S-MATRIX THEORY WITH REGGE POLES*

Geoffrey F. Chew

Physics Department and Lawrence Radiation Laboratory,
University of California, Berkeley, California

Introduction
The idea of a nuclear theory based directly on the S-matrix goes back to the 1943 papers of Heisenberg\textsuperscript{1}. The motivation then, as now, was to avoid unobservable local space-time concepts which become troublesome when quantum principles are combined with relativity. Although macroscopic space-time is an essential component of S-matrix theory, there is no way to construct on-mass-shell wave packets whose spatial localization (in the particle rest frame) is sharper than the particle Compton wavelength. The divergences plaguing conventional local field theory correspondingly have difficulty finding their way into S-matrix theory.

Heisenberg clearly identified two key S-matrix properties, Lorentz invariance and unitarity, and partially appreciated a third—analyticity in momentum variables. The special aspect of the latter that interested him was the correspondence between poles and particles, an essential idea pinpointed by Kramers. S-matrix theorists of the forties, however, did not appreciate the concept now called 'maximal analyticity', and correspondingly they came to believe that interparticle forces are necessarily ambiguous without appeal to local space-time concepts.

During the late fifties the potent dynamical content of analyticity became apparent, with the generalization by Mandelstam\textsuperscript{2} of the static pion-nucleon force model that had been developed by Low and myself\textsuperscript{3}. Other essential precursors to Mandelstam’s double dispersion relations were the single dispersion relations, first formulated correctly for hadrons by Goldberger\textsuperscript{4}, together with the principle of crossing—identified by Gell-Mann and Goldberger\textsuperscript{5}. It is by now familiar how analytic continuation in both angle and energy variables, without any appeal to local space-time, generalizes the Yukawa concept that forces between hadrons are due to exchange of other hadrons.

In studying the dynamics of analyticity by the so-called $N/D$ method, Mandelstam and I in 1960 found a relativistic definition of the force concept and became aware that forces due to exchange of particles considered

* This work was supported in part by the U.S. Atomic Energy Commission.
'composite', like the deuteron or the rho-meson, are unavoidably of the same character as forces due to particles considered 'elementary', like the pion or the nucleon. This led us to the partial 'bootstrap' idea that composite particles may generate themselves, although there was no immediate implication of 'nuclear democracy', i.e. that all hadrons are composite. Elementary particles were no longer essential to the dynamics, but they were not excluded. On the other hand it continues today surprisingly often to be forgotten that, once a composite hadron exists, general principles require it to generate forces of a strength and range determined by its mass and partial widths. These forces cannot be 'turned off' at the convenience of theorists in favour of conjectured forces due to exchange of elementary particles.

The definition of the S-matrix in terms of physical observables makes no distinction between elementary and composite particles, but through the pole-particle correspondence may not some difference be found? In his 1961 Solvay report Mandelstam described the attempt to employ a distinction that had arisen in static model work by Castillejo, Dalitz, and Dyson, when combined with a theorem of Levinson for potential scattering. The characterization of the so-called CDD poles, however, depends crucially on approximating the unitarity condition by a finite number of channels, no matter how high the energy, while experiments by now seem unequivocal in their indication that dynamics at higher and higher energies is dominated by channels with higher and higher thresholds. The prevailing opinion at present finds the CDD classification of poles unhelpful for the actual hadronic S-matrix.

At the same 1961 Solvay meeting there was for the first time intense discussion of an alternative S-matrix definition of 'compositeness': that nonelementary particles should correspond to poles continuable in angular momentum, i.e. Regge poles. Frautschi and I proposed, as a formulation of 'nuclear democracy', that all hadrons should lie on Regge trajectories. If it were found that certain hadrons did not exhibit the characteristic Regge recurrence at a succession of different spin values, these particles should be classed as elementary. Our suggestion was coupled with the conjecture, still tenable today, that there might exist a unique hadronic S-matrix which not only was Lorentz invariant, unitary, and analytic but which contained only Regge poles. It was proposed in other words that the combination of unitarity, Lorentz invariance and maximal analyticity might suffice to define a 'complete bootstrap' theory of strong interactions, without the need for input parameters or 'master equation of motion'.

This conjecture arose from our inability to discern in models based on master equations any hope for nuclear democracy. A favoured status for
certain special quantum numbers seemed inevitable for any master equation within the Lagrangian-Hamiltonian framework; the absence of a master equation to us meant ‘bootstrap’. You may ask, of course, why we rejected the ancient motion of elementary substructure. This was wishful thinking but with a historical motivation. Suppose some hadronic particle or field should have to be classed ‘elementary’; where would that put physics? Back in the morass where it had struggled since the late twenties. The conflict between quantum theory and relativity would remain.

The possibility seemed dazzlingly attractive that, in the hadronic domain, already identified \( S \)-matrix principles might render unnecessary the very idea of elementarity. To me at least this possibility was, and is, irresistible.

It is at first exceedingly hard to swallow the notion of dynamics without an equation of motion. Physicists who have learned to live with this apparent absurdity make no pretence of a clear grasp of dynamical completeness for general \( S \)-matrix principles. By concrete calculations, however, they have identified a force concept with at least as much precision as in models based on master equations. The difference is that the \( S \)-matrix force does not occupy a central position in the dynamics, being merely one link in a circular chain of constraints. In fact the constraints are so severe that no theoretical calculation has come close to satisfying all at the same time. Far from fearing that Lorentz invariance, unitarity and maximal analyticity are insufficient to define a complete dynamical theory, I worry that these requirements may be too much for any \( S \)-matrix.

In 1961 bootstrappers were insufficiently bold to forego the \textit{a priori} assignment of internal hadronic symmetries, but subsequent bootstrap research, especially by Cutkosky\textsuperscript{10}, has made it plausible that self-consistency requirements might lead uniquely to the observed pattern of \( SU_2, SU_3 \), etc. Arguments also have been given that parity and time reversal may be inevitable attributes of a self-sustaining dynamics.

The programme initiated by Heisenberg two dozen years ago is thus very much alive, the potentialities today appearing more exciting than ever. Let me move now to a more detailed examination of the current position.

\textbf{Maximal analyticity of the first degree}

Although \( S \)-matrix theory still lacks a well-defined axiomatic basis, among practitioners there is no divergence of opinion about a set of principles which I shall call ‘first-degree analyticity’. One key principle,
already implicit in Mandelstam's double dispersion relations, states that the only momentum singularities occur at the analytic continuation of the kinematic constraints corresponding to physical multiple processes. By a 'multiple process' I mean a reaction that proceeds via a succession of two or more macroscopically separated collisions. For example, double scattering of the type shown in Figure 1 implies a pole in the variable $(p_1 + p_2 - p_4)^2$ at $m^2$. Triple scattering of the type shown in Figure 2 implies a branch point when the initial and final momenta are such as to allow all three intermediate moments to be physical, that is, on mass-shell and corresponding to macroscopic space-time displacements that correctly add together. Stapp and coworkers have shown the connexion of such singularities with macroscopic causality. It was pointed out by Coleman and Norton that the analytic continuations of these multiple scattering conditions are precisely the Landau rules for the singularities of Feynman graphs. Outside the physical region of real momenta there are delicate questions of sheet structure, but Olive and collaborators have shown how these can be resolved by appeal to the analytic continuation of unitarity, which gives rules for the discontinuity associated with any Landau singularity. Olive's approach shows in fact that the very existence of the Landau singularities could be considered a consequence of unitarity. Macrocausality, in other words, is interlocked with the analyticity–unitarity combination.

