pb}}(t, M^2) + \cdots$$

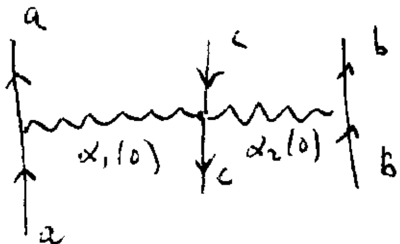
for S/M^2 sufficiently large and particle c = a. Here one must pick out the coefficient of $(S/M^2)^{2\alpha_p(t)-1}$. The momentum transfer t becomes the mass squared of the pomeron. As $M^2 \to \infty$, $\sigma_{pb} \to (M^2)^{\alpha_p(0)-1} \; \Gamma(0,t,t)$ where Γ is now the triple pomeron coupling. (Note that phase space factors and t dependent definitions are being treated very cavalierly.)

He finds roughly a curve of the form



We shall see later that this cross-section is of importance in evaluating the Pomeron-Pomeron cut.

The Mueller approach to scaling in the pionization region of c is



slower, like $s^{-[\frac{\alpha_p(0)-\alpha_M(0)}{2}]}$, or $\sim s^{-1/4}$, since the Regge diagram there is with s_1 and $s_2 \sim \sqrt{s}$. The relevant trajectories for scaling are $\alpha_1 = \alpha_2 = \alpha_p$; for the next correction are $\alpha_1 = \alpha_p$, $\alpha_2 = \alpha_M$ and vice-versa. As we saw in the Ferbel plot, this also seems to work quite well. Thus the data are consistent with a Regge picture of factorized poles, and a leading pole at $\alpha = 1$. Wrobleski's formula is inconsistent with Regge picture, but the $1/s^{1/4}$ approach to scaling could be very slow in 2 part distribution.

Still keeping our assumption of $\alpha_P(0) = 1-\epsilon$, we next ask about cuts. If $\alpha_P = 1-\epsilon + \alpha' t$, the two pomeron cut is at

$$\alpha_c = 2\alpha_p(t/4)-1 = 1-2\epsilon + \frac{\alpha' t}{2}$$

and will contribute a term of order $s^{-\epsilon}$ smaller than the pole at $t = 0$. It is therefore of considerable importance to find a way of calculating the sign and magnitude of this contribution.

This is a subject that has been controversial for many years, and I have to report with some embarassment that the controversy is still raging.

The cross-section will have the form

$$\sigma_{t_{ab}} = \beta_{ab}\left[S^{\alpha_p(0)-1} + \int_{-\infty}^{\alpha_c(0)} S^{J-1}\ \rho_{ab}(J)dJ + - \right]$$

and the question concerns the function $\rho(J)$, the cut discontinuity.

The first approximate calculation using s channel unitarity, by

Amati, Fubini and Stanshellini, gave $\rho(J = d_c) = +\frac{\beta}{32\pi\alpha'_p}$; the next also approximate, by Mandelstam, using t channel unitarity, changed the sign; we remark that the minus sign would be obtained by a Schroedinger equation iteration. Gribov, using Feynman diagrams, agrees with the Mandelstam sign, and Gribov and Misdal have given a purportedly exact formula based on Grobiv's earlier work

$$\rho(J = \alpha_c) = -\frac{\beta}{32\pi\alpha'_p}\left(1 + \eta + \underset{\text{triple pomeron}}{\underset{\downarrow}{\text{correction}}}\right)$$

where

$$\eta = \frac{\sigma_{DIF}}{\sigma_{EL}}$$

and σ_{DIF} is the single plus double diffraction cross-section of a on b, and σ_{EL} the corresponding elastic cross-section. The correction depends on the explicit presence of P-particle diffraction scattering, i.e., the P pole itself in $\sigma_{p\text{-particles}}$, or the triple Regge vertex. This correction has also been considered by Muysnich, Pasye, Treiman and L. L. Wang. Ter-Martirosyan and Kaibolob have attempted an evaluation of η using the P-particle cross-section given earlier and found .2±.2 for nn, .4±.2 for πN and .6±.3 for KN. The numerical importance of the entire effect is perhaps 20% in the total cross-section, but at present energies is indistinguishable from a pole. It is already the case that the emperical success of factorization raises difficulty for large cuts.

