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Geoffrey F. Chew

After some previous lectures that I have given on the S-matrix I have been accused of an almost religious intolerance of the field theoretical approach. I'm afraid that there is at least a grain of truth in this accusation in the sense that I sometimes express my feelings on the subject with emotion - and science is supposed to be carried on without emotion. Also, according to the gentlemanly rules of the game one does not suggest to one's colleagues - but only to one's students- what problems are worth investigating. To imply that a line of investigation chosen by many colleagues is likely to be fruitless is in bad taste. Why have I sometimes violated this very good custom?

I have behaved badly because of an intense desire to see physicists solve the problem of the microscopic universe within my intellectual lifetime - that is - within the span of years in which my brain will be capable of appreciating the solution. One of the great tragedies of science is that the finiteness of human life more often than not cuts off the story for the individual scientist in the middle of an exciting chapter. Of course no one ever will get to the end of the book, but there have in the past been rather well-defined chapters and presumably we can count on a continuation of this precedent for the future. In particular, one may reasonably anticipate a comprehensive solution of the microscopic problem without invoking cosmological considerations, and this is the chapter on whose last page I have my heart set.

But _if_ I am to see the last page it will be because of the work of others during the next twenty years and that is why I cannot resist the temptation to push others in what I am convinced is the direction of progress. When I encounter a talented theoretical physicist playing games with field theory - and enjoying the games immensely - his happiness should make me happy. But it doesn't; all I can think of is that this same brain disentangling the S-matrix would bring that last page closer.

It is an entirely selfish attitude, but that's the way it is.

Before going further I should make clear that my convictions about the sterility of field theory extend so far only to the strong interactions. It is only here that experimental evidence is unmistakeable that the elementary particle concept has outlived its usefulness. The leptons and the photon have special and remarkable characteristics that set them apart; perhaps for weak and electromagnetic interactions we still can learn something within the apparatus of field theory. But <u>all</u> the strongly interacting particles from the pion to the transuranic nuclei seem to have an equivalent status. Each is a composite of all the others; none is more elementary than another in any essential sense. We <u>must</u> find a theoretical framework that recognizes this equivalence from the start.

Heisenberg has proposed a field theory involving a single fundamental matter field from which all particles are supposed to emanate, but he has had difficulty in evaluating its predictions. Heisenberg is <u>not</u> playing games, but in my opinion his technique of calculation (if not his basic equations) places the spin $\frac{1}{2}$ particles in a preferential position over other particles and is therefore doomed to frustration. All concrete approaches based on the field concept, in fact, seem to share this kind of deficiency to some degree. Considering such a basic flaw together with the well-known divergence difficulties and the inadequacy of perturbation calculational methods for strong interactions, one must conclude that the field concept is currently throwing no light on the nature of strong interactions.

Historically, of course, Yukawa's ideas about nuclear forces arose from field theory. But the quantitative realization of these ideas has been achieved through the S-matrix in a way that makes the field concept irrelevant. One now realizes that the general force between two nuclear particles arises from the exchange of <u>all</u> kinds of nuclear particles (not just pions) and that the force strength is given in terms of appropriate elements of the analytically continued S-matrix. I may add that where the force picture is sufficiently simple to be theoretically predicted, at least approximately, the result has always been in accord with experimental evidence. Given a knowledge of which particles exist, one may say with considerable confidence that forces between strongly interacting particles have now been understood through the S-matrix. To know which particles exist, of course, is to know a great deal; I shall return later to this point.

Other historical achievements of field theory have been the connection between spin and statistics and the T.C.P. theorem, as well as the very notion of anti-particles, but Henry Stapp has been able to show that these relationships are also consequences of a pure S-matrix approach, so again the field concept is unnecessary. Every now and then someone thinks of another relationship deduced from field theory (such as the fact that boson and anti-boson have the same intrinsic parities while fermion and anti-fermion have opposite parities), but until now Stapp has always been able to get the same result out of the S-matrix. All the solidly correct aspects of field theory, in other words, seem to be embedded in the analytically continued S-matrix. Those aspects that cause difficulty are absent.