Because any multiple scattering may be associated with a graph that looks like a Feynman graph, there is temptation to think of the S-matrix as a superposition of contributions from each of its singularities. The
singularities, however, are so intertwined that no simple superposition is possible. For example, a pole typically occurs on only one sheet of that Riemann surface defined by branch points which share the same quantum numbers as the pole. A manifestly correct statement is that sufficiently close to a pole the $S$-matrix can be approximated through a Laurent expansion by a knowledge of this pole position and residue. Breit-Wigner theory and effective range theory legitimately exploit this circumstance. Physicists nevertheless often may be heard speaking carelessly of a 'pole term'—implying that a given pole makes a well-defined contribution everywhere in the momentum space. There is no basis for the concept 'pole term' in $S$-matrix theory.

A further source of confusion with Feynman graphs is the factorizability of pole residues and the identification of the individual factors with reaction amplitude (connected parts) of lower dimensionality. The rule, derivable from unitarity, is most conveniently remembered through the multiple scattering diagram. The diagram of Figure 1, for example, not only identifies the pole but gives the physically obvious prescription that each factor in the residue must be a 2 particle $\rightarrow$ 2 particle scattering amplitude. This result sounds like one of the Feynman rules, but the individual factors are themselves complete scattering amplitudes.

By the use of factorization, amplitudes (connected parts) may be unambiguously defined for unstable particles, and the rules for the singularities of unstable-particle amplitudes look like the ordinary rules—except that complex masses occur. By the same token, analysis of unitarity shows that unstable particles must be included among intermediate configurations in the diagrammatic enumeration of singularities. First-degree analyticity ends up treating all particles, stable and unstable, on essentially the same footing, even though the multiple scattering picture at the beginning picks out stable particles for a special role.

Included in what I call 'first-degree analyticity' are the principles of 'crossing' and of 'hermitian analyticity'. Olive has made considerable progress in showing that these properties of the $S$-matrix are physically inevitable, but their precise axiomatic status remains obscure. Suffice it here to remind you that crossing associates negative energies with antiparticles while hermitian analyticity ensures real masses and coupling constants for stable particles and allows the unitarity condition to be analytically continued as a discontinuity equation.

The above aspects of first-degree analyticity were already embodied in the 1958 Mandelstam representation, but that work was restricted to the four-line connected part with low spin. The generalization to arbitrary multiplicity and spin was achieved by Stapp in 1962 with his postulate.
that it is the \( M \) functions, in their dependence on momentum components, that have only the Landau singularities. This is a non-trivial point. With more than five lines in a connected part the invariants formed from the momentum vectors are non-linearly related, so the formulation of analyticity properties through invariants becomes ambiguous. So far as spin is concerned, \( S \)-matrix elements have Lorentz transformation properties that depend on the momentum variables in a non-analytic fashion. Analyticity properties thus seem to be frame-dependent. Stapp emphasized, however, that the \( M \) functions transform in a manner independent of the particle momenta and correspondingly are suitable candidates for a maximal analyticity property. During recent months the importance for practical calculations of the \( M \)-function postulate has begun to be widely appreciated.

As stated previously, the validity of the properties included here under the category of first-degree analyticity is at present not an active source of controversy. The logical interrelationship of these properties remains unclear, but in the absence of zero-mass particles there are no signs of inconsistency and the cumulative experimental support is impressive. Zero-mass particles do present an essential complication, requiring a redefinition of the very concept of \( S \)-matrix and apparently leading to a totally different singularity structure. The usual approach to electromagnetism exploits the small magnitude of the fine-structure constant by defining an artificial hadronic \( S \)-matrix in the absence of electromagnetism and then tacking on the latter by field-theoretical perturbation techniques. Since it appears unlikely that the fine-structure constant and the zero photon mass should be determined by considerations of dynamical self-consistency, this divided approach to strong interactions and electrodynamics is natural when one is thinking in bootstrap terms of a nuclear democracy. The photon, that is to say, has an unmistakably aristocratic appearance.

The leptons are not quite so unique in appearance, but they are very different from hadrons and can hardly be expected to emerge from the same mould. Again, the usual approach is to tack weak-interaction effects by perturbation methods onto the purely strong-interaction \( S \)-matrix.

Second-degree analyticity

First-degree analyticity determines all singularities once the poles (i.e. the particles) are given, but a further principle seems required for determining the poles themselves. I say, 'seems', because it has never been shown that first-degree analyticity allows any poles to be arbitrarily assigned. An impression of arbitrariness for spin 0, \( \frac{1}{2} \), and 1 is created by
renormalizable Lagrangian field theory, which when evaluated by perturbation methods leads order by order to an S-matrix satisfying all the components of first-degree analyticity. The fields and coupling constants in the Lagrangian are, to a considerable extent, arbitrary and thus appear to allow arbitrary assignment of corresponding particles. The flaw in this reasoning is the substantial possibility that power series expansions of the S-matrix inevitably diverge; so it remains conceivable that first-degree analyticity is a sufficient constraint to determine a unique set of particles. Nevertheless, at the present stage of theoretical S-matrix development it is probably unprofitable to be concerned about possible redundancy of assumptions. The first order of business is to find the truth; the most beautiful manner of expressing the truth can wait.

As stated in my introduction, a promising additional S-matrix assumption is that all poles are Regge poles, a principle sometimes designated as 'second-degree analyticity' since it reflects an additional kind of continuability—in angular momentum. The original object of the second-degree assumption was to eliminate the apparent arbitrariness just described as characteristic of renormalizable Lagrangian perturbation field theories. Such field theories have so far always turned out to contain at least one low-spin pole not continuable in angular momentum; so they are excluded by second-degree analyticity. During the past six years, of course, experiments have more and more strongly suggested that established hadrons all lie on Regge trajectories.

Experiments furthermore have tended to confirm the conjecture made independently by Gribov and Frautschi and Chew that Regge poles control the high energy behaviour of scattering amplitudes at fixed momentum transfers. This aspect of second degree analyticity has an obvious impact on the question of arbitrary parameters, which in pre-Regge S-matrix theory seemed unavoidable as subtraction constants in dispersion relations. When asymptotic behaviour is interlocked with the poles, arbitrary subtractions become impossible.

Recently, in fact, Horn and Schmid, Logunov, Soloviev and Tavkhelidze, and Igi and Matsuda have extended a technique invented in 1962 by Igi to relate integrals over the low energy resonance region directly to the Regge parameters that control high energy. These Reggeized sum rules† have attractive possibilities for bootstrap investigations, in avoiding

* Regge cuts probably exist, as shown by Mandelstam, but the cuts are presumed to be determined by the poles.

† The so-called superconvergence relations may be regarded as special cases of the Reggeized sum rule, applicable when there happen to be no high-lying Regge trajectories or sufficient spin-flip to compensate for high trajectories.
the traditional truncation of unitarity sums that heretofore formed the basis of all dynamical calculations. The relevance to the bootstrap is illustrated in Figure 3, where a few possible high-lying trajectories are sketched; the associated low-energy resonances being indicated. A Reggeized sum rule relates the positive $E^2$ portion of trajectories belonging to a 'direct' reaction to the negative $E^2$ portion of trajectories belonging to 'crossed' reactions. Evidently the role of direct and crossed reactions may be reversed, the bootstrap problem being to find a set of trajectories that is mutually self-consistent.