IMPORTANT: Cannot yet separate until $s \sim 10^4$.

Within the last few months two more purportedly exact formulae for $\rho(J)$ have been derived, one by White, of Cambridge, and one by Abarbanel of NAL, both using s matrix-mentods, White is the t channel, Abarbanel on the s channel. They obtain results which differ from each other; White agrees with Grobiv, thus White has the minus sign, Abarbanel the plus sign. All three calculations involve extremely subtle points, Gribov and Abarbanel of possible double counting, and White of analytic continuation.

I am afraid I cannot give you a judgment, although I have an opinoin. I am sure we will have a unaminous decision before our next meeting.

Turning from bad to worse, I next examine the question of the consequences of $\alpha_p(0) = .1$ exactly. It has been known for many years, based on approximate calculations, that this assumption seemed to lead to inconsistencies. However, in the last few years, these inconsistencies h have been sharpened by the use of the Mueller technique, and the use of energy-momentum conservation sum rules, such as

$$(p_a + p_b)_\mu \sigma_{ab} = \Sigma_c \quad (p_c)_\mu \frac{d\sigma_{ab}}{dp_c} \; dp_c .$$

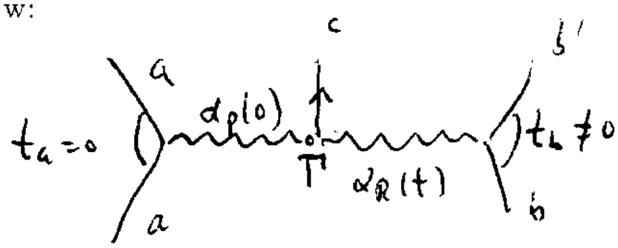
If you assume

1. a factorizable Regge pole$_p$ with $\alpha_p(0) = 1$, Re $\alpha_p{}'(0) \neq \infty$,

2. non-leading cuts, i.e., no terms like log (logs) in σ_T, then it follows that

(1) the triple pomeron vertex vanishes at $t = 0$, that is, the triple Regge term $(S/M)^{2\alpha_p(t)} (M^2)^{\alpha_p(0)}$ in any inclusive cross-section vanishes at $t = 0$. This result emerges from the Gribov Reggeon calculation; it was also shown more directly by Abarbanel, Chew, Goldberger and Saunder.

(2) from this it follows that the double Regge coupling $\Gamma\,(\alpha_p(0),\ \alpha_n(t), c)$ vanishes for all trajectories R and comentum transfers t. This coupling is measured in the process $a + b \rightarrow a + b' + c$, as shown below:



The last step is continuation to any particle pole in the trajectory α_p; this shows immediately that the coupling of $\alpha_p(0)$ to particles c and c' vanishes. For c = c', there are special subtleties, connected with certain kinematic singularities that appear as $t_a \rightarrow o$, $t \rightarrow m_c^2$. However, Brower and Weis have studied the problem and have ashown that the elastic pomeron coupling must vanish. I should point out that this last conclusion (perhaps not surprisingly) is also controversial--the same Dr. White previously cited, together with Moen, claims that it is incorrect. I would say, even if White is right, it is clear that the pole α_p with $\alpha_p(0) = 1$ has great difficulties associated with it. For the benefit of those who blame this on moving Regge poles, I remind them that the fixed pole at J = 1 [is f(t) in the forward amplitude] makes things worse, not better.