Does this necessarily mean that field theory is wrong? One may be confident that old-fashioned Lagrangian field theory is wrong, but experts such as Wightman and Haag have long ago given up on such an unsophisticated version. They have accepted the absence of a preferred status for particular particles and work with the field notion in an extremely broad sense. This type of activity has progressively become more mathematical and less physical, however, and its practitioners see little hope of coming to grips with experimental realities. Those field theorists who try to approach concrete physical questions are still forced to work with Lagrangians, where for strong interactions the difficulties already mentioned are manifest. Nevertheless the playing of games with Lagrangians still goes on, as a glance at any issue of the Physical Review or Nuovo Cimento will show.

The troubles with field theory have been known for such a long time that one must ask why its adherents are so loyal. Partly, the answer is that they have expended an enormous effort in learning field theoretical techniques; partly it is that many of them do not yet understand the techniques suitable to the S-matrix. Partly they feel (and they are right) that there does not yet exist a mathematically well-defined set of postulates based on the S-matrix. The essential point, however, is that the very notion of a relativistic space-time continuum of points seems to imply the existence of fields. Those of us who are willing to give up fields must be willing to abandon space and time in the microscopic sense.

Actually it has been known for thirty years that the combination of relativity and quantum mechanics precludes an experimental localization of space-time to distances smaller than $\hbar/Mc$, where M is the mass of the lightest particle in question. (For strong interactions this is the π-meson, whose mass corresponds to a distance 10^{-13}cm. or a time 10^{-23} sec.) Leaning on the philosophy so important to the development of quantum theory that concepts immune to experimental test are to be avoided whenever possible, one must be dubious about the existence of a space-time continuum. And of course some of us are dubious. The great majority of theoretical physicists, however, cannot yet bring themselves to throw into the street this old mistress (as Gell-Mann puts it) with whom we have slept so long and with so much satisfaction.

Now why am I so sure that the S-matrix is going to carry us a long way toward the end of the microscopic chapter. Stated simply, I have faith that the three fundamental principles on which S-matrix theory rests will not fail within the microscopic domain; and the combination of these three principles has enormous physical content still to be explored. What are these principles? The first two were clearly stated by Heisenberg in the early forties, Lorentz invariance and unitarity; these have never been seriously challenged either theoretically or experimentally. To give the theory dynamical content, however, a third principle must be added: maximal analyticity. Two questions arise here: (1) What does maximal analyticity mean? (2) Is the principle to be trusted? Let me concede immediately that a precise mathematical statement of maximal analyticity has not yet been achieved. However, the search for such a statement is the subject of an intense effort by a number of highly talented people at the present moment, and I do not know of any of these who doubts that the statement will eventually be found. Polkinghorne and Stapp have published the most ambitious efforts in this direction, and in the few months since their papers appeared more has been learned. Crudely speaking the following picture seems to be emerging:

The S-matrix as an analytic function of energies and momenta appears to have only pole and branch point singularities. Each pole corresponds to a particle and each particle to a pole, the real part of the pole position giving the particle mass and the imaginary part the lifetime. There are an infinite number of poles, but most are so far from the physical region that the corresponding particles will never be observed. A few lie on the real energy axis, corresponding to stable particles, and a larger but finite number sufficiently near to the real axis so that they can be observed as resonances. A given pole appears in all S-matrix elements where the quantum numbers are appropriate, and as Stapp has emphasized one can easily see that the existence of poles is essential to the physical interpretation.