**Monotonic Regge trajectories**

A further and more tentative $S$-matrix assumption, in this case chiefly motivated by experimental observations, is that the real parts of Regge trajectories as functions of energy squared rise indefinitely as $E^2 \to +\infty$ and fall indefinitely as $E^2 \to -\infty$. A theoretical motivation for this assumption lies in what Fermi used to call 'lack of sufficient reason.' The trajectory asymptotes in dynamical models can always be traced to the presence somewhere in the model of elementary particles. In a complete nuclear democracy it is hard to see where a trajectory would find a reason for going in the complex $J$-plane—except to infinity.

Mandelstam has probed this question with a perturbation theory model and shown that whereas a trajectory coupled only to 2-particle channels approaches $J = -1$ as $E^2 \to -\infty$, it approaches $J = -2$ when coupled to 3-particle channels, $J = -3$ for 4-particle channels, etc. For
positive $E^2$, model calculations show that trajectories tend to fall after
crossing the threshold of the dominant channel. But if higher and higher
thresholds tend to dominate as the energy increases, there is no reason
for a trajectory ever to reach a maximum. Such reasoning suggests that
ultimately it will be unnecessary to assume the monotonic property;
this may well turn out to be only self-consistent trajectory behaviour.
Current practice, nevertheless, treats monotonicity as an assumption.

It may be imagined that if elementary particles (e.g. quarks) exist, then
trajectories will turn over after reaching the elementary particle thresholds
and will approach negative-integer asymptotes corresponding to the
number of basic particles communicating with the trajectory. One can
only guess about such questions, but here is a potential future experi­
mental distinction between an $S$-matrix governed by the bootstrap and one
resting on elementary constituents that are difficult or impossible to observe
directly. We obviously need some qualitative evidence for or against
elementary particles that can be found in ordinary composite hadron
amplitudes. The monotonicity of trajectories may be the kind of feature
required. (Notice that if quarks are elementary and are not Regge poles,
one could only observe the corresponding fixed asymptotic powers in
amplitudes directly involving quarks.)

A related issue is the often-conjectured equivalence of field theory to
$S$-matrix theory. The dynamical content of $S$-matrix approximations
based on unitarity in ‘direct’ but not in ‘crossed’ reactions (such as the
$N/D$ method), with a finite limit on the particle multiplicity included in the
unitarity sum, has so far been expressable through off-shell equations
(such as the Bethe-Salpeter equation) in which certain selected particles
are given a favoured status. This is roughly equivalent to defining local
fields for these particles and seems to support the view that field theory and
$S$-matrix theory are equivalent. With no limit on channel multiplicity,
however, and an infinite number of particles to accommodate, it becomes
difficult to imagine the form of a Bethe-Salpeter-type equation. Which
particles, for example, do you select for special treatment? Perhaps the
monotonic-trajectory phenomenon is intrinsically unrepresentable through
off-shell equations. If so, it may have an impact on the continuing con­
troversy over the equivalence of field theory to $S$-matrix theory.

$S$-matrix models based on monotonic trajectories are beginning to be
studied, with Reggeized sum rules expected to provide new insight into
the dynamics. One hopes at the same time that the qualitatively en­
couraging bootstrap indications previously obtained from finite-channel
models (either $N/D$ or Bethe-Salpeter), particularly the rough dynamical
self-consistency of the observed low-$J$ multiplet structure, will not be lost.
A plausible mechanism to reconcile monotonic trajectories with finite-channel models would be for each physical pole (i.e. particle) to be dominated by the communicating channels of low kinetic energy and low orbital angular momentum. Moving up along a given trajectory, the dominant communicating channels would thus shift progressively toward those whose constituent particles have appropriately higher-mass and higher-spin. For example, the $\Delta(1240, \frac{3}{2}^+)$ may be dominated (as long believed) by the channel $\pi(140, 0^-)N(940, \frac{3}{2}^+)$, whose threshold is nearest and for which $l = 1$, while the first $\Delta$-trajectory recurrence $\Delta(1920, \frac{3}{2}^+)$ would get a bigger contribution from the channel $\pi(140, 0^-)N(1690, \frac{5}{2}^+)$, at $l = 1$ and with a nearby threshold, than from the channel $\pi(140, 0^-)N(940, \frac{1}{2}^+)$, at $l = 3$ and a distant threshold. To understand an entire trajectory, one needs to consider an infinite number of channels; but to understand approximately a small interval along a trajectory, a finite number of channels may suffice.

It is relevant in this connexion to recall that classical nuclear physics from the $S$-matrix viewpoint is simply the study of baryon number 2 and higher, not to be qualitatively distinguished from high-energy nuclear physics—which may be described as the study of baryon number 0 and 1. A fundamental and successful working principle of classical nuclear dynamics is to consider only those channels whose threshold is close to the particle mass (i.e. nuclear energy level) under consideration.* Needless to remark, the classical nuclear precedent makes it hard to understand, even if quarks exist, how distant-threshold quark-channels could be more important than nearby thresholds in the dynamics underlying low-mass particles.

The mystery of the small pion mass

A number of special correlations and predictions involving the pion have over the years been adduced from field theoretical methods. $S$-matrix theory has proved in some instances to be less productive than its rival in pionic exploitation, a count not in its favour, and it is important to know whether some essential ingredient is missing or whether the fault lies merely in our inadequately developed understanding of maximal analyticity. As one example, consider the prediction from the so-called ‘PCAC hypothesis’ that all pion amplitudes should become small near threshold. This prediction follows from the assumption of a smooth off-shell continuation of pion amplitudes that vanishes at zero pion four-momentum: Since the pion mass is small, one is relatively close to the point of zero

* In classical nuclear physics the neglected channels are all of higher threshold.
four-momentum whenever the kinetic energy is small. This idea, although imprecise, leads to useful predictions and suggests that, in staying on-shell, $S$-matrix theory may lack some significant element.

With second-degree analyticity, the pion in $S$-matrix theory evidently has no special status. There is an infinite number of Regge trajectories, bounded above by the Froissart limit, and one trajectory (not even the highest) happens to cross a physical $J$-value close to zero energy. (See Figure 4.) Considering the average spacing of trajectories one sees nothing remarkable about this fact. There must be a least massive hadron

and it happens to be the pion. Once we know which particle is lightest, is the $S$-matrix approach capable of yielding especially simple approximate predictions about this particle’s properties?

The answer in general is affirmative. Because the pion pole in certain amplitudes comes closer to the physical region than do any other singularities, it is often found that special and simple approximations can be based on pion-pole dominance. This is the phenomenon sometimes called ‘peripheralism’. The static model, furthermore, has long exploited the small ratio of pion to baryon mass to yield simple approximate coupling-constant ratios of the type later deduced from group theoretical models. Finally, it has been shown recently that, at zero total energy, Lorentz invariance imposes powerful special constraints on Regge trajectories and residues. It seems likely that such constraints, even though we are at present unsure how to extend them, will have an important effect at a point as close as $E^2 = m_p^2 = 0.02$ GeV$^2$. Superficial arguments suggest a suppressive effect, as in PCAC.
My personal (and perhaps overly optimistic) reading of this picture is that, once given a knowledge of the small pion mass, on-shell analyticity, unitarity, and Lorentz invariance will probably turn out sufficient to produce the successful approximate predictions of off-shell models. A more perplexing issue is the possibility of a direct relation between the small pion mass and the spin and parity, \(0^-\). Field theory suggests a connexion between an approximately conserved weak axial current and the small mass of a particle whose quantum numbers coincide with the divergence of the axial current. Bootstrap theory, in its exclusion of weak interactions, appears bereft of such a connexion. The low pion mass is supposed to emerge purely from dynamical self-consistency.