At this point, I should interject a worning. We insist in all this in talking about asymptotic limits; however, there may be no region in which an asymptotic limit of strong interactions makes sense, even for diffraction processes. For example, in a Fermi theory of weak interactions, as is well known, the weak interaction will complete with the strong at the unitarity cut-off $s_o \cong \frac{1}{G} \cong (300 \text{ BeV})^2$. The consistency we are demanding (and which is violated by $\alpha_p(0) = 1$) requires energies so high that log (logs) $\gg$ 1; that is, the inconsistancy is shown by deriving the inequality const. > log (logs); but log (log s_0) ~ e. Even if a renormalizable weak interaction theory of the type Dr. Lee has discussed holds, where the theory becomes strong at $s \sim e^{137}$, one finds log log 137 is still < 6! Therefore, it may be that our asymptotic is unphysically far away, and that we have no right to demand a consistent theory without taking into account other interactions, since by the time our theory becomes inconsistent the strong, weak and electromagnetic interactions will have become mixed up.

Nevertheless, one may whish to achieve an absolutely consistent theory of strong interactions. This is not so stupid as it seems; it only means that we solve equations consistently that do not include these weak and electromagnetic interactions. If we do, the equations will not give us a pole at $\alpha \neq (0) = 1$. They will do something else. For example, they may give us $\alpha_p(0) = 1 - \epsilon$, as previously discussed. If so, there appears to be two ways out. The first, of course, is to have $\alpha_p(0) = 1 - \epsilon$, as previously discussed. If the cuts are moderately weak, this theory could look a lot like a simple pole at $\alpha_p(0) = 1$, approximately factorize, and account for present data. The only questions is, why is ϵ so small? That is, why is $\alpha_p(0)$ so near 1? This is also not quite so ridiculous as it seems. A theory that gives an $1 - \alpha_p(0) = \epsilon \neq 0$ will have $\Gamma_{c<p} \neq 0$, and there will be some constants relating then. Chew reports calculations of Sorenson that do this.

The second way out is to abandon the pomeron pole, and assume only cuts. The point here is that cuts do <u>not</u> factorize, and so invalidate the theorems. They also seem hard to reconcile with experiment for the same reason. Schwarz suggested several years ago that a cut might generate itself self-consistently, according to the equation

$$\alpha_S(t) = 2\alpha_S(t/4) - 1 \qquad \text{(S for Schwarz)}$$

whose unique solutions are

$$\alpha_S(t) = 1 \pm c\sqrt{t}, \qquad \text{c arbitrary, but real;}$$

you see that this assumption naturally chooses the value $J = 1$ at $t = 0$. Notes written to yourself as follows: σ vs log s, singularity, F + Zach $(\log s)^2$, B + Zach log s A second natural way of arriving at $J = 1$ is the Froissart mechanism. Froissart showed that a potential that was exponential in r, i.e., $e^{-\mu r}$, and that grew like a power of s, thus

$$V \sim s^{a}\, e^{-\mu r} \underline{p}(r),$$

with $a > 0$ and $\underline{p}$ nonexponential, would be cut down by unitarity to give a cross-section

$$\sigma_T \underset{S \to \infty}{\sim} \text{llog } s)^2.$$

Cheng and Wu, and independently Gribov and collaborators, have shown how to achieve such a potential by summing selected diagrams in field theory, and how to unitarize the potential by summing some more. Most interestingly, they find an amplitude whose leading singularity is precisely the Schwarz cut, with a singular discontinuity leading to the expected log $s)^2$ growth of the cross-section. Their model consists of spin 1/2 baryons coupled to a neutral vector gluon. I don't think one can take the details of their calculation too literally; however, the general conclusions are very suggestive. Dr. Walker of NAL has fitted all pp elastic data with their parametrized version of the Cheng-Wu model and claims good agreement. In spite of the $(\log s)^2$, the pp cross-section remains quite constant up to medium ISR energies. To give you an idea of what to expect from this kind of a model, σ_{pp} at T energies (400 BeV c-of-m.) should be pushing 50 mb, i.e., an increase of 25%.

In summary, the cut models

1. choose J=1 in a natural way
2. they avoid the problem of a pole at J=1.

However, they have only minimal factorization properties at best.