The point is that a given S-matrix element is always experimentally determined through a pole in <u>another</u> element of <u>higher</u> dimensionality. For example, one might detect one of the protons following pp scattering in a Geiger counter where it collides with an electron. Thus the overall process is

$$p_1 + p_2 + e_1 \rightarrow p_3 + p_4 + e_2 \quad ,$$

which for most values of the energies and momenta is highly improbable, but we are looking in a special region of the variable

$$(p_4 + e_2 - e_1)^2$$

where the S-matrix element is enormously enhanced by the presence of a pole

$$\sim \frac{\Gamma}{(p_4 + e_2 - e_1)^2 - m_p^2} \quad .$$

We interpret this situation, of course, in terms of two successive scatterings,

$$p_1 + p_2 \rightarrow p_3 + p_4', \text{ and } p_4' + e_1 \rightarrow p_4 + e_2 \quad ,$$

which is possible because the residue Γ factors into the two matrix elements appropriate to the simpler processes:

$$\Gamma = \left\langle p_4, e_2 \,\middle|\, A \,\middle|\, p_4', e_1 \right\rangle \left\langle p_3, p_4' \,\middle|\, A \,\middle|\, p_1, p_2 \right\rangle .$$

It has been shown that the residues of poles always factor in this way, leading to the common-sense result that two successive scatterings - with a macroscopic spacing between them - are independent of each other.

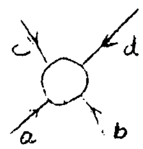
Thus the presence of poles in the S-matrix follows from elementary requirements of a consistent physical interpretation. Where do the branch points arise? These have a completely different origin, stemming from the competition between different channels, that is to say, from the unitarity condition. It was realized long ago that at the energy threshold of a reaction a scattering amplitude must have a branch point whose character is determined by the multiplicity of the channel that is opening. From the more modern point of view one finds that the existence of poles in variables other than the total energy automatically leads to branch points in the total energy when an analytic continuation is made of the unitarity condition. Some of these branch points occur in the physical region and may be identified with normal thresholds, but the great majority lie in unphysical regions. Nonetheless the location and nature of all such branch points appears to be determined entirely by unitarity once the location of all the poles is known. They are called the Landau singularities because a compact recipe for their location was given in 1959 by Landau in terms of diagrams. The same recipe was discovered independently by J.C. Taylor and Bjorken.

Not only does the unitarity condition prescribe the location and nature of branch points, however, but it leads to explicit formulas in terms of products of S-matrix elements for the discontinuities across the cuts associated with the branch points. These formulas are sometimes associated with the name of Cutkosky; as I'll discuss later they give S-matrix theory its dynamical content.

Roughly speaking maximal analyticity is the assumption that S-matrix elements are free from singularities, apart from the poles and the branch points required by unitarity in the presence of poles. There is by now abundant experimental support for maximal analyticity for both energy and angle variables in the neighbourhood of physical regions where scattering amplitudes can be directly measured. In this connection it is noteworthy that we know of no absolute limit to our ability to measure momenta and energies; the assumption of a point continuum in these variables is entirely consistent with the non-existence of a space-time continuum. We might not expect experiment to tell us much about regions of the complex plane distant from the physical region, but surprisingly it has in one case: the forward-direction pion-nucleon scattering amplitude. Through Goldberger's dispersion relation - which is simply Cauchy's theorem combined with maximal analyticity - it has been possible to verify experimentally that for a distance of the order of 2 Gev in all directions in the complex energy plane there are no strong singularities other than the expected poles and branch points. (a weak singularity would of course go undetected.)

When one adds to this picture the success achieved on the basis of maximal analyticity in understanding the forces acting between nucleon and nucleon, pion and nucleon, pion and pion, the total evidence seems to me overwhelming. No theoretical assumption ever has or ever will be completely verified from an experimental standpoint. What happens is that when a sufficient number of partial tests have been passed and no theoretical contradictions uncovered one's skepticism about the assumption dies away. Each physicist has his own idea of what is "sufficient", of course, and many are not yet willing to accept maximal analyticity. A growing number do accept it, however, and needless to say I am in this group. When I am pressed for a philosophical justification I find it in the notion of "lack of sufficient reason". It seems to me natural that scattering amplitudes should be smooth functions of their continuous variables and a natural mathematical expression of smoothness is through analyticity. The only irregularities are those forced by the unitarity condition. There is no "reason" for any others.