A possible resolution of this dilemma is hinted by in a suggestion of Zachariasen and Zweig\(^{20}\) that the structure of hadron weak interactions could be more complicated than just \(J = 1^{\pm}\) currents. They have shown that \(0^{\pm}\) and higher-spin currents might so far have escaped observation. One could add the conjecture that those special components of the weak hadronic current are enhanced for which corresponding low mass hadrons happen to exist.

A related conjecture is that the apparent locality of weak currents may be an approximation resting on the small mass of appropriate hadrons. It seems conceivable, in other words, that certain equal-time commutation relations are valid to the order of a few per cent because \(m_n^2/m_N^2 = 0.02\), while other local commutation relations are meaningless because none of the associated hadrons has an especially small mass. Again appealing to classical nuclear physics for a precedent, one may observe that the extremely useful notion of a local nucleon wave function can be derived as an approximation to \(S\)-matrix theory but would be untenable if \(m_n^2/m_N^2\) were not small.

If there is something truly fundamental about an approximately conserved weak axial current, then the small pion mass would not seem 'accidental' and the idea of nuclear democracy is in trouble. Note that the \(SU_3\) assignment of the pion to an octet of hadrons, in which all other members have more normal masses, tends to support the 'accidental' interpretation given by a bootstrap-sustained democracy. The unambiguous confirmation of Regge recurrences along the pion trajectory would constitute further support.

**The Pomeranchuk trajectory**

A second major mystery from the \(S\)-matrix standpoint is the possibility of a special status for the vacuum quantum numbers. Striking and distinctive experimental properties attach to the vacuum quantum
numbers (hereafter abbreviated as V.Q.N.) which appear to distinguish them from all others. These properties are intimately bound up with the controversial notion of the Pomeranchuk trajectory.

It was conjectured in 1961 by Gribov and independently by Frautschi and Chew that a Regge trajectory belonging to the V.Q.N. (even signature) may pass through the angular momentum value $J = 1$ at precisely zero energy. If other $J$-singularities are less important, one thereby achieves an immediate understanding of the following five features of high-energy hadron reactions, features that are suggested by experiment:

1. All hadron total cross sections approach constant nonzero limits at high energy.
2. All forward elastic amplitudes become pure imaginary in the high energy limit.
3. For a common target, particle and antiparticle total cross sections approach the same limit.
4. For a common target, the total cross sections for all members of an isotopic multiplet approach the same limit. (Analogously, to the extent that $SU_3$ is a 'good' symmetry, the total cross sections for all members of an $SU_3$ multiplet approach the same limit.)
5. Those special inelastic reactions where the exchanged quantum numbers are those of the vacuum will dominate at high energy, having the same dependence on energy as elastic scattering.

Properties (3) and (4) had been proposed earlier, before there was experimental evidence, by Pomeranchuk, using arguments which involved properties (1) and (2). The trajectory supposed to underlie the Pomeranchuk properties was therefore given his name. Property (5) is often called 'diffractive dissociation'.

Once the conjecture had been made of a V.Q.N. trajectory through $J = 1$ at zero energy, predictions became possible for high energy cross sections properties in addition to the above five. The factorizability rule for total cross sections is one of the simplest, but because of the limited variety of hadrons available or targets and beams there are as yet no good experimental tests. A second prediction is that if the slope of the Pomeranchuk trajectory is similar to that of other leading trajectories, there should be an indefinitely continuing shrinkage as the energy increases, in forward peak widths for all reactions, both elastic and inelastic. Unfortunately this shrinkage is slow and difficult to observe; for currently accessible energies, similar variations in the shape of the forward peak can be produced by trajectories lying below the Pomeranchuk. Thus we must await the construction of larger accelerators before the existence or non-existence of asymptotic peak shrinkage can be cleanly established for
reactions with V.Q.N. exchange. Even then the effect of branch points in angular momentum may obscure the picture.

The elusive experimental character of elastic peak shrinkage, the most characteristic physical consequence of the conjectured Pomeranchuk trajectory, has permitted widespread scepticism about the existence of this trajectory. A number of trajectories for other quantum numbers are by now regarded as reasonably well established, but severe doubt continues to exist about the V.Q.N. Two different but related sources of scepticism may be pinpointed. First, the dynamical mechanism that would cause the Pomeranchuk trajectory to pass exactly through $J = 1$ at $t = 0$ remains unexplained. Dynamical arguments based on crossing matrices have been given to suggest that a trajectory carrying the V.Q.N. should lie above all others, and the Froissart limit\(^6\) forbids any trajectory from lying above $J = 1$ at $t = 0$, but the necessity for an intercept at precisely $J = 1$ has never been shown. (Remember that no other hadron trajectories pass through integer $J$ at $t = 0$.) Recently it was pointed out\(^{24}\) that the currently available evidence on the energy dependence of total cross sections does not preclude a Pomeranchuk intercept slightly below 1 (the value $\alpha_\pi(0) \approx 0.93$ was proposed), corresponding to total cross sections asymptotically vanishing according to a small negative power of the energy, $\alpha_\pi(0) - 1$. Should such decreasing behaviour be established, there would be no basis for belief that the V.Q.N. are qualitatively different from other quantum numbers. Most physicists, however, feel it would be ugly for total cross sections to almost, but not quite, approach constants at high energy.

Assuming non-zero limits for high energy total cross sections, a second aspect of the experimental facts seems unnatural from the Regge point of view: The order of magnitude of these hadron total cross-section limits corresponds roughly to the geometrical 'radius' of the particles as defined by the width of the diffraction peak; in other words the cross sections seem to be approaching the unitarity limit for orbital angular momenta below that corresponding to the 'radius' as impact parameter. This familiar statement has a simple semiclassical interpretation commonly expressed through optical models, but from the Regge-pole point of view the unitarity limit is not approached; instead the magnitude of a high energy total cross section is determined by a residue of the Pomeranchuk trajectory at $J = 1$. Now remember that at $J = 2, 4, 6$ the Pomeranchuk residues determine the partial widths of mesons with the V.Q.N. (including perhaps the $f(1250)$). Thus, to believe in the Pomeranchuk trajectory one must believe that the analytic extrapolation of a partial width sequence to $J = 1$ is controlled at the latter point by considerations of geometrical 'size'. This idea seems so weird that some physicists dismiss the notion.
What alternative to the Pomeranchuk trajectory is possible that will preserve the properties (1) to (5)? One conceivable alternative is a $J = 1$ V.Q.N. fixed pole which has no connexion to physical particles. It was pointed out in 1961 by Froissart\textsuperscript{25} that such a fixed pole is allowable if appropriately shielded for $t > 0$ by moving branch points in angular momentum. Gribov\textsuperscript{26} had shown earlier that without branch points there would be conflict with unitarity and recently it has been realized that the kind of branch points envisaged by Mandelstam\textsuperscript{27} would not suffice: A new type is required, with peculiar but not inconceivable properties.

An objection to a fixed $J$-pole is the observed qualitative similarity between elastic scattering and inelastic reactions with non-V.Q.N. exchange, such as $\pi^- p \rightarrow \pi^0 n$. The order of magnitude of the peak widths is similar, as are the implied pole residues, the larger cross section for the elastic reaction being attributable to the higher V.Q.N. trajectory intercept at $t = 0$. Such phenomenological similarity between elastic and inelastic (non-V.Q.N. exchange) reactions is strange if the underlying mechanism is totally different. It would not be strange if both were due to Regge (moving) poles.

A second objection to a fixed pole at $J = 1$, to be amplified below, is that, with factorization, it would lead to multiple production at high energy in excess of the Froissart limit. It appears, consequently, that if the Pomeranchuk trajectory does not exist we shall require an even more bizarre singularity to replace it.

Thus there continues to be available within $S$-matrix theory no more palatable an explanation of the special V.Q.N. properties than the bootstrap idea that the highest-lying Regge trajectory carries the V.Q.N. because the attractive forces here are strongest. One important circumstance, however, has never been exploited. This is the distinguished role in the unitarity condition of diagonal $S$-matrix elements—as opposed to non-diagonal elements. It is well known that under analytic continuation the unit matrix must be separated out and treated on a special basis, and it is precisely the crossed V.Q.N. that communicate with this unit matrix. Here then is an obvious source of exceptional properties for the V.Q.N. The difficulty is that no theorist has so far been clever enough to construct a model satisfying unitarity in both direct and crossed reactions. In consequence we have no understanding of how the poles in a given reaction are affected by unitarity in crossed reactions. One important aspect of this deficiency is our ignorance of the mechanism by which a pole is forbidden from occurring at negative values of energy squared. $N/D$ or Bethe-Salpeter equations fail to preclude such a pole, even though it would violate crossed-unitarity. A second aspect is the Froissart limit forbidding negative-$E^2$ Regge poles above $J = 1$. This limit is absent from
equations that enforce unitarity only in pole-communicating reactions. Crossed unitarity again is essential.

Considering the close relation of such questions to the Pomeranchuk trajectory, it is not surprising that the latter should seem so puzzling. The Pomeranchuk mystery, as opposed to that of the pion, carries no implication of a deficiency in recognized S-matrix principles. The defect lies in our ability to interrelate these principles.

The multi-Regge-pole hypothesis

It was realized in 1963 by Kibble28 and Ter-Martirosyan29 that the assumption of Regge asymptotics in one variable of a four-line connected part suggests an extension to several variables in connected parts with more than four lines. In 1965 Toller30 made a group theoretical analysis of kinematics which can be used to confirm that single-variable Regge asymptotic behaviour, together with factorization, does in fact imply a unique extension to arbitrarily many variables. The extension leads to specific and important predictions about multiparticle production processes, some of which are qualitatively supported by experimental evidence but most of which remain untested. An already mentioned theoretical aspect of the multi-Regge hypothesis is its incompatibility with a fixed \( J = 1 \) V.Q.N. pole of factorizable residue. Straightforward calculation shows that such a pole would lead to an \( N \)-particle production cross section proportional to \( (\log \text{energy})^N \), in violation of the Froissart limit31.

A more positive consequence of multi-Reggeism is the unambiguous definition it implies for connected parts with any number of Reggeized external lines. The Toller analysis, which generalizes a proposal by Joos32, allows any amplitude to be expanded according to the little groups associated with a set of spacelike momentum transfers, as in Figure 5, which represents a possible momentum-transfer decomposition of a reaction amplitude for four incoming and four outgoing particles. Associated with each momentum transfer \( Q_i \) of magnitude \( Q_i^2 = t_i \) there is a non-compact sub-group of Lorentz transformations which leaves \( Q_i \) unchanged. The amplitude may be expanded in the unitary irreducible representations of this group \([SU(1, 1)]\), which require a continuous label \( \langle \sigma \rangle \). The projection onto a particular representation may be called a ‘partial-wave amplitude’, which in our example would be designated as \( A(\sigma_1, \sigma_2, \sigma_3, \sigma_4) \). If Regge poles exist, corresponding poles would appear in each of the \( \sigma \) variables, the position of a pole in \( \sigma_i \) as a function of \( t_i \) constituting the usual trajectory. These poles control the asymptotic behaviour for large elements of the little groups in a manner analogous
to that for single Regge poles. By factorization of the pole residues one may then define a connected part each of whose lines is continued in $\sigma$, with a simultaneous corresponding continuation in mass. (See Figure 6.) Although no rigorous connexion has been made between the Joos-Toller partial wave amplitude, defined for spacelike $Q$, and the Froissart-Gribov amplitude, defined under more restrictive circumstances for timelike $Q$, Joos, Toller and Boyce have made plausible an analytic relation between the two, with $\sigma$ recognizable as a complex angular momentum. Thus the $a_i(t_i)$ in Figure 6 may be described as 'complex spins'. Note that we are not going off-shell in these considerations. The mass of each particle follows the spin in such a way that whenever the latter becomes a physical integer of half-integer we are talking about a physical particle.
A conceivable future avenue of dynamical development opens up with the Reggeization of external spins. Heretofore, as I have stressed repeatedly in this report, dynamical models have been limited to a finite number of participating channels. If, however, one somehow could formulate a model in terms of trajectories (rather than individual particles) an infinite number of channels would be included even when the number of trajectories is finite. The approximation scheme presumably would be based on the ranking of trajectories, starting at the top and assuming that the lower the rank the less is the dynamical importance. Such an approach would be very different from the traditional one of keeping only low-mass, low-spin particles in dynamical equations.

**Regge daughters and conspiracy**

A fascinating recent series of developments has centred on special properties of the S-matrix arising when a momentum transfer $Q_t$ vanishes. The associated little group then enlarges to the full Lorentz group, with a corresponding increase of symmetry. If Regge poles exist, remarkable correlations between different poles are required in order to satisfy the increased symmetry. One special aspect of this situation was noticed in 1963 by Gribov and Volkov, and other aspects were found by Domokos and Suranyi in a 1964 Bethe-Salpeter model. The problem has been analysed systematically by Toller on a purely group-theoretical basis.

Toller points out that the unitary irreducible representations of the Lorentz group are labelled by a continuous index $\lambda$ and a discrete index $M$. The natural assumption then is that in a $Q_t = 0$ partial wave amplitude $A(\lambda, M)$, poles occur in $\lambda$, each pole carrying a definite value of $M$. These $\lambda$ poles (called 'Lorentz Poles' by Toller) control $Q_t = 0$ asymptotic behaviour for large group elements in much the same way as do Regge poles for $Q_t \neq 0$. When the larger group $SU(2, 1)$ representations are decomposed in terms of the smaller $SU(1, 1)$ it is found that a single Lorentz pole at $\lambda = \alpha$ corresponds to an infinite series of Regge poles at $j = \alpha - 1, \alpha - 2, \ldots$, with correlated residues and alternating parity and signature. This is the so-called daughter sequence illustrated in Figure 7a. Furthermore, if the Lorentz pole has $M \neq 0$ then Regge poles of both parities are required, again with correlated residues, as shown in Figure 7b. This phenomenon has been called 'conspiracy'.

Parity doubling of fermion trajectories at zero energy was pointed out in 1962 by Gribov on the basis of the MacDowell symmetry, but doubling for mesons had not been anticipated. It turns out that the top-ranking meson trajectories have $M = 0$ and consequently are not doubled. There is much discussion and analysis in progress over whether any second-rank
Fig. 7a

Fig. 7b
trajectories, such as \( \pi, B, A_1, \) etc., may have \( M = 1 \); the picture at present remains cloudy.

It is remarkable and significant that certain aspects of the above story were independently deduced by arguments that employed analyticity as the primary principle, going not to the point \( Q_i = 0 \) where the larger symmetry holds but only to the surface \( Q_i^2 = 0 \). I have already mentioned the phenomenon of parity doubling for fermion trajectories. A second example was the deduction of the daughter sequence by Freedman and Wang\(^{39} \) from the requirement of compatibility between the Regge expansion and first-degree analyticity in an amplitude for unequal mass particles. The interrelation between analyticity and Lorentz invariance evident in these examples illustrates the difficulty of formulating a non-redundant set of S-matrix axioms.

The deduction from general principles of the daughter sequence reinforces the notion that the number of S-matrix poles must be infinite. The new element is that now we anticipate increasing-mass sequences within which all other quantum numbers remain the same.

**Conclusion**

Let me close this report by emphasizing once again that the S-matrix theory of hadrons may require no further essential physical ingredients. Properties already identified, with solid experimental backing, are more than theorists presently can handle. The problem seems not to be the discovery of additional basic principles but rather the understanding of the mechanism by which recognized properties manage to be mutually compatible. It is here that experiments will continue to play a crucial role. After all, nature has somehow managed to solve the fiercely nonlinear and circular conditions implied by unitarity in different reactions connected by analytic continuation. By looking at enough different aspects of her solution we should be able to figure out how she did it.

A good example of experiment's role in S-matrix theory is the power behaviour at high energy with fixed momentum transfer. Observation of this behaviour has been a powerful stimulant and its implications are far from exhausted. Recently experiments have begun to suggest some kind of exponential behaviour in energy when the angle is fixed. Were such behaviour established as a general phenomenon it would have a major impact. A similar statement evidently applies to the monotonic character of trajectories. One hopes that S-matrix asymptotics ultimately will be deduced from simpler considerations, but experiments may have to lead theorists by the nose to the appropriate mathematical techniques.

Frequently I have alluded to the difficulties of formulating a clean set
of $S$-matrix axioms. This problem seems more and more to be interlocked with the extraction from the axioms of their physical content. A complete statement of first-degree analyticity, for example, may require a definite procedure by which the singularities, including the poles, are systematically located. When and if such a procedure is established it is conceivable that second-degree analyticity will turn out to be redundant, the only poles consistent with unitarity and first-degree analyticity being Regge poles. A related paradox is that traditional axiomatic approaches start with stable particles as given, while in a bootstrap regime such a procedure does not seem natural.

Although one must anticipate periods of theoretical frustration over these enormously difficult questions, we may be confident that the theory will not stagnate so long as experiments are continued. Unless some of the apparently established principles are overthrown, there is present in analytic $S$-matrix theory a superabundance of still unexploited physical content that cannot be ignored.

References

(36) Toller, M., Internal Reports No. 76 and 84, Instituto di Fisica ‘G. Marconi’ Roma (April 1965); CERN preprint TH 780/April 1967.
Discussion on the report of G. F. Chew

M. Gell-Mann. You have implied that someone who wants to formulate theoretical principles governing the electromagnetic and weak interactions of hadrons and relate them to those of leptons (which seem so similar) would be foolish to introduce local operators: can you give a constructive suggestion as to what we should do instead?

G. F. Chew. I have no immediate suggestion for tackling the problem of currents or, equivalently, form factors, except to employ the standard dispersion relations with an open mind about asymptotic behaviour, which may be related to the question of locality. It seems to me likely that the asymptotic behaviour of the form factor is connected to aspects of S-matrix asymptotic behaviour which we still do not understand, such as the possibility of exponential decrease in certain directions.

J. Hamilton. I wish to ask a practical question, in order to understand the first part of your talk. How am I to calculate the short range part of the interaction in something like the p-meson bootstrap, bearing in mind that there is an ellipse of convergence which limits the extent to which crossing can be used to calculate the discontinuity across the unphysical cut of the partial wave amplitude. Can you tell me how to calculate this far away discontinuity, or do you say that the short range part of the interaction is unimportant?

G. F. Chew. I presume that your question about short-range forces is to be understood within the finite-channel $N/D$ framework. As indicated in my report, I feel that this approximation has basic defects that preclude it ever giving a satisfactory answer to the ‘short-range’ force problem.

The new approach based directly on Regge trajectories recasts the dynamics in such a fundamental fashion that one has not yet identified therein a distinction between force components based on ‘range’. This may be a hopeful sign that the old ‘short range’ dilemma can be circumvented.

J. Hamilton. If that is to be the solution, then it should be emphasized that the method you propose would appear to be considerably removed from the methods used in those applications of dispersion relations which have been successful up to the present.

F. E. Low. How does one define macroscopic causality? i.e. how would one recognize an acausal event that did not violate energy and momentum conservation in some co-ordinate system?

E. P. Wigner. I am afraid I do not have anything very useful to contribute: I do not know of any rigorous definition of causality. As Dr. Low already
implied a set of events cannot be said to violate causality ipso facto though it may not be possible to specify which of the events are the causes of the others.

In practice, the situation is different because one uses also some plausibility assumptions. Thus, if two particles collide and then separate, the distance of their separation after a given time cannot exceed a certain value. In order to arrive at a given distance, they must traverse, during the time available, the distance of original separation, and also the distance at which they are finally separated, minus twice some reasonable range of interaction. Expressed in terms of the $S$ matrix, this means that the energy derivative of the phase of the characteristic value must be larger than some negative number, proportional to the 'reasonable range' mentioned before.

However, as the reference to the 'reasonable range of interaction' shows, this is not a sharply defined condition.

**B. Ferretti.** One way of looking to macroscopic causality is to try to define an asymptotic velocity of signals and to show that this asymptotic velocity has an upper limit which does not exceed the velocity of light no matter which is the state of the considered system. This is impossible, unless microcausality is valid, if one considers signals between two space-time points. It should be noted that signals between two space-time points are completely devoided of any physical meaning. However the definition of the asymptotical velocity is possible even if microcausality is not valid, if one considers that the source and the detector of the signals are localised in a finite space-time region of suitable shape, such that a 'frame of reference of the detector' can be defined (see B. Ferretti, *N. Cim.*, 43, 507, 1966, Figure 2). In this case both a space distance and a time interval with its sign between source and detector can be defined in an invariant way, and so does consequently a speed.

It can then be investigated whether the requirement of the asymptotic upper limit of this speed with increasing distance can be satisfied.

A number of examples in which this requirement is satisfied, in spite of the fact that in these examples the macroscopic causality is not valid, can be constructed (see B. Ferretti, *N. Cim.* 43, 507, 1966 and *N. Cim.* 43, 516, 1966).

**R. E. Marshak.** You state that you hope to develop a complete theory of hadrons and strong interactions on the basis of the bootstrap principle. You do not attempt to understand the electromagnetic and weak interactions of the hadrons ab initio. Your bootstrap method must therefore explain the approximate symmetry groups (broken $SU_3$, asymptotic $SU_3 \otimes SU_3$ group as reflected in the Weinberg sum rules, etc.) that underlie
the hadrons and strong interactions and at the same time yield the $SU_3 \otimes SU_3$ algebra for the hadron currents as well as P.C.A.C. which works so well for the electromagnetic and weak interactions. Is there any chance that the bootstrap method can find this structure without inserting the answers (octets of particles, suitable parity doubling, etc.) at the beginning?

**G. F. Chew.** There is indeed a chance, in my opinion. I suggest that we wait until Mandelstam has described his recent work before having discussion on this point.

**W. Heisenberg.** May I make a remark concerning the question of the difference between the bootstrap mechanism and a theory starting from a master-field equation. To my mind, the latter adds only two statements to those of the bootstrap mechanism. It states the group structure of the underlying natural law and it defines the kind of analyticity which should be meant by the postulate of causality. Both statements seem necessary, since I cannot imagine that the bootstrap mechanism really defines the group structure completely (e.g. exact $SU_2$ or only approximate $SU_2$), and I wonder whether such terms as maximum analyticity can be defined better than by a differential equation. Would your definition of analyticity differ from that coming from a differential equation?

**G. F. Chew.** In all Lagrangian field theories with which I am acquainted, the special angular-momentum values selected by the master equation become reflected through a non-democratic particle spectrum. In other words, the theories contain first-degree but not second-degree analyticity.

**H. P. Dür.** Chew has mentioned a point which, I think, is important. He indicated that one of the difficulties in writing down a master equation for elementary particles seems to be that it destroys the democracy of elementary particles because it has to be written down in terms of certain fundamental fields which have certain transformation properties and hence will always single out some particles which have the same properties to be more fundamental.

Hence, it appears that writing down a master equation introduces usually more than what Heisenberg has just mentioned, namely certain symmetry group properties and a shorthand description of what we mean by correct analyticity in the $S$-matrix language arising from the causality requirement, but, in addition, it introduces certain elementary particles, as distinct from composite particles. To establish true democracy for all particles we hence have to learn how to formulate a master equation without introducing an elementary particle. A general prescription to do this is not known. However, it is our impression that by formulating a local field theory in terms of field operators which do not obey the canonical commutation rules we can prevent this field from associating itself
directly with an elementary particle and hence avoid this difficulty. At
the same time the divergence difficulties can be removed. An alternate
method, which probably would achieve the same, would be to introduce
canonical fields but with some kind of non-local interactions. From this
point of view, it indeed is suggestive to consider the divergence difficulties
as being closely connected with the introduction of elementary particles
into the formulation. Perhaps somebody knows a better way how to write
down field equations without establishing at the same time an elementary
particle.
R. Haag. Just a question for information. Is there anything wrong with
a fixed pole at angular momentum less or equal to 1?
G. F. Chew. There is no compelling reason to exclude fixed singularities
at low $J$-values (e.g. at $J = \frac{1}{2}$). By definition, however, such singularities
would violate the idea of nuclear democracy.
S. Mandelstam. I should like firstly to make some remarks on a dynamical
scheme based on rising Regge trajectories. As usual we have the four
ingredients
(i) analyticity
(ii) unitarity
(iii) crossing
(iv) bootstrap condition.
As Chew remarked in this talk, the fourth condition is introduced as
the requirement that there are no Kronecker-delta singularities in the
$J$-plane rather than as a requirement involving Levinson’s theorem.
This last requirement, together with the indefinite rise of Regge trajecto-
ries, can easily be imposed, if we work with equations for the Regge
trajectories themselves. It is known that one can treat potential theory
by using such an approach. The approximation made is that the scattering
amplitude is dominated by a finite number of Regge trajectories—in the
lowest approximation by one trajectory. The Regge parameters $\alpha$ and $\beta$
satisfy dispersion relations (this statement is not always true but the
complications can be dealt with). Unitarity gives us non-linear equations
for the weight functions. The equations were first proposed, I believe, by
Zachariasen, and were developed more fully by Cheng and Sharp and
others. The dispersion relations for $\alpha$ and $\beta$ require subtractions; the value
of the subtraction terms is determined from knowledge of the potential.
The results in the one-trajectory approximation are reasonably accurate
for a wide range of Yukawa potentials, especially if modifications of the
Regge representation are used.
In the elementary-particle problem one would use two subtractions
instead of one in the dispersion integral for $\alpha$, in order to ensure that the
trajectories rise indefinitely. Now the trajectories appear experimentally to be fairly straight lines, which suggests an approximation where one keeps only the two subtraction terms and neglects the dispersion integral entirely. Such an approximation implies infinitely narrow resonances. It simplifies the calculations enormously, non-linear integral equations becoming numerical equations. Furthermore, numerous correlations between resonance parameters have been obtained from current commutation relations or super-convergence relations, and a scheme based on a narrow resonance approximation may well include such correlations. This point of view has been stressed consistently by Gell-Mann. It is one of the advantages of the present scheme that it can be treated in the narrow-resonance approximation. Nevertheless, we can go beyond the narrow-resonance approximation if necessary; we would then have to use equations of the Cheng-Sharp type.

We now come to the question of the subtraction terms in the dispersion integrals. Indeed, in the narrow-resonance approximation, the whole contribution consists of subtraction terms. In the potential model these subtraction terms were introduced from knowledge of the potential; in the elementary-particle problem they must be determined from the crossing relation. There is no unique way of applying the crossing relations, but one attractive possibility is to use the Reggeized sum rules which Chew discussed. He explained how they contain the crossing relation within them.

One has to make several types of approximations. I have already mentioned the narrow-resonance approximation. Another approximation is in the treatment of the Reggeised sum rule, which is only exact if the upper limit of integration is infinite. In practice we have to cut it off above a finite number of resonances and, in the lowest approximation, we cut it off above a single resonance. One does not then expect accurate results, but it is worthwhile to investigate whether the scheme can be implemented to give a consistent, reasonable solution which may serve as a basis for a more adequate treatment.

The particular problem I chose was to obtain the pseudoscalar, vector and axial-vector mesons as bound states of a baryon antibaryon pair. We therefore look at the baryon antibaryon channel, assume that the amplitude is dominated by the three trajectories corresponding to these particles, and cut off the Reggeized sum rules above the lowest resonance. Since we are only investigating one channel we cannot expect to obtain all quantities; the same quantities appear as parameters in different channels. For example, a meson–baryon coupling constant appears in the contribution of a meson resonance to a baryon antibaryon channel, and also in the
contribution of a baryon resonance to a meson–baryon channel. We therefore have to take certain quantities from experiment if we look at the baryon antibaryon channel alone. I defined the unit of \( (\text{mass})^2 \) as the inverse of the slope of the trajectory, and took the nucleon to have unit mass on this scale. I also assumed that the vector trajectory was one unit above the pseudo-scalar trajectory and half a unit above the axial-vector trajectory. The equations could then be solved to yield reasonable meson masses \( (\mu^2 = 0.3 \) for the vector and pseudo-scalar mesons). It also turned out that the ratio of the squares of the coupling constants was positive, a feature that was not automatic from the structure of the equations.

One quantity which could not be calculated from the equations was the magnitude of the coupling constants, as the equations were linear and homogeneous in these variables. To obtain the magnitude of the coupling constant one will have to go beyond the narrow-resonance approximation.

Another bootstrap calculation based on Reggeized sum rules has recently been done by Schmid. He attempted to obtain the \( \rho \) as a bound state of the \( \pi \pi \) channel. By using the Reggeized sum rules at \( t = m_{\rho}^2 \), he was able to avoid making assumptions about the nature of the trajectories. He obtained results in reasonable agreement with experiment.