It may be that somehow the cut simulates a pole (and therefore factorizes approximately) at low energies. For example, in the Cheng and Wu model, it may be a good approximation to set $a = 0$, which could then have to have some factorization properties.

In conclusion, there seems to be three possibilities:

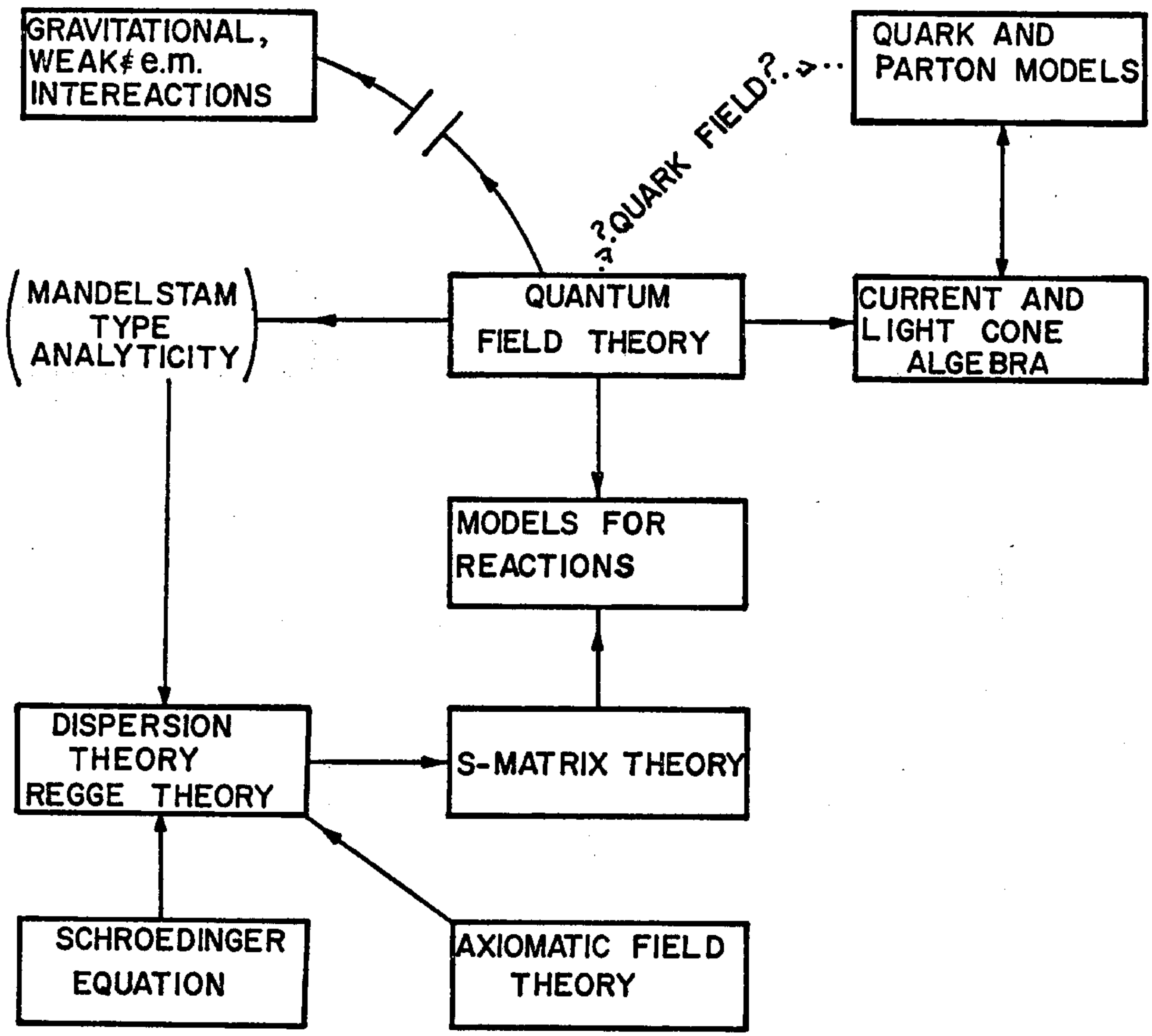
1. $\alpha(0) = 1$, $\Gamma \neq 0$, and thus leading cuts like log (logs), and the contradiction resolved only by electromagnetic and weak interactions at very high energy,

2. $\alpha(0) = 1-\epsilon$, with cuts, factorization of the pole, and as we have seen, calculabilities of cuts. Question: why is ϵ so small?