So closely associated with maximal analyticity that it may be regarded as part of the same assumption is the particle-antiparticle relationship in the S-matrix. One finds that because of the relativistic connection between energy and momentum, $E^2 = p^2 + m^2$, both positive and negative energy regions in any S-matrix element are capable of physical interpretation. If the positive energy interval refers to a particular incoming particle, then all conservation laws can be maintained throughout the analytic continuation if when we reach negative energies we say that we are now talking about an outgoing particle with all internal quantum numbers reversed, i.e., the anti-particle. Thus, for example, an S-matrix element schematically represented by



has six different physical regions corresponding to the reactions,

$$a + b \rightarrow \bar{c} + \bar{d} \qquad c + d \rightarrow \bar{a} + \bar{b}$$
$$a + c \rightarrow \bar{b} + \bar{d} \qquad b + d \rightarrow \bar{a} + \bar{c}$$
$$a + d \rightarrow \bar{b} + \bar{c} \qquad b + c \rightarrow \bar{a} + \bar{d}$$

and all are interconnected by analytic continuation. It also turns out that analytic continuation from total energy E for any given reaction around the lowest threshold branch point to a point E* on the opposite side of the cut leads to the complex conjugate of the physical amplitude for the inverse reaction. The total number of physical regions for our one analytic function is thus increased to twelve.

From each of these regions separate analytic continuations may be based on the appropriate unitarity conditions, and all are supposed to define the same analytic function! How can such a miracle possibly happen? We don't know the answer in general yet, but Mandelstam in 1958 found a mechanism that smoothly connects the elastic regions of two-body amplitudes in a way consistent with all twelve unitarity conditions. Polkinghorne and Stapp subsequently have used diagrammatic arguments to support the belief that such continuations exist in the general case.

Roughly speaking, the forces generating any given reaction arise from singularities due to the unitarity condition in the reactions reached by analytic continuation. (We usually call these the "crossed reactions"). Sometimes these crossed singularities are known from experiment and then one can make predictions about the forces acting in the original reaction. These are the predictions I have twice referred to as already constituting a major success for S-matrix theory.

S-matrix theory on the basis of Lorentz invariance, unitarity and maximal analyticity in the sense already discussed should be able to encompass weak and electromagnetic as well as strong interactions, although the problem of physical interpretation when there are zero mass particles has never been seriously analysed. (E.g. all experiments involve an infinite number of soft photons). Nature, however, has seen fit to make a sharp distinction between nuclear particles and the leptons and photon, and I now come to a conjectured property that Frautschi and I wish to associate with the strong-interaction part of the S-matrix. Crudely speaking, we want not only all branch points but also all poles to be consequences of the unitarity condition.

A conceptual correspondance between S-matrix theory and old-fashioned Lagrangian theory may be maintained if one asserts that the positions and residues of <u>certain</u> poles are to be arbitrarily specified. These then correspond to elementary particles, the pole position giving the mass and the residue the coupling constant. It is clear, however, that not all the strongly interacting poles can be arbitrarily specified. For example, in 1955 a connection between the residues of the nucleon and N_{33}^* poles was pointed out by Low and me and recently Froissart has shown on general grounds that independent assignment of poles with spin greater the 1 violates unitarity. Since all strong poles seem on a dynamically equivalent basis Frautschi and I go to the opposite extreme and propose that none are arbitrary, the positions and residues of all being determined by unitarity.

This notion was first given concrete form in terms of the so-called CDD poles, whose possible presence with arbitrary position and residue in partial wave amplitudes was pointed out by Castillejo, Dalitz and Dyson. Frautschi and I made the negative suggestion, in other words, that strong CDD poles simply do not occur. Then, however, we became aware through Mandelstam of Regge's magnificent discovery that poles corresponding to composite particles in non-relativistic potential theory may be analytically continued as a function of angular momentum. This led us to the positive notion that all strong poles are Regge poles. We believe that the negative and positive concepts become equivalent if one requires in addition that for sufficiently large energies all Regge poles retreat to the left half of the angular momentum plane, i.e., to Re $J < 0$.