I should like to make a remark on the subject of the small mass of the pion. It has been shown by Gilman and Harari, following a work by Low, that certain superconvergence relations involving pion scattering are only consistent with saturation by lowest resonances if the pion has zero mass. The saturation by the lowest resonance can clearly not be justified theoretically at present, but it is often assumed and does seem to lead to reasonable results in some cases. If we do assume it we may thus be able to understand the small mass of the pion. In better approximations, where the superconvergence relations are not fully saturated by one resonance, the mass of the pion will not be exactly zero.

I should also like to say something about P.C.A.C. We know that P.C.A.C. imposes certain limitations on the strong interactions. We shall consider the limit where the pion mass is zero; approximate results then become exact. One of the restrictions on the strong interactions is that threshold pion amplitudes vanishes in certain cases (Adler self-consistency condition). Another is that the anti-symmetric part of the amplitude for the scattering of pions off any target is equal to a universal constant multiplied by the isotopic spin of the target. I would like to indicate how these results can be obtained by considering the on-shell strong amplitudes alone, without introducing currents. At the moment I do not have all arguments sufficiently tight, but I believe they are correct.

The assumption that we make is that the conspiracy quantum number \( M \)
of the pion trajectory at $t = 0$ is equal to 1. This assumption follows from the following two facts, both of which appear to be true experimentally:

(i) The interaction of the pion trajectory with nucleons does not vanish at $t = 0$.

(ii) There is no axial vector particle with mass approximately equal to that of the pion.

It is a feature of conspiracy theory that the ratio of the sense to the nonsense amplitudes is determined by the kinematics. In this respect the situation at $t = 0$, where conspiracy theory applies, is different from that at $t \neq 0$, where it does not. Furthermore, with $M = 1$ (or, in general, $M \neq 0$) and with a trajectory passing through $t = 0$ at $J = 0$, which we are assuming, the ratio of the sense to the nonsense amplitudes is zero. The nonsense amplitudes cannot be infinite without violating the required analytic properties in $s$-$t$ space, so that the sense amplitudes must be zero.

When we examine carefully the implications of the last statement, we realize that they are precisely the Adler self-consistency conditions. If we consider a process $\pi + A \rightarrow B$,

where $A$ and $B$ are in general multi-particle states, we remind the audience that conspiracy theory applies only when all four components of the pion momentum are zero (which implies $m_A = m_B$). Thus, under these conditions, the amplitude vanishes. This is the Adler self-consistency condition.

To proceed further, let us consider an amplitude $\pi_1 + A \rightarrow \pi_2 + B$.

From what we have just said it follows that the amplitude must vanish if either pion has all four momentum components equal to zero. Let us ask the question whether we can find an amplitude which is linear in the pion momenta and which satisfies this condition. It turns out that we can find such an amplitude. It has the form

$$A_\mu (p_1 - p_2)_\mu$$
where \( p_1 \) and \( p_2 \) are respectively the momentum of \( \pi_1 \), and minus the momentum of \( \pi_2 \). \( A \) is restricted by the condition

\[
A_\mu (p_1 + p_2)_\mu = 0
\]

This condition looks very much like the so-called gauge condition in electrodynamics and has similar consequences. For instance, by following Zwanziger's and Weinberg's proof that the interaction constant of a photon with a particle is equal to a universal constant times a conserved quantity, we obtain a similar result here. The only conserved quantity is the isotopic spin, and we thus conclude that the anti-symmetric part of the amplitude for the scattering of pions against a target is proportional to the isotopic spin of the target. This is what we wanted to prove.

Now that we have obtained these on-shell results, let us consider the definition of a current. In the approximation where the pion mass is zero, a partially conserved axial current becomes an exactly conserved axial current. We shall assume that dispersion relations for currents have solutions, though we cannot prove anything in this direction at the moment. If we attempt to construct a conserved axial current, we find poles at \( q^2 = 0 \), where \( q \) is the momentum vector corresponding to the current. It is thus inconsistent to assume the existence of a conserved axial current unless there are pseudo-scalar particles with \( m = 0 \). This is a dispersion-theoretic way of approaching Goldstone's theorem. The coupling of the zero-mass particles must satisfy the Adler self-consistency condition. If we know that we have such particles in the theory, we have no difficulty in constructing a conserved axial current.

Adler and Weinberg have related the commutator between two axial currents to the anti-symmetric part of the amplitude for scattering of pions against a target. We can now reverse their argument and, from our result about this anti-symmetric part, we can show that the commutator between two total axial charges is proportional to the vector charge. If we define the current so that the constant of proportionality be equal to one, and assume weak-interaction universality in the form that the weak Lagrangian involves a current so defined, we obtain the Adler-Weinberger relation in the usual way.

**S. Weinberg.** Just two short questions to Mandelstam or Chew. Can you calculate the coefficient of proportionality in the relation between pion scattering lengths and the isotopic spin of the target particle? and could you put a 'Reggeized sum rule' on the blackboard?

**S. Mandelstam.** To reply to the first question: if we assume weak interaction universality in the form stated, we can calculate the scattering
lengths in terms of \( g^2 \) and \( (g_n/g_A)^2 \) and obtain the usual Adler–Weisberger formula.

To reply to the second question: if an amplitude has an asymptotic behaviour
\[ A(s, t) \sim \sum B_i(t) \left( \frac{-s}{s+1} \right) \] for \( s \to \infty \), then the sum rule takes the form
\[ \int_{-s}^{\infty} ds \text{Im} A(s, t) \sim \sum B_i(t) \left( \frac{s \alpha_i(t) + 1}{\alpha_i(t)} \right) \]
where the formula is asymptotically true as \( N \to \infty \).

R. Omnès. Concerning the problem of the asymptotic behaviour at finite angles mentioned by G. Chew, I want to mention that, at least in the case of \( \pi^0 \pi^0 \) scattering where crossing is a simple symmetry of the amplitude, falling Regge trajectory contribute to this behaviour an exponentially decreasing expression \( e^{-\beta s} \).

E. C. G. Sudarshan. I wish to comment on the bootstrap method of inducing symmetries: in the method pioneered by Cutkosky and followed up by several people it is necessary to put in the correct multiplicity of particles. In this sense a trace of the symmetry is already inserted at the beginning of the bootstrap.

With respect to the Toller expansion in terms of the \( 0(2, 1) \) partial waves, I have the following question: since it is known that not all functions can be expanded in terms of these ‘partial waves’ is one making an assumption? Or has he (or you) proved it?

G. F. Chew. A sufficient condition for the expansion is square integrability, which in Regge terms means that all poles should lie to the left of \( J = -\frac{1}{2} \). We believe that such a condition should always obtain for sufficiently large negative values of momentum transfer squared. By analytic continuation from such a region the general partial-wave amplitude may be defined.

A. Tavkhelidze. Question: Were Reggeized sum rules used for bootstrap investigation?

Remark: The reggeized sum rule is a consequence of analyticity and Regge behaviour at infinity. This sum rule connects the low-energy part of the scattering amplitude with high energy behaviour and has been shown to be in a good agreement with experiments.

The requirement that the low energy part be described by resonances alone is an extra condition.

G. F. Chew. This condition seems reasonably well satisfied. Once this additional approximation is invoked, the type of theoretical bootstrap calculation described by Mandelstam becomes possible.