3. Essential cuts - no problem of principle, but why do high enerby processes factorize so nicely?

Probably, the right answer is a fourth!

Fig 1



GRAVITATIONAL, WEAK & e.m. INTEREACTIONS
QUARK AND PARTON MODELS
?QUARK FIELD?
MANDELSTAM TYPE ANALYTICITY
QUANTUM FIELD THEORY
CURRENT AND LIGHT CONE ALGEBRA
MODELS FOR REACTIONS
DISPERSION THEORY REGGE THEORY
S-MATRIX THEORY
SCHROEDINGER EQUATION
AXIOMATIC FIELD THEORY

Fig 2

FIRST ORDER PROBLEM OF DUAL MODELS

	PROBLEM	SOLUTION
1.	GHOSTS	(BROWER, GODDARD AND THOM)
2.	TACHYON	(NEVEU-SCHWARZ)
3.	SPIN 1/2	(UNSOLVED)
4.	CORRECT SPECTRUM	"
5.	DIFFRACTION SCATTERING. (POMERON EXCHANGE)	(REQUIRES EXOTICS PRESUMABLY O.H. WITH DUAL LOOPS)
6.	UNITARITY	(PRESUMABLY O.H. WITH LOOPS)
7.	CURRENTS (as in q.e.d.)	(UNSOLVED)

Fig 3

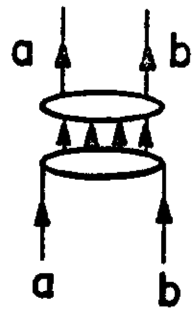
HIGHER ORDER PROBLEMS OF DUAL MODELS

PROBLEM	SOLUTION
1. $d < 26$: VACUUM SINGULARITY HAS UNACCEPTABLE T BEHAVIOR	(CORRECT FIRST ORDER(?))
2. $d = 26$: DUAL LOOPS ARE NON-RENORMALIZABLE	" "

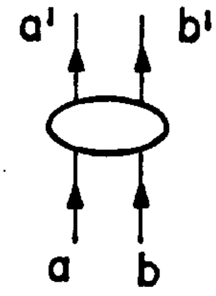
Fig 4

1. OPTICAL THEOREM

$\sigma_{ab} = \sum$



$= \mathrm{disc}_s$

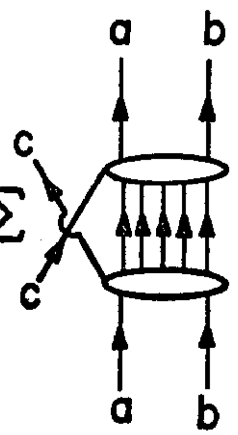


$S=(a+b)^2$

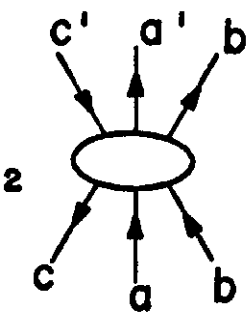
$a', b' \rightarrow ab$

2. GENERALIZED OPTICAL THEOREM

$E_c \dfrac{d\sigma_{ab}}{dp_c} = \sum$



$= \mathrm{disc}_{M^2}$



$a', b', c' \longrightarrow a, b, c$

$M^2=(a+b-c)^2, \; t=(a-c)^2$

$$E_c E_d \frac{d\sigma_{ab}}{dp_c\, dp_d} = \ldots\ldots\ldots$$

etc.