The point here is that if the only singularities in the right-half J plane are poles then the asymptotic behaviour in momentum transfer goes with a power equal to the position of the rightmost pole. If for some energies the J-plane position of all poles has a negative real part, scattering amplitudes must tend asymptotically to zero for large momentum transfers; Cauchy relations then lead to unambiguous formulas (because the big loops at infinity may be completed) that leave no room for CDD parameters. The current tendency is to associate the leptons and the photon with CDD poles, because it is difficult to see how the special characteristics of these particles can arise purely from the dynamics.

It must be emphasized that the possibility of a consistent analytic continuation in J for the full S-matrix has not yet been established. In particular, Amati, Fubini and Stanghellini and also Mandelstam have suggested that difficulties occur when channels with more than two particles are considered. A substantial group of optimists, however, including Gribov and Pomeranchuk, Gell-Mann, Goldberger, Blankenbecler, Domokos and Lovelace as well as Frautschi and me, have been fascinated by the enormous insight into strong interactions to which Regge's idea leads, if it can be generalized. My feeling is that this insight is so great that there <u>must</u> be something to it, even if the picture turns out to be more complicated than just poles.

I should love to tell you about the exciting predictions concerning the high energy behaviour of cross-sections that follow if one is optimistic about analytic continuation in J, but there just isn't time tonight. (You may find a brief survey of some points in my Reviews of Modern Physics article, just published.) I shall only say here that many striking features of high energy experiments appear to be clarified and a close connection established between high and low energies. Furthermore, the combination of all low and high energy data, when examined from the point of view of Regge poles, supports the idea that there are no arbitrary parameters in the strong interaction S-matrix. In other words it appears that the combination of Lorentz invariance, unitarity and maximal analyticity in the extended sense (no CDD poles) may very well be sufficient to determine <u>which</u> nuclear particles exist, as well as their masses and mutual interactions.

The techniques by which one approaches the strong interaction problem at present are clumsy and sure to be superseded as time goes on. The basic difficulty is that we have trouble at present in thinking about the S-matrix without knowing its dimensionality in advance. But to assume a particular dimensionality is to assume at least some of its poles, and this must be avoided in a fundamental approach. A related difficulty is that of the strong-interaction quantum numbers, B, S, and I, as well as parity and time-reversal. It seems reasonable to suppose that these are not to be arbitrarily inserted but should emerge from considerations of self consistency. I believe they eventually will, but no effective approach to such questions has yet appeared.

Anyone not deeply involved in S-matrix dynamics may feel discouraged by the complexity of the overall problem, but the history of the subject over the last eight years offers encouragement. Progress has been sustained by people trying to solve relatively small and specific problems - usually motivated by experiment. Herein lies the secret weapon of S-matrix theory that guarantees its continued vitality. Because the fundamental quantity in the theory is in certain regions susceptible to direct measurement, one often is able to cheat and take a peek at the answer. Knowing what the answer is, even though its origin may be obscure, gives S-matrix theorists an enormous advantage. A good example is the fact that total cross-sections all approach constants at high energy. This simple empirical fact was of no use at all to field theorists, but because one region of the S-matrix is just as fundamental as any other we felt free to start making analytic continuations from the forward direction at high energy where we <u>knew</u> the situation had to be simple. The exciting Regge pole developments were the consequence. Another example is Froissart's proof that cross-sections cannot increase as a positive power of energy; he was encouraged to seek a proof by knowing that they damn well don't. Still another example is the Mandelstam representation; this was developed because a formula suggested by Francis Low and me for the 33 resonance actually fitted experiment. Mandelstam concluded that there <u>must</u> exist a relativistic generalization of the static model with which we had worked; and he found it.

I see no reason to expect this interplay between experiment and S-matrix theory to stop. On the contrary; I am sure it will grow and lead us to that last page. We are still a long way off, but if I may change metaphors, the trail is hot and the hunters are eager. If nuclear war does not fall upon us during the next twenty years, there is a sporting chance that my wish will come true.

Thank